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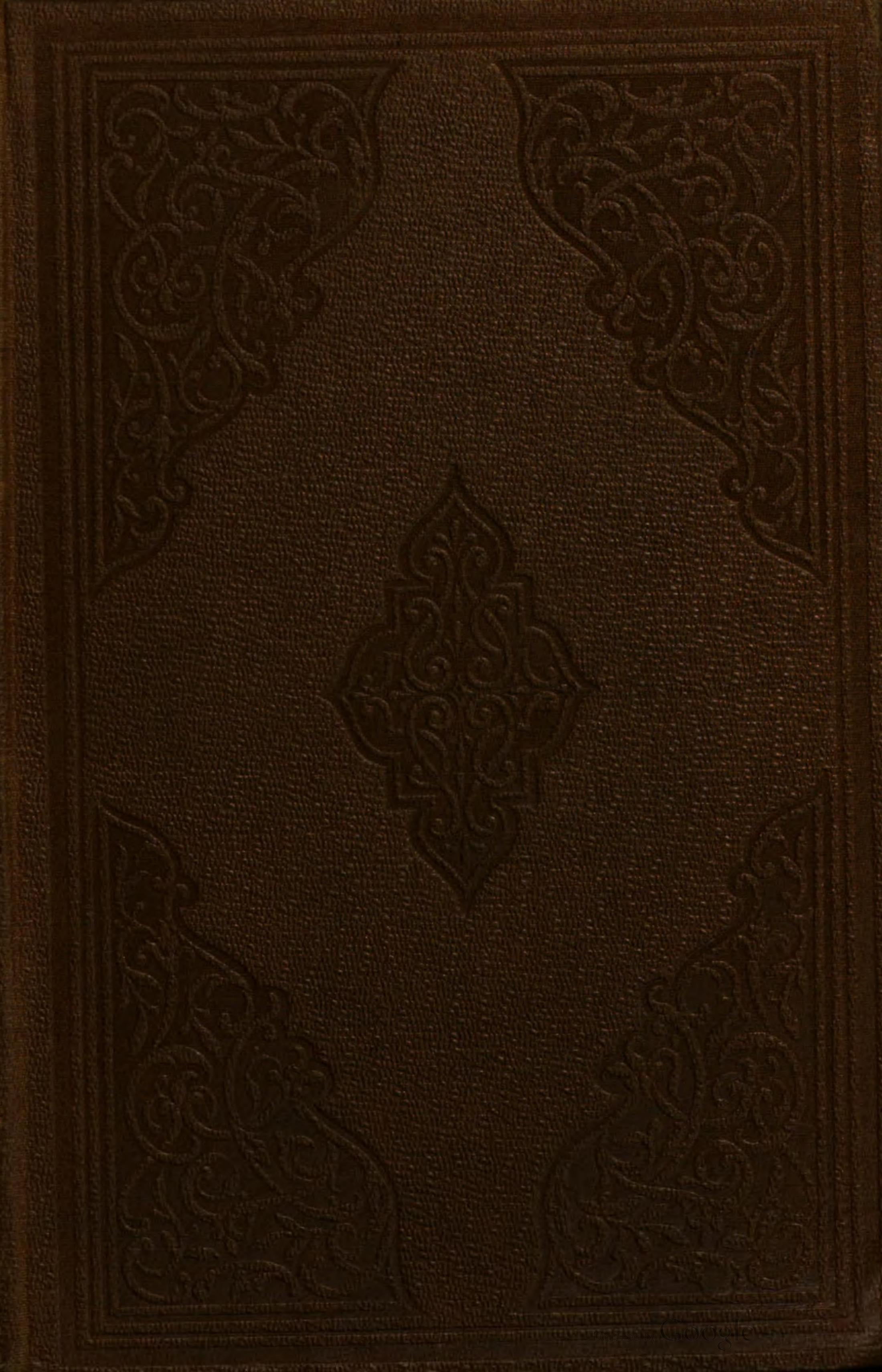

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NOTICES
OF THE
PROCEEDINGS
AT THE
MEETINGS OF THE MEMBERS
OF THE
ROYAL INSTITUTION OF GREAT BRITAIN
WITH
ABSTRACTS OF THE DISCOURSES
DELIVERED AT
THE EVENING MEETINGS

VOLUME XXI
1914—1916

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ROYAL INSTITUTION OF GREAT BRITAIN.

WEEKLY EVENING MEETING,

Friday, January 30, 1914.

HENRY EDWARD ARMSTRONG, ESQ., Ph.D. LL.D. F.R.S.,
Vice-President, in the Chair.

H. WICKHAM STEED, ESQ.,
Author of "The Hapsburg Monarchy."

The Foundations of Diplomacy.

THE title and the genesis of this discourse need some explanation. In the autumn of 1912, soon after the outbreak of the war between the Balkan Allies and Turkey, I received in Vienna a letter from London that started the train of thought which I propose to-night to lay before you. The Servians had decisively defeated the Turks and were advancing through Northern Albania towards the Adriatic. The Austro-Hungarian intimation that their advance should cease at Prizrend had been ignored. The question arose whether Austria-Hungary would use force to prevent the Servians from reaching Durazzo or any other point on the Adriatic, and, if so, what the consequences would be. Russia was strengthening her frontier garrisons. Austria-Hungary was secretly mobilizing. Had the Cabinet of Vienna listened to the warlike spirits who counselled active intervention against Servia, an Austro-Russian war would have been inevitable. The dreaded European conflagration might then have been a question of days or weeks. At this juncture an Englishman occupying an influential position and closely in touch with currents of political feeling, wrote to me: "I dread European complications over a subordinate Adriatic question. How are people in England, and still more in the oversea Dominions, to be made to understand that we can possibly have any interest in the destiny of this or that Albanian village of whose very existence most of us were, until yesterday, unaware? We can only fight for clearly defined British Imperial interests."

I will only indicate the tenor of my reply. It was to the effect that an Austro-Russian conflict would entail a Russo-German and a Franco-German war, which latter might result, within ten days, in the seizure of the Mouth of the Scheldt and even of Calais by a German army, with a view to their conversion into anti-British naval bases. Would these developments, I asked, affect no clearly defined British Imperial interest?

It would be too much to say that every child on the Continent instinctively perceived the relationship between the comparatively obscure question then suddenly thrust into prominence and the larger issues of European diplomacy; but it is certain that every intelligent newspaper reader in Austria-Hungary, Germany, France, Italy and Russia was alive to the possibly vital importance of the diplomatic struggle that was going on in connexion with apparently minor developments of the Balkan War. Why is it, I then asked, and have since repeatedly enquired, that British comprehension of foreign affairs should lag so far behind that of other countries? Why should the British Government be deprived of the strength that comes from the support of an awakened and well-informed public opinion? Why should our diplomatic action be hampered by the inability of our people to understand the bearings of issues that may drag us, willy-nilly, into a life-and-death struggle, whereas appreciation of the dangers involved might have enabled us to exercise our influence discerningly, and in time?

More by luck than by skill Europe then escaped the worst of the dangers that threatened her. By dint of changing their attitudes with bewildering rapidity, the Great Powers managed to march, with imperfect alignment, not abreast, but in the rear of events. When they sought to regain control and leadership, the uninformed diplomacy of some of them sanctioned expedients which were, a few months later, again to plunge South-Eastern Europe into a welter of blood. Diplomacy has rarely suffered defeats more discreditable than those of 1912–1913. It failed to prevent, by timely action, the first Balkan War. It failed to prevent the resumption of that war. It sanctioned arrangements that made the second Balkan War inevitable; and its only title to honour was that it succeeded, by muddling along the line of least resistance, in staving off a European conflagration.

What is diplomacy, and what are its foundations? Diplomacy is foreign policy "in being." Its foundations are usually supposed to be a strong and efficient army; an ample and well-trained navy; a well-filled war-chest. These elements doubtless form, if not the foundations, at least the sanction (in the juridical sense of the term) of diplomacy. But the corner-stone of diplomacy, as I conceive it, is living knowledge in the service of an ideal, a set and clearly understood purpose—not merely the knowledge contained in geographical manuals, in handbooks of International Law, in historical treatises or in statistical annuals. The knowledge I refer to is that which places its possessors a day ahead rather than a day behind the times. If, *gouverner c'est prévoir*, diplomacy *c'est savoir d'avance*.

Within these precincts, set apart for the dissemination of scientific and "useful knowledge" and for learned discussion, it may seem strange that I should presume to address you on a subject so nearly akin to politics as diplomacy. Diplomacy is essentially political, and politics have not yet been reduced to the position of a science. Their nature is rather that of an art than of a science—an art sometimes more histrionic than plastic. But I do not ask your attention for any consideration of this art in itself, or of the methods of its application. I propose to deal rather with the bases of diplomatic art, the condition on which alone it can become effective. This is essentially a matter of "useful knowledge." To me it seems that the foundations of successful and effective diplomacy in a democratic State can only be found in national education.

Let me illustrate this point. The two supremely successful diplomatists of the second half of the nineteenth century, Cavour and Bismarck, were, it is true, men of rare knowledge and ability, men possessing, in virtue of their knowledge, an intuitive insight into, and the power to anticipate, events. Such men are rare at all times, but they would have been powerless had not certain conditions of national knowledge and national will rendered their success possible. The existence of these or analogous conditions is a *sine quâ non* of successful diplomacy.

What are these conditions? An instructed public opinion, and a public will clearly directed towards definite objects. How is public opinion to be instructed and the public will directed? By the universities, by the Press, or by public speakers? These agencies are effective provided they be in their turn informed and inspired by an exalted purpose. In some foreign countries, notably in Germany, and to some extent in France, the universities or cognate institutions provide special opportuities for the study of "political science" in the abstract. Professors of *Staatswissenschaft* discourse to students, some of whom are grown men, upon the history of States, the application of political maxims, and the development of such institutions as limited monarchies and constitutions out of the older forms of monarchical or feudal organization. Those who receive this instruction may possess, at the end of their studies, a general knowledge of the principles involved in statecraft, but they are not therefore necessarily fitted to be judges of the rightness or the wrongness of contemporary diplomatic action. For this something more is required.

But why, it may be asked, cannot these matters be left to the management of responsible ministers and of trained diplomatists? Have not they made it the business of their lives to become "experts"? The answer is that the changes which have taken place in the forms of public life, especially in Western Europe, have gradually shifted power and the ultimate responsibility from the Crown and the upper classes on to the masses of the community. Where the power is, there also should the knowledge be. Diplomacy, or the management of foreign affairs, was originally a prerogative of the Crown. Even now ambassadors and ministers are the direct personal representatives of their Sovereign, even though their political instructions be received from the ruler's principal Foreign Secretary of State for the time being.

This Secretary of State is, in theory, subject to the control of Parliament, which, in theory, represents the people. In practice, the control exercised by the people through Parliament is at best intermittent, and may be exercised at the wrong moment. The tacit agreement to treat foreign policy as a non-party matter has further weakened parliamentary control. In diplomatic circles on the Continent the only kind of popular control that is recognized ever to affect the management of British foreign policy is that sometimes exercised by what a horrified Continental ambassador once called "the public opinion of British spinsters." Far be it from me to deride such public opinion. The British spinster, even though her manner of action and expression be extravagant, and though her feelings may be moved by very one-sided or inadequate presentations of a case, is at least acting in the name of an ideal and is not guided by self-interest or by the hope of personal advantage. In her way, she has a conception—sometimes an impracticable conception—of foreign policy (one which the official bureaucratic or diplomatic mind is fond of regarding as "dangerous"), but a conception born of solicitude for what she feels to be the higher interests of her country. The contention I wish to place before you this evening is that if a nation is to play the part of a living, driving force in the world, it must have a conscious ideal. This ideal must be so plain and clear, so evidently connected with the higher interests of the community, that it shall commend itself to and command the instantaneous support of the majority of right-feeling citizens.

History amply proves the accuracy of this claim. Without returning to the era of struggle between England and Spain, a struggle waged originally for religious freedom and national existence, it is only necessary to refer to the contest with France culminating in the struggle against Napoleon, to show that whenever British foreign policy has been most effective, it has embodied the aspirations of the great majority of thinking citizens. The history of other countries teaches the same lesson. The Italian Risorgimento, the diplomatic miracles achieved by Cavour, would not have been possible had not educated Italian sentiment supported the action of the Foreign minister. It is true that the masses of the Italian people were at first comparatively indifferent to the ideal that inspired the middle and upper classes. The people were brought into the movement by the red shirt and the magnetic genius of Garibaldi; but when once they lent the weight of the mass to the policy already inaugurated by the more enlightened classes, they drove that policy on to its final accomplishment and forced the hands of timorous ministers. The story of German unification teaches the same truth. The seeds had been sown by poets and thinkers long before they sprouted sufficiently for Bismarck to tend and stimulate their growth. Bismarck deliberately worked in accordance with the half-unconscious and ineffectually inarticulate national ideal. He knew that the heart of the nation was with him, and, strengthened by that knowledge, he applied himself to the realization of the national ideal. His diplomacy, though inferior in texture and quality to the finer web woven by Cavour, was inspired throughout by a burning patriotic purpose nourished by knowledge of the obstacles to be overcome, and by the determination to overcome them. The really heroic epoch of the process of German unification was not that of the war against Austria in 1866, nor even that of the victories over France in 1870–71. It lay rather in the *Konfliktperiode* between 1863 and 1866, when Bismarck, greatly daring, broke through the restrictions which the pedantic and professorial spirit of the Liberals in the Prussian Diet sought to impose upon him, and prepared, in the light of his knowledge, the military and diplomatic wherewithal for the overthrow of Austria and France.

Equally significant with Bismarck's action in thus taking immense risks in order to prepare the realization of an ideal which he knew to lie deep in the hearts of his fellow-countrymen, however far it may have been from the thoughts of the men who were supposed to represent the people, was his conduct after 1870 in deliberately striving to promote national education in matters of foreign politics by means of agents best fitted for the work.

He himself spoke in Parliament and out of Parliament with a fulness and efficacy rarely surpassed by statesmen in other countries. But he knew that his words alone could not achieve the task in hand. Through the famous historian, Professor von Treitschke and others, he sought to reach, and reached, the minds of the younger generations, to inflame them with the ideal of German greatness, to teach them pride in the Fatherland, and to urge upon them the need for patriotic sacrifice, not only at supreme moments, but on every day and at every hour of their lives. I myself had the privilege in the early nineties of sitting at the feet of Treitschke and hearing his public lectures to the students of Berlin University. The fire and force of his eloquence, the intensity of his conviction, his magnetic power need to have been experienced in order to be fully understood. By my side sat a typical young German whose philosophic tendencies and wide though ill-digested reading had led him to adopt a conception of individual and national life tinged with doctrinairism and vague cosmopolitan aspiration, and who was at first inclined to resent the "narrowness" of Treitschke's intense national idealism. A few years later I met my young friend again and found him transformed. When I alluded to Treitschke's lectures, he said with glowing enthusiasm, "*Den Mann verehre ich; der hat mir mein Vaterland wiedergegeben*" ("That man I revere; he gave me back my Fatherland"). It was as though I had witnessed the passing of a German mind of the old type and the birth of the modern German spirit—the pouring of a new, almost religious, fervour into minds that had previously lacked inspiration and had floated on through life upon a broad, sluggish stream of sentimental inefficacy.

On returning to England after an absence of more than twenty-one years, spent chiefly among the highly-organized States of the Continent, I have been struck by nothing so much as by the ignorance, insularity and sleepy carelessness of the British nation. It is true that we are not, and never have been, a supremely intellectual people. We rarely apprehend changes in national or international conditions until the effects of those changes force themselves upon our notice by affecting our physical comforts or emptying our pockets. Then we indulge in the hysterical luxury of a "scare," put forward amateurish theories to explain what has happened, essay some empirical remedy—and go to sleep again. True, our slumber is not tranquil. It is broken by uneasy memories of past "scares," of shrieks of "Wolf!" when no wolf appeared. The public to-day seems hardly to believe in the survival of "wolves," and professors of political zoology are ever ready with the soporific assurance that predatory animals are extinct or are, at worst, chained up with international chains of gold. But neither recurrent panic, nor recurrent somnolence can do duty for a positive, conscious purpose. At the present moment our nation seems to have no positive purpose, no ideal. It is difficult to conceive a single issue that would unite all classes of the community as one man in timely and set resolve to see its purpose realized.

Whence proceeds this politico-moral disintegration? I cannot presume to analyse all its causes, but some symptoms strike me. Instruction, as distinct from education, is to-day more wide-spread than ever before. Parallel with the spread of instruction, and, to some extent, as a result of it, runs a progressive loss of faith in the older forms of belief, be the belief religious, social, economic or moral. The word "interest" is acquiring remarkable prominence. "Interest" is supposed to lie at the root of, and to provide a sufficient justification for, all things. The conduct of a private citizen, of a statesman, a party or a Government is judged in the light of his or its supposed "interest," by which the balance of immediately realizable advantage is usually meant. In diplomacy—that is to say, in the conduct of foreign affairs—a "policy of interests" has largely replaced what, in some countries at least, was formerly a policy of ideals. This development is, in part, an off-shoot of a mistaken interpretation of the German word *Realpolitik*, which is regarded as a synonym for Bismarckianism. Bismarck was a great though not a vague idealist, but he was, on occasion, unscrupulous to the point of criminality. In his own mind he probably thought that the greater good to be attained would outweigh the lesser evil to be done in attaining it. But his example has given rise to a school of politicians and diplomatists who reason thus: Bismarck was a great man; he was unscrupulous; therefore the unscrupulous are great. This fallacy is one of the parents of the mistaken conception of *Realpolitik*. The modern "aristocratic" doctrine of "supermen" springs from a similar stock. It is an inverse and perverse rendering of the sound and truly aristocratic Latin principle, *Aliis si licet, tibi non licet*, which it transforms into, *Si aliis non licet, tibi licet*. It tends towards social, moral and political anarchy. It is not on such a basis that the greatness of a nation can be founded or maintained. Signs of reaction against the narrower interpretation of *Realpolitik* are to-day visible even in Germany. The leading German pedagogue, Professor Rein, of Jena University, whose name is honoured by educationalists the world over, dealt not long ago with the problem of "Education to political judgment" (*Die Woche*, Heft 6, 1913), and insisted upon the need for education of the ethical will of the nation. He analysed with penetrating insight the doctrine of modern German politicians that moral elements have no place in international intercourse, and that the ruthless application of force or guile is the only task of the diplomatist. "What would it profit us," he exclaimed, "if we Germans should gain the whole world and lose our own soul?"

The tendency of modern diplomacy, or rather of the Governments for which it works, to pursue a policy of interests will, unless checked, reduce foreign affairs to a level little higher than that of banking, and statesmen to the level of concession hunters. No community, however enlightened, no public feeling, however generous, can be expected to grow enthusiastic over or spontaneously to support so mean a conception of foreign policy. I am inclined to attribute much of the present indifference towards foreign affairs to this progressive degradation of diplomatic endeavour. The only recent instance of diplomatic action having been inspired by a principle apparently higher than that of immediate separate advantage was during the Balkan wars of 1912–1913, when the Great Powers subordinated, for a moment, their individual cupidities to the avowed object of keeping the peace amongst themselves—a policy which resulted largely in acceptance of the claims of the least unselfish and most bellicose of their number. And even this result was speedily compromised by the readiness with which, when occasion offered, they one and all proceeded to barter their decisions, jointly sanctioned, against separate advantages promised them from a quarter which had reason to violate those decisions.

It has been said that a diplomatist is a man sent abroad to tell lies for the benefit of his country. The definition contains an element of truth, inasmuch as it indicates that foreign affairs are conducted in a rarified moral atmosphere in which ordinary notions of right and wrong, truth and untruth, lose much of their validity. There are doubtless unwritten rules and traditions that regulate the diplomatic game. A downright official lie is deemed reprehensible, especially when it is found out; then, a dozen ambassadors indignantly exclaim, "On n'est pas menteur *à ce point*." Deceit is admissible provided it be decently masked. Blundering falsehood is frowned upon less because it is falsehood than because it is blundering. The difference between a supremely expert and a less expert diplomatist may sometimes be defined in the terms of the comparison once made between the methods of Talleyrand and those of Metternich. "M. de Talleyrand," said a contemporary observer, "deceives without lying; M. de Metternich lies without deceiving." But were the possibilities of diplomacy confined to this sort of exercise, diplomacy would deserve to rank with the less exalted but unquestionably dexterous profession of thimble-rigging. Diplomacy involves other possibilities, and draws such respectability as it may possess from other assumptions. These assumptions are that behind the words and gestures of an ambassador stands the resolve of the State which he represents, and that behind the State stand the people of his country, determined to spend their blood and treasure in making good the claims of their accredited representative. But what are to-day the causes for which a nation would be ready to spend its treasure or shed its blood? What are the issues on which it would be prepared to stake its existence? The old spirit of the Crusades is dead. The old wars of religion are past, the causes of oppressed nationalities no longer appeal dynamically to the emotions of civilized communities. The idea that war is justifiable when its object, or its effect, may be to spread notions of liberty and justice among neighbouring peoples, as the French Revolutionary and Napoleonic armies spread political enlightenment in several parts of Germany, now appears ridiculous. For one nation to risk a conflict with another over a mere question of justice or humanity seems inconceivable. To fight for a railway or other industrial concession would surely be more rational and legitimate. Side by side with the shrinkage of moral forces proceeds the intertwining of financial and commercial interests that is supposed to be making of bankers and merchants the supreme arbiters of international affairs. In point of fact, there are few worse guides in foreign policy than international moneylenders. While nations are thus losing their moral impulses, they are piling up against each other the most terrific armaments the world has yet seen, and have, by thought and labour, attained a point of progress at which a battleship stationed in Mid-Channel could effectively and simultaneously bombard both Calais and Dover, unless it were blown up by an invisible submarine from below or sunk by explosives dropped from an almost invisible aeroplane above. Happy the era that can celebrate such triumphs! Yet the significant fact remains that hardly one of the nations which wield these fearful engines of destruction is, as a nation, conscious of any cause, save that of eventual self-defence, for which it would employ its armaments with the vigour, the spirit of organized self-sacrifice, the pertinacious courage which alone could make their employment thoroughly effective. Nations conduct their intercourse with timorous, suspicious hearts, encased in iron and bristling with the means of murder.

It is true that in all the marvels of technique that characterize our age one element stands out with refreshing and encouraging clearness. Old faiths may be dying or dead, old forms and formulas be shattered, old aspirations abandoned, but in the individual, though rarely in the mass, the ancient physical virtue of fearlessness remains. Nations may have learned to conceive the possibility of and to fear death, but individuals still count the chance of destruction a small thing in comparison with the joy of achievement, even though that achievement be apparently purposeless. The old Greek doctrine that the highest good lies not in the reward gained, but in the effort towards achievement, is again and again vindicated before our eyes. The pusillanimous question, "What is the good? Is it worth while?" is silenced by the general instinctive recognition that self-sacrifice and the joyful acceptance of risk are in themselves glorious things. While such instincts remain and heroes abound why be despondent? May not the energies absorbed to-day by the solution of dangerous technical or geographical problems be to-morrow directed towards the solution of the social, national, and international problems that now block the road to a higher civilization? Just as the technical achievements that excite our admiration are the outcome of years and decades of patient study and experiment, may not the possibility of progress upon other lines depend upon the dissemination and application of knowledge?

Doubtless; if the will to acquire, spread and apply knowledge be assumed to exist. But what national educational knowledge is to-day being spread? To judge by many utterances of public men it would seem that what the broad masses of the nation are supposed to need is flattery mingled with appeals to individual and class cupidity. The people are alleged to be "out for a good time," to have made a god of their bellies, and to care nothing for things that do not appeal to their appetites, tickle their curiosity, or amuse them in one way or another.

Fortified by an ignorance born of long absence, I decline to accept these allegations as exhaustive evidence. Not long since, sundry demagogues in the service of a political party endeavoured to arouse the enthusiasm of an English agricultural constituency by descanting upon the virtues of this or that particular plank in the party programme. They were startled by impatient interruptions from their hearers: "Tell us about Macedonia!" The People, I believe, needs to be lifted above the perpetual contemplation of its daily needs. After all, man liveth not by bread alone, nor even by flattery nor by boastful references to national or imperial greatness. The ideas of duty and justice, right and wrong will move hearts which appeals to personal or even national interest would leave cold. If our statesmen, those who *know* from intimate experience of public and international affairs, would speak to the people on questions outside the ordinary range of party politics; if professors and students would propagate and popularize the results of their studies; if newspaper owners and editors would remember that the idealism which lifts a man out of himself and a nation out of the muddy rut of every-day affairs is more attractive than even the details of a murder or the contortions of post-lascivious dances, the moral situation might change. At times, the conclusion seems irresistible that, having no faith themselves, our trustees of public opinion assume scepticism everywhere to exist. But it is my profound conviction, based, it is true, more upon experience of foreign countries than upon recent acquaintance with England, that no State can be powerful unless the action of its Government be based upon a public opinion, or, rather, upon a state of a public feeling, founded on sane idealism nourished by knowledge, and that the true strength of a nation consists less in the multitude of arms it possesseth than in every true and honest impulse that moves the heart of its citizens.

I would not have it thought that small importance is to be attached to the possession of arms and to efficiency in their use. The doctrines known as "pacificism," and all cognate apologies for national unreadiness based upon faith in the pure intentions of others, are, I believe, the surest pledge of disaster. Only a radical change in the present constitution of States, perhaps even a revolution in human nature itself, can alter the fact that, entrenched behind a stockade of national and international legal precepts, "force rules the world still." Force is the sanction of law, from the strong constable who seizes the burglar to the gun-boat that suppresses the pirate. Force also is the sanction of diplomacy, the *ultima ratio* of kings and peoples whose representatives diplomatists are. The problem we have to consider is what constitutes, in the modern world, effective force—to what extent physical power is valueless without the coefficient of moral strength used in the light of knowledge.

In a word, the question is one of education, education of the will as much as education of and instruction of the mind. The problem has never in the history of the world presented itself in quite the same form as it wears to-day. Universal suffrage; a cheap press ruled by the commercial spirit—a press given to sensationalism, striving to concentrate the attention of its readers upon one main topic for a short space of time, and changing the topic from day to day; the dissemination of superficial cognitions, or rather of visual impressions, by "pictures"—pictures in the press, pictures on the cinematograph film, pictures in the theatre. The appeal is to the eye, not to the reasoning intelligence, and rarely to the ethical instinct. Picture-books used to be considered accessories of the nursery. Does it follow that the present prevalence of pictorial representation is evidence of national childishness? Not necessarily. But just as a child may be bewildered by too rapid a succession of pictures, so the mind of a nation may be dazed by the impact of too many images in too rapid succession. The bewilderment of the British public mind is recognized and reckoned upon abroad. In those of our embassies and legations that are presided over by diplomatists—and there are still such—whose main solicitude is the welfare of their country, its greatness, its moral prestige, the despondent exclamation may often be heard : " If one only knew what the people at home care about !" Whereas a Russian, a French, or a German diplomatist will often go ahead at the risk of " breaking his neck," as the phrase runs, in order to make his country " tell," a British diplomatist has of late years been handicapped by the feeling that " people at home " know little, and care less, about the problems with which he is seeking to grapple, and that the Government which he serves will probably think more of him if he devotes his attention rather to commercial and financial details than to the larger issues of national influence. The problem to be faced here at home is how to generate an informed national will, and how to switch the power-current of public feeling on to the wires through which our influence is felt abroad. How can the public be made to distinguish between the permanent and the episodic ; how to appreciate the fact that, despite the rapid changes of modern life, the old principles of national efficiency, the co-efficients of national greatness, are unaltered ?

It can only be done by steady painstaking propaganda. Propaganda in the Universities, where the present generation of undergraduates is eager for knowledge and instruction in matters of national, imperial and international import. Propaganda in and through the press, which will rapidly open its columns and put its disseminating power at the disposal of national educationalists as soon as it is made to feel that such matters are as " interesting " as a football match. Propaganda from the platform, from the pulpit, in debating societies, and in working-men's clubs. Who is to undertake such propaganda ? One of the most influential and " initiated " of our diplomatists with whom I recently discussed this question, said, " A nucleus must be formed, a nucleus of volunteer national educationalists." Who can form this nucleus ? Every man or woman who believes it necessary to teach a higher national ideal than that of amusement and self-indulgence, who knows that in any contest it is grit, determination, pertinacity,and belief in some superior principle underlying phenomena that counts and guarantees victory ; everyone who holds that Gambetta's faith in " la justice immanente des choses " corresponds to some fundamental scheme of the Universe, some cosmic rhythm. Lest cynics and superior sceptics cavil at the ultimate practical value of such propaganda, let them be reminded of some historical examples. The injustice done to the kingdom of Poland in 1772 at the instance of Frederick the Great, and with the complicity of Austria and Russia, is to-day, as it has been for more than a century, a mill-stone round the neck of at least two of those countries. The ingratitude shown by Austria towards Russia on the occasion of the Crimean War —the requital, by an attitude of selfish " trimming," of a generous impulse on the part of a quixotic Tsar—induced Russia in 1866 to stand idly by while Prussian arms were crushing Austria, and led Russia, four years later, effectively to veto any combination that could restore the Hapsburg Monarchy to its place in Europe: the pusillanimity of the neutral Powers in sanctioning, in 1871, the annexation of Alsace-Lorraine by Germany—a sin of omission that has entailed, and is entailing, the crushing burden of European armaments and the maintenance of the huge idle armies that are a menace to civilization; and the injustice done to the Balkan peoples in 1878, when, from motives of short-sighted jealousy, and equally short-sighted "policy," the treaty of San Stefano was torn up by the Powers and the situation created which, through oppression, murder, and rapine, was to lead to the recent Balkan wars and to the present condition of unstable equilibrium, of which the end is not yet. Though I speak for myself alone, you will not, I think, refuse some weight to the testimony of a witness who has been obliged for more than twenty-one years continuously to observe the development of European affairs, if I declare it to be my profound conviction that no shrewd calculation of interests, no canny avoidance of moral responsibilities, avails to replace a sane ideal in the management of foreign affairs; and that if our nation is to come safely through the trials that may be in store for it, our people must again be taught a sound ideal—not, indeed, an ideal divorced from reason, but such as to inspire it and those who control its affairs with the belief that the thing chiefly needful is to know what is just and right, and to be ready and able to do it because it is just and right.

[H. W. S.]

GENERAL MONTHLY MEETING,

Monday, February 2, 1914.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

The Right Hon. Lord Avebury, M.A. D.L.
Herbert A. Humphrey, Esq., M.Inst.C.E.
Gerald J. Moody, Esq., D.Sc. F.C.S. F.I.C.
Arthur Ross, Esq., F.C.S. F.I.C.

were elected Members of the Royal Institution.

The Honorary Secretary read the following letter, received on behalf of the Honorary Member who was elected at the General Meeting on December 1, 1913 :—

OBSERVATOIRE D'ASTRONOMIE PHYSIQUE DE PARIS,
SIS PARC DE MEUDON,
SEINE & OISE.
13th December, 1913.

SIR,

I reply to your letter of the 8th of December in place of M. Deslandres, who, after an accident in which he broke his right arm, is unable to hold a pen.

Dr. Deslandres has received the letter of the Secretary of the Royal Institution, and the Diploma of Honorary Membership.

He begs you to convey to the Members of the Society his very best thanks for the great honour they have done him.

As soon as he can hold a pen, he will have the pleasure of thanking you himself by a personal letter—the President and the Secretary of the Royal Institution, and also Professor Dewar.

Please accept, Sir, the expression of my most distinguished sentiments.

(Signed) CH. LAMIABLE,
Secretary of the Observatory.

The Special Thanks of the Members were returned to Lady Huggins for her present of a portrait of the late Sir William Huggins, and to Mr. A. B. Bence-Jones for his present of three cases, containing two small Polariscopes and a Prism, given to his father, the late Dr. H. Bence-Jones, Honorary Secretary of the Royal Institution, by Faraday.

The Honorary Secretary announced the decease of Sir David Gill, K.C.B., on January 24, and Mr. H. F. Makins, on January 11, Members of the Royal Institution, and the following Resolutions passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and Astronomical Science in the decease of Sir David Gill, K.C.B. LL.D. D.Sc. F.R.S. F.R.A.S., Commander of the Legion of Honour, Knight of the Prussian Order pour le Mérite, and formerly H.M. Astronomer at the Cape of Good Hope.

Sir David Gill was a Manager in 1912-13. He delivered Friday Evening Discourses on "Recent Researches on the Distances of Fixed Stars and some Future Problems in Sidereal Astronomy" (1884); "The Application of Photography to Astronomy" (1887); "An Astronomer's Work in a Modern Observatory" (1891); and "The Sidereal Universe" (1911). He gave a Christmas Course of Juvenile Lectures in 1908-1909 on "Astronomy; Old and New."

Sir David Gill rendered important services to Science by his original investigations in Sidereal Astronomy; the Geodetic Survey of Egypt, Natal and Cape Colony; and in determining the Distances of the Heavenly Bodies. He was the author of many Works and Papers on Practical Astronomy and Geodesy, including a "History and Description of the Cape Observatory" (1913). He initiated the Photographic Survey of the Heavens known as the Astrographic Chart.

Sir David Gill was awarded the Gold Medal of the Royal Astronomical Society in 1877 and 1908; the Valz Prize of the Institut de France for his observations of the Transit of Venus at Ascension in 1877. He occupied the Presidency of the British Association in 1907, and of the Royal Astronomical Society in 1909-11.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Lady Gill in her bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution in the decease of Henry F. Makins, Esq., F.R.G.S.

Mr. Makins became a Member of the Royal Institution in 1870, and his Membership extended for the exceptionally long period of forty-three years. He always took a warm interest in the welfare of the Institution, and as Manager and Vice-President in the years 1906-10 and 1911-12, rendered valuable assistance in the transaction of business affairs of the Institution.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Mrs. Makins and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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Zoological Society of London—Proceedings, 1913, Part IV. 8vo.

WEEKLY EVENING MEETING,

Friday, February 6, 1914.

ALEXANDER SIEMENS, M.Inst.C.E. M.I.E.E., Secretary and Vice-President, in the Chair.

H. S. HELE-SHAW, LL.D. D.Sc. F.R.S. M.Inst.C.E. M.R.I.

The Mechanics of Muscular Effort.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 13, 1914.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Professor J. Norman Collie, Ph.D. LL.D. D.Sc. F.R.S.

The Production of Neon and Helium by the Electric Discharge.

The starting point of the following experiments by the author was due to a number of observations on gases that are evolved when many minerals are bombarded by the cathode rays in vacuum tubes. Amongst the minerals experimented on, fluor spar was found to give off a considerable amount of gas; on the suggestion of Sir William Ramsay this gas was examined for helium and neon, with the result that a small amount of neon was found. This observation led the author to a long series of experiments, the interest of which lay in the fact that helium often as well as neon made their appearance in various tubes through which the electric discharge had been passed in a high vacuum.

From the beginning, however, it had been always obvious that as air contained both helium and neon great care must therefore be taken, firstly, to entirely get rid of all the air in the experimental tube by pumping it out and subsequent washing out with some pure gas, oxygen or hydrogen; secondly, to be sure that no air leaked into the apparatus during the experiment.

About four months after the author had noticed the occurrence of neon in the gases given off from fluor spar when bombarded by the cathode rays, he heard that Mr. H. S. Patterson, who was working at the University of Leeds, had obtained similar results; since then they had worked together, and later on Mr. Masson, of University College, London, had joined in the research. The idea of Mr. Patterson that had led him to his experiments was that if it were possible by means of the electric discharge to impart to hydrogen atoms an extra electric charge, these hydrogen atoms might possibly be converted into helium atoms.

In the joint work of the author with Mr. Patterson and Mr. Masson a large number of different forms of apparatus were employed, designed to test in every way conceivable what were the best conditions for the production of the gases, and at the same time exclude the possibility that the gases were occluded in the glass of the tubes or in the metals used as electrodes. In one way many of the experiments were disappointing, for it was found that very often an experiment that had yielded the gases, when it was repeated gave none, but at the same time there were many positive experiments where it was almost impossible that the gases were due to any leak in the apparatus.

Some of the more important experiments were as follows: to prove that the gases were not occluded in the aluminium electrodes used, the aluminium was both fused in vacuo and the gases evolved tested; aluminium was also dissolved and the hydrogen produced examined: in both cases neither helium nor neon could be detected. The glass used in making the experimental tubes was treated in the same way and negative results obtained. Glass bulbs were evacuated as far as possible by means of the mercury pump, and washed out with pure gases, and then heated for hours almost to their softening point, but no helium or neon leaked through from the outer air. Large quantities of mercury were boiled in vacuo, but not a trace of helium or neon was given off. In another experiment the tube was entirely jacketed by another tube that was so far evacuated as not to allow the electric spark to pass; neon was found as usual in the inner tube, but what was remarkable, helium with only a small amount of neon was present in the gases in the outer tube. In another experiment a large glass bulb was surrounded by a coil of wire and a powerful electrodeless discharge used: helium was found inside the bulb. Different metals were used as electrodes, copper, silver, platinum, iron, potassium, sodium, lithium, magnesium, uranium, etc., and in nearly every case helium, or helium and neon, proved to be present in the gases that were pumped off from the experimental tube. A quartz mercury lamp that had been in use over 3000 hours was found to contain a relatively large amount of helium and neon. A special form of lamp was therefore constructed consisting of an inverted U-tube made of quartz, the end dipping into mercury in two large test tubes, so arranged that the level of the mercury at the top of the U-tube could be raised and lowered. A side tube from the top of the apparatus led to a pump so that the gases could be pumped out and examined. This lamp was running for over two months, and gave helium mixed with a small quantity of neon during the whole period. For the first few weeks the top of the U-tube was surrounded by the air, but for the rest of the time it was enclosed in a jacket filled with water; the reason for doing this was that quartz is known to be pervious to helium and neon at high temperatures, i.e. 1000° C.

The summary of all these experiments had been: helium and neon did not come from the glass of the apparatus; the electrodes could not be made to give either of these gases by heating or by dissolution, and as far as possible all leak of air into the apparatus was excluded, yet in a very large number of experiments both helium and neon appeared in the tubes after a powerful electric discharge had been passed for some hours.

In December 1913, Professor Strutt, in a note to the Royal Society, stated that he had repeated the experiments of the authors, and had failed to confirm them. He suggested that pumping out the gases and transference to another apparatus for the purpose of testing for helium and neon, would favour leakage of small quantities of air, quite sufficient to account for the helium and neon obtained by the authors. He had therefore used a form of apparatus in which all the operations could be carried out without any such transference. To account for Professor Strutt's failure to obtain positive results was difficult, for, as had been already pointed out, the authors found great difficulty often in repeating any particular experiment; moreover, in several of the positive experiments apparently helium but no neon appeared in the experimental tube.

This fact cannot be explained by air leakage, for air contains four or five times as much neon as helium. At present the investigation was only begun; many more experiments would have to be made, for the source of the helium and neon was still obscure; but if it could be proved that these gases were produced from many metals and other substances under the influence of the cathode discharge, it it obvious that it would be a discovery of the most far-reaching importance

[J. N. C.]

WEEKLY EVENING MEETING,

Friday, February 20, 1914.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Professor Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S.,
Conservator of the Museum, Royal College of
Surgeons, England.

An Anthropological Study of some Portraits of Shakespeare and of Burns.

[abstract.]

I became interested in the bust and numerous portraits of Shakespeare in the following manner. Three years ago Mr. H. Oatway came to the Museum of the Royal College of Surgeons, bringing with him a terra-cotta mask he had discovered in the shop of a curio-dealer in one of the English Midland counties. The mask* represented the features of Shakespeare—those features with which the monument at Stratford-on-Avon has made us familiar. There were numerous differences in detail when the bust and mask were compared, and the question had arisen whether the newly discovered mask might not be a squeeze—a modelled-up squeeze was the term Mr. Konody used in his description—from the cast which is said to have been taken from Shakespeare's face after death. To assist me in settling the problem, Mr. Oatway helped in every way; he placed at my disposal measurements of the original bust at Stratford, two casts made from the bust, one of them being the well-known Bullock cast; and he obtained photographs of the various portraits and busts which are believed to represent Shakespeare. My investigations of the Oatway mask yielded only negative results. As in the original bust, the eyes are open; the eyelids and eyeballs are modelled on conventional lines; the lips are full, slightly parted and shaped so as to represent the condition in life. Neither the original bust nor the Oatway mask show any trace or mark of having been taken from a cast or model of a dead man's face. It was also plain that there was a direct genetic relationship between the original bust and the Oatway mask, for every curl in the hair of the original bust was accurately reproduced in the terra-cotta mask. The features of the

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* The mask is described and figured by Mr. P. G. Konody in the "Illustrated London News," July 17, 1911.

Stratford—the original—bust appeared to me to be modelled about 10 per cent above the dimensions of an average Englishman's face. In Mr. Oatway's mask the features come within the normal range. In particular the nose is smaller and more compressed than in the original—in taking a death mask the nose and lips are always compressed by the weight of the plaster. We also found, however, that a terra-cotta mask shrinks as it dries, and its various parts do not shrink equally. Although the origin of Mr. Oatway's mask cannot be regarded as finally settled, yet it is not unlikely that it represents a cast from the monument at Stratford-on-Avon, and that its peculiarities are due to unequal shrinkage. In this manner I was introduced to the mysteries of Shakespeare's portraits.

It was a fortunate circumstance that at the time I was investigating Mr. Oatway's mask, Mr. M. H. Spielmann * was finishing a systematic examination of the evidence relating to the history of the monument at Stratford-on-Avon, and to the authenticity of the numerous portraits of Shakespeare. His final opinion is that only the monument itself and the Droeshout portrait can be regarded as well authenticated—as having been in existence soon after Shakespeare's death in 1616. As regards the monument, even Mr. Spielmann has reservations; he is of opinion that its sculptor, Gerrard Johnson, "evolved" the head and neck, and that there is an inconsistency between the proportions of the head and face. In his exciting book, entitled "Bacon is Shakespeare," the late Sir E. Durning Lawrence refused to regard either the monument or the Droeshout portrait as an authentic document. As regards the portrait, he has written (The Shakespeare Myth, 1912):—

> "It is almost inconceivable that people with eyes to see, should have looked at the so-called portrait for 287 years without perceiving that it consists of a ridiculous, putty-faced mask, fixed upon a stuffed dummy, clothed in a thick coat."

The monument, in Sir Edward's opinion, represents a flat-headed Chinaman, and was made, he believes, more than a century after Shakespeare's death. The doubts cast on the authenticity of the Monument, first by Mrs. Stopes ("Monthly Review," 1904), and later, by Sir Edward Durning Lawrence, have been cleared away by the late Mr. Andrew Lang ("Morning Post," 5th July, 1912), and by lectures given at the Royal Institution by Mr. M. H. Spielmann in the same year. Anything we can know concerning the personal appearance, and of the racial character of the greatest Englishman has to be gathered from these two sources: the Monument at Stratford-on-Avon, and the Droeshout portrait. There are two other

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* Mr. Spielmann is now preparing for publication a work which gives the results of his enquiries into the Shakespeare portraits. See his "Essay," reprinted from the Stratford Edition.

documents relating to Shakespeare which have a special interest for me One of these is the Davenant bust now in the Garrick Club, London. It was discovered in 1849, when the Duke's Theatre in Portugal Row, Lincoln's Inn Fields, was demolished to make room for an extension of the Museum of the Royal College of Surgeons. It was, at one time, in the possession of my predecessor, Sir Richard Owen. In the same year (1849) Dr. Becker came from Germany, bringing with him the "Kesselstadt mask" of Shakespeare, which he submitted to Owen for examination. I do not think that either the bust or the mask * can be regarded as "original documents."

If, then, we are to know the outward appearance of Shakespeare —to measure those characters of the head which are of particular interest to students of the human body, we must deduce them from either the Droeshout portrait or the Stratford Monument. If the bust represented on the Monument is largely imaginary, then the "imaginings" of the sculptor will become manifest when we apply to it the various criteria which are used when heads and skulls are examined by modern anthropological methods. The remarkable dimensions which the sculptor of the Stratford bust has given to Shakespeare's head become evident when an exact drawing is made of the crown from above (Fig. 1). Within a drawing of the crown, I have placed a drawing of a skull of a modern Englishman—poised in the same position and drawn to the same scale as the Shakespearean bust. It is at once seen that there is a double contrast—one of shape and one of size. The skull of the Englishman, slightly under average dimensions, is 188 millimetres long; when covered with flesh, the head would have been 8 millimetres more—196 millimetres. Shakespeare's head is 212 millimetres long; his skull should have measured 204 millimetres—a long skull—reaching well towards the maximum limit of cranial length. In width, there is a much greater difference; the modern skull is 137 millimetres wide; in life, the width of the head would have measured 10 millimetres more —147 millimetres. The width of Shakespeare's head, as represented in his bust, is 168 millimetres. His skull should have measured 158 millimetres—a wide skull—reaching well towards the limits of maximum width. In the modern skull, the width represents 73 per cent of the length; in Shakespeare's bust, the width represents 77·5 per cent of the length. As tested by measurement, Shakespeare's head falls neither into the long-headed group nor into the round or short-headed, but within the intermediate group. Yet, when we come to examine the bust in profile, all the features which characterize the short-headed or round-headed type of men become manifest.

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* I have not seen this mask, but from the drawing and measurements given by Mr. William Page ("Study of Shakespeare's Portraits," 1877), I have come to the conclusion that the mask is not that of Shakespeare.

To those who have given no special attention to the form of human heads, it may seem that the short-headed and the long-headed are but variants of one type. The difference is much greater than a mere individual variation ; it is a radical difference. It is fortunate for my present purpose that Robert Burns was a good representative of the long-headed type of man. In Fig. 2, the profile of his skull is contrasted with a drawing made from the skull of the short-headed race which reached England for the first time at the close of the neolithic period—some 2,000 years before the time of Christ. A glance at those two figures reveals an essential difference in conformation—a difference which is chiefly confined to that part of the head which lies behind the ear-holes. In Burns, the long-headed, the occiput forms a backward, projecting eminence ; in the short skull, the occiput is flattened as if it had been compressed from behind by the application of a board during infancy. There is another difference between these two types of heads ; in the long head, the vault is low and flat ; in the round head, the vault wells upward to a crown, as if the brain, when compressed from behind, had forced up the middle part of the roof of the skull. How it has come about that there is this radical difference between the two prevailing types of human heads in Europe, we do not know ; there is a secret hidden here which has not been discovered as yet. All

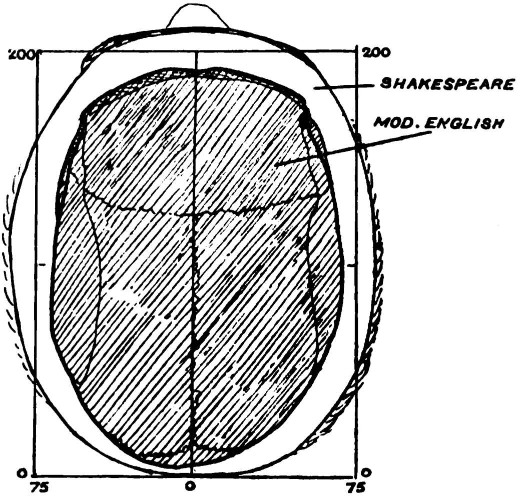

FIG. 1.—The crown of Shakespeare's head as represented in the Stratford bust, with the corresponding view of a skull of moderate size. Both are drawn to the same scale.

we are certain of is that the long heads represent one derivative of the common human stock: the round heads, of the type here described, belong to a different branch of that stock.

When we come to study the Shakespeare bust in true profile, there can be no doubt as to the type of man it represents; he is depicted for us in the Stratford Monument as a representative example of the short-headed type. All but in one respect: the dimensions exceed those of any skull of this type known to me. We have seen that, in length and breadth measurements, Shakespeare's head approaches maximum diameters. This is also true of its height. The bust represents the crown as rising 150 millimetres above the level of the ear-holes—for, although the ears are covered by the ample side-locks of hair, the position of their passages can be fixed approximately. The "auricular" height of the skull—deducting

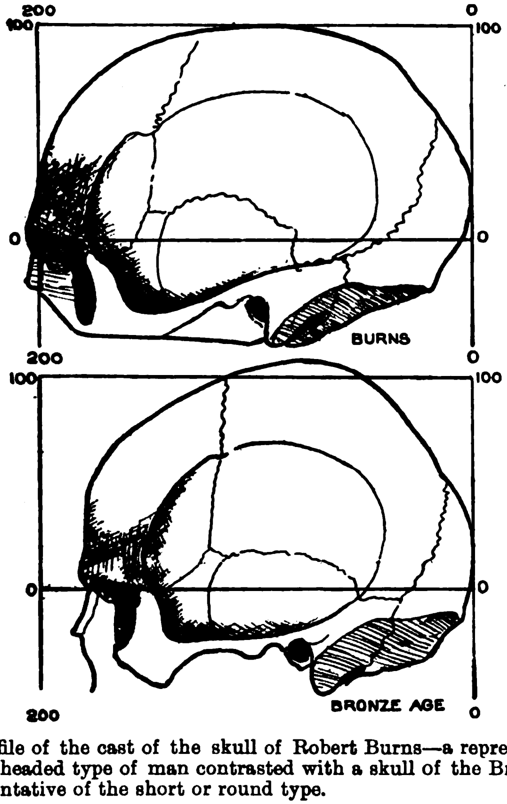

Fig. 2.—Profile of the cast of the skull of Robert Burns—a representative of the long-headed type of man contrasted with a skull of the Bronze Age—a representative of the short or round type.

7 millimetres for the thickness of the scalp on the crown—should have measured 143 millimetres—almost a maximum amount. When I first noted those maximum dimensions of the head of Shakespeare, as portrayed by Gerrard Johnson, I was inclined to suppose the good sculptor had wished to give the poet a head which would be in keeping with a brain of marvellous power. Either that, or he had taken a sculptor's licence with the absolute dimensions of Shakespeare's head, and enlarged them to give to his subject that imposing dignity which increased size can give a modelled head. The width of the forehead, at the level of the eyebrows, is represented in the Stratford bust as 120 millimetres—quite 10 millimetres beyond what a forehead, shaped as Shakespeare's is, should measure. The face is 15 millimetres longer than one would expect. If we suppose that the sculptor had added 8 to 10 per cent to the exact measurements of the head he actually portrayed, then, in every respect, the head represented in the Stratford Monument as that of Shakespeare becomes a representative specimen of the short-headed type. I had reason, when seeking for an explanation of certain

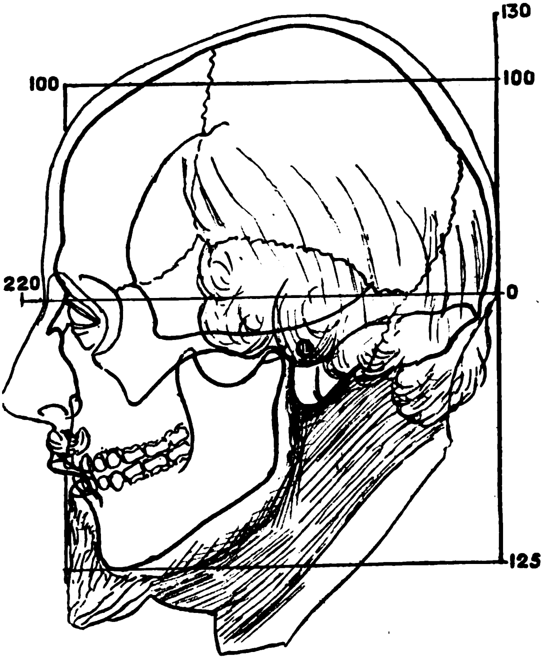

Fig. 3.—Profile of Shakespeare's bust, with drawing of a skull of a Bronze Age man inset. The dimensions of the skull have been enlarged 10 per cent.

features of Shakespeare's forehead, to measure Chantry's bust of Sir Walter Scott in the National Portrait Gallery. I found in that case —and it is so in many others—the sculptor had modelled the head about 10 per cent above the actual dimensions. I think Gerrard Johnson took a similar liberty.

In Fig. 3 I have taken the skull of a man who lived in Kent during the Bronze Age—the period which succeeded the Neolithic—and have done, in regard to the dimensions, what I suppose Gerrard Johnson did to Shakespeare's features—enlarged them 10 per cent. It will be seen how accurately the skull—thus enlarged—fits within the Stratford bust. From the accuracy of the fit, we may be certain that the man who wrought the Stratford bust did not portray an imaginary head, but had before him an actual subject or model, and that subject was of the short-headed type. Was it Shakespeare's head which was portrayed? Mr. Spielmann assures us that the men who paid for the Monument knew Shakespeare's features and were satisfied with the likeness. They would not be satisfied, we may presume, with a radical alteration in the shape of the head. We cannot suppose that the sculptor gave the poet a typical short head to satisfy those who had paid him for a true likeness.

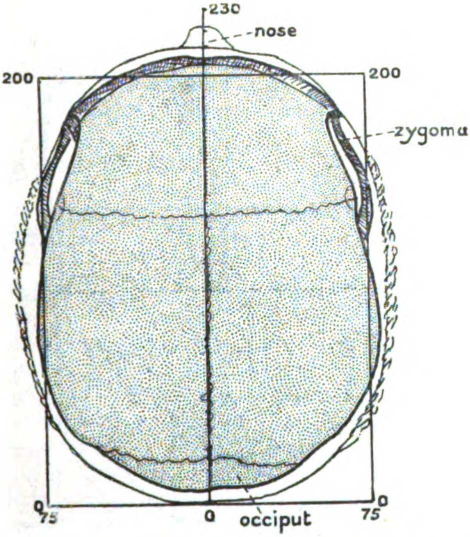

Fig. 4.—The crown of Shakespeare's bust from above, with the corresponding outline of a Bronze Age skull inset. The skull has been enlarged 10 per cent.

It is not only from the side that the Bronze Age skull fits within Shakespeare's bust: the view is equally good when we view its inset from above and from the front (Figs. 4 and 5). The poet is thus represented—if we allow a reduction of 10 per cent in face and head—as a man with facial features of average dimensions. The face, when so reduced, measures from the root of the nose to the lower margin of the chin, just over 120 millimetres; its width, from the zygomatic arch of one cheek to that of the other, is a little over 130 millimetres. The mean length of the face of a modern Englishman is 120·7 millimetres; its width, 132 millimetres. Indeed, if we presume that the sculptor added about 10 per cent to the actual

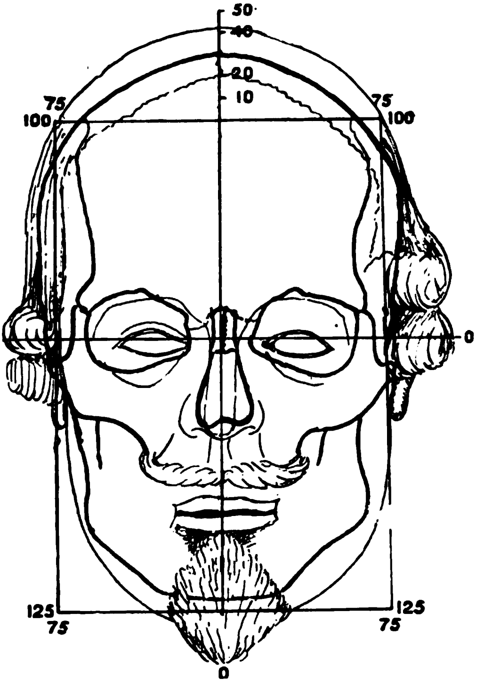

FIG. 5.—Full-face view of Shakespeare's skull, with similar views of a Bronze Age skull inset. The skull is enlarged 10 per cent.

dimensions, then, in size of head and face, Shakespeare might be regarded as a representative individual of the short, or round-headed type.

If there is any truth in the Stratford bust—if it represents the features of Shakespeare—then there is no doubt that our national poet is a product of the round-headed branch of European humanity. We are certain, at least, that Shakespeare does not belong to the early British breed, for not a trace of a "round-head" has been found in

England earlier than the age at which men began to replace their stone weapons with implements made of bronze. In the Neolithic period we have evidence that the "round-heads" were pushing their way westwards through Central Europe towards the coasts of the North Sea. In the Bronze period, commencing about 2000 B.C., they began to arrive in Britain. They gradually permeated every part of the country, England, Wales, Scotland and Ireland, but at no period did they form the majority of our population. If I call those round-headed people Celts, I use the term in the same sense as Continental anthropologists do, as round-headed people speaking a Celtic tongue. In England we speak of the Celtic fringe, and refer this fringe to the people of Wales, the western parts of Scotland and of Ireland. The majority of the inhabitants of these parts are long-headed people; they are permeated by a round-headed element added by the invaders of the Bronze Age. The people of the western fringe represent, in the main, the original long-headed stock of this country. At a later date, in the Roman and Saxon periods, the additions made to our population were of the long-headed type.

Shakespeare is a descendant of the Bronze Age invaders, the true, or round-headed Celts. That is a remarkable fact, for it is this same short-headed stock, spread abroad in Central Europe, throughout Germany, France and Italy, which has produced the world's finest artists.

Looking away for a minute from the question of type, it is of interest to see what size of brain the Stratford bust suggests for Shakespeare. If we accept the dimensions of the bust as real and not enlarged, we must estimate the dimensions of the skull as follows: length, 204 millimetres; width, 158 millimetres; auricular height, 143 millimetres. Employing one of the formulæ invented by Miss Lee and Professor Pearson for estimating the size of the brain from the dimensions of the skull, we find that Shakespeare's brain capacity was a little over 1900 c.c.—not an impossible amount. Cuvier's brain capacity was over 1900 c.c.; Cromwell and Byron are said to have had brain capacities well over 2000 c.c. The average Englishman has to be content with a capacity of 1477 c.c.; the average Scot, according to Sir William Turner,* with one of a cubic centimetre more. Scotland also had her invasion of round-heads during the Bronze Age, and, as in England, the type still persists, forming nearly 30 per cent of her population. Sir William Turner noted that the people with the long heads had the larger brains. He found that the mean capacity for short-headed Scots is 1469 c.c.; for long-headed ones, 1516. The short-headed type has the smaller brain. If we suppose, and I think we must accept the proposition, that the Stratford head has to be reduced about 10 per cent to find the true dimensions of Shakespeare's brain, then the power of the poet's brain must be ascribed to quality rather than quantity. In the reduced scale, the length of the head would be 184 millimetres; width, 143 millimetres; auricular height, 129 millimetres; the brain capacity, 1540 c.c., a rather large brain for a short-headed man.

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* Craniology of the People of Scotland: Trans. Roy. Soc. Edin., 1903, vol. 40, p. 547.

I shall deal briefly with Shakespeare's features as represented in the Droeshout portrait. I feel certain that it is a rather crude engraving made from some previous portrait or drawing which was placed at Droeshout's disposal five or six years after Shakespeare's death. Such a portrait as that known as the "Felton," now in the possession of Mr. Burdett-Coutts, might well have served as the original from which Droeshout worked. The Droeshout portrait is simply the Felton portrait, with certain marks of age added—loss of hair and bagginess beneath the eyes. The loftiness and the modelling of the forehead in the Felton and Droeshout portraits suggest an abnormal development of Shakespeare's head. In every race of mankind there occasionally appear individuals with remarkably high heads—individuals in whom the vault rises up to form a "tower," or "sugar-loaf." The condition is due to a maldevelopment in the base of the skull. The front part of the base is prematurely arrested in its growth, with the result that the developing brain, in order to find room, has to raise up the crown into a characteristic sugar-loaf

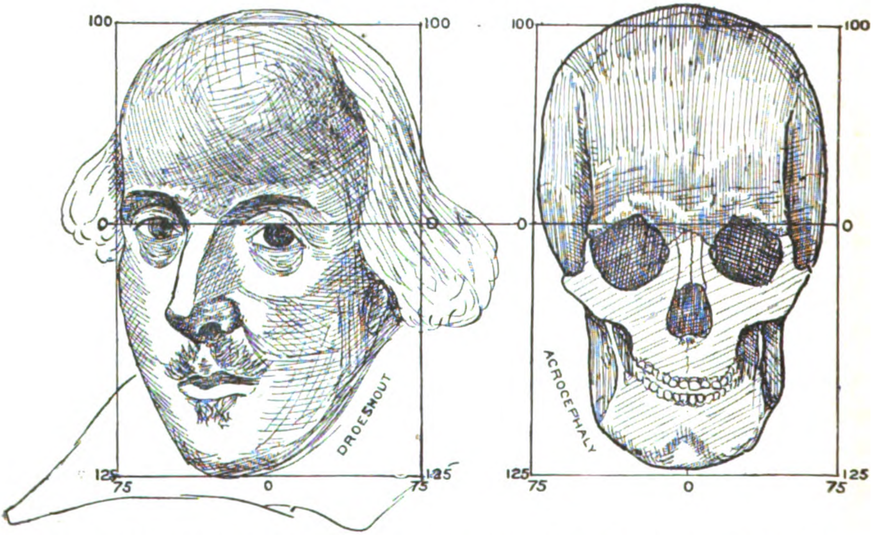
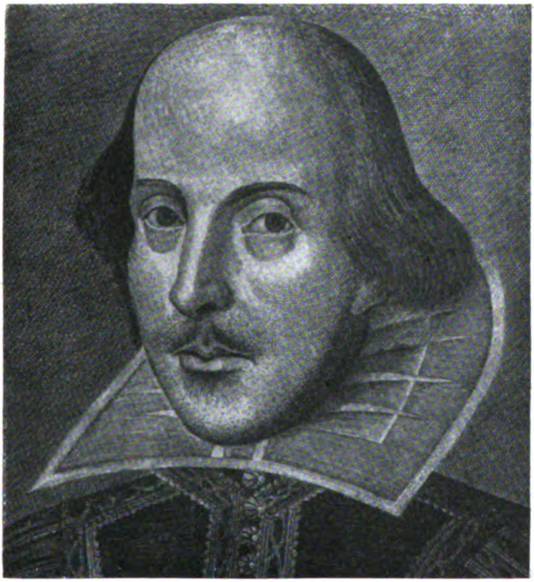


FIG. 6.—A free copy of the Droeshout portrait, compared with an acrocephalic skull.



Shakespeare: Droeshout Portrait.

SHAKESPEARE: MONUMENT AT STRATFORD.

form. The condition is now recognized as a distinct kind of maldevelopment known as *Acrocephaly*. Sir Walter Scott was certainly the subject of this condition, at least to a mild degree. Was Shakespeare also a subject of acrocephaly ?

If the Droeshout portrait be set side by side with a skull which shows the acrocephalic condition (Fig. 6), it is seen that the vault of the skull rises higher than does the dome of the Droeshout head. When the acrocephalic skull is viewed in profile and placed within the outline of the Stratford bust, it is seen that all grounds for suspecting acrocephaly in Shakespeare disappear. The most characteristic feature of the acrocephalic head is its shortness from back to front. The head of Shakespeare's bust is absolutely long. Yet, both bust and portrait show certain features with which we are familiar in acrocephalic skulls, features which do not exist in skulls of the Bronze Age type. The short skulls of the Bronze Age period are remarkable for their prominent, well-developed eye-brow ridges. In acrocephalic skulls these ridges are not well marked, and the lower

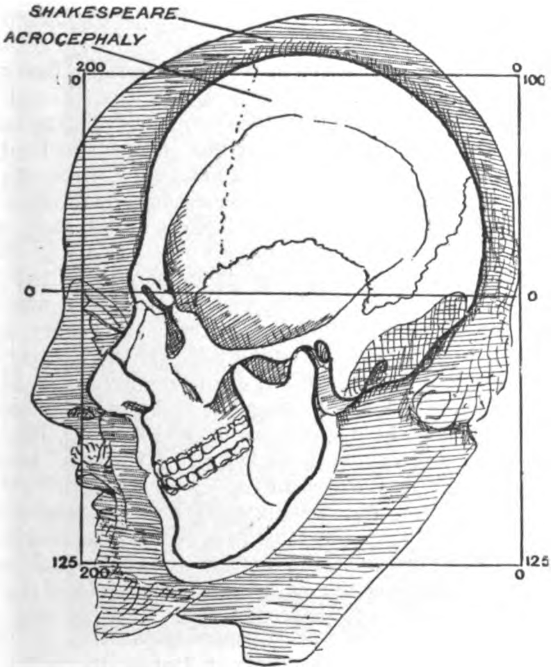


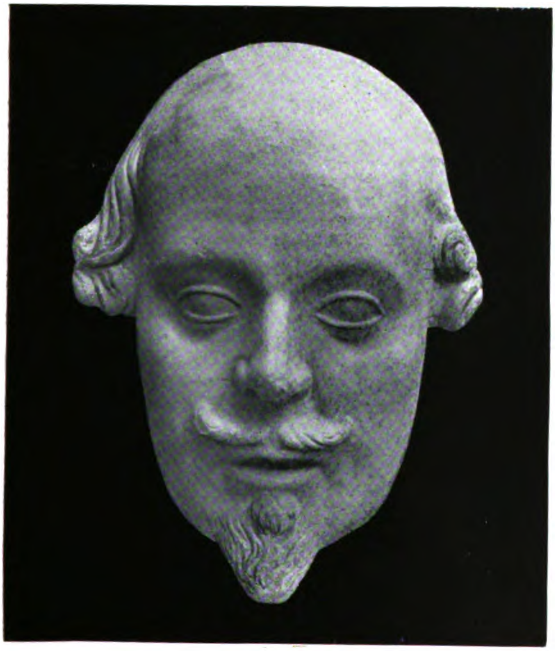
FIG. 7.—Skull, showing the malformation known as acrocephaly, set within a profile of the Stratford bust. Both are drawn to the same scale.

part of the forehead is often retracted or drawn inwards. But it is also true that in sensitive, less robust, delicately moulded human heads of the short type, the eye-brow ridges remain undeveloped. I am, therefore, of opinion that the lofty dome given to Shakespeare's head is due, not to acrocephaly, or any pathological condition, but represents the well-sprung vault and delicately-moulded forehead seen in the more highly evolved of modernized round-heads.

I have already mentioned that my attention was drawn to the portraits and bust of Shakespeare by the visit of Mr. Oatway to the Museum of the Royal College of Surgeons. It was a similar happy chance which sent certain information concerning Robert Burns my way. Less than a year ago, I received a letter from Mr. Barrington Nash, well known to students of Raeburn and of Burns, informing me that he had in his possession two valuable documents relating to both his heroes. One of them was a life-size portrait of Burns by Raeburn; the other a cast of the poet's skull. I was not aware of the existence of either of them, but a visit to Mr. Nash's studio in Chelsea made me realize their great and permanent value, especially their utility for my present purposes.

That the cranial cast, which has been generously presented to the Museum of the Royal College of Surgeons by Mr. Nash, was made from the poet's skull there can be no doubt. Its history is well known. Burns died in 1796 at the age of 37. In 1834, thirty-eight years after the poet's death, his widow, Jean Amour, died. At that time Dr. George Combe was stimulating the study of phrenology in Scotland, and, when the grave was opened in 1834, Dr. Blacklock and Mr. McDiarmid, Editor of the "Dumfries Courier," took the opportunity of making a plaster cast of the poet's skull. Dr. George Combe published very accurate drawings of the skull-cast with an account of his examination, couched in the phrenological language of the day. The cast, as may be seen from the drawings which accompany this paper, is not complete; it includes the whole of the part which contains the brain, but, in front, stops short at the lower borders of the orbits.

The outstanding features of the skull of Robert Burns becomes at once apparent when we set it within the profile of Shakespeare's bust—both being drawn to the same scale and orientated on the same plane. We have seen that Shakespeare's head has been represented in large dimensions, but, as regards length, the Scottish skull is the longer. Its length—206 millimetres—is remarkable. In 117 skulls of Scotsmen, drawn from all parts of the country, Sir William Turner found eight which measured 200 millimetres or more in length, but the longest was 2 millimetres shorter than the poet's skull. In life his head must have measured 216 millimetres in length—a head of altogether exceptional length. Examination of Fig. 8 also shows that the heads of Shakespeare and Burns represent contrasted types—one is an example of the short-headed, the other the long.



SHAKESPEARE: THE OATWAY MASK.

The two national poets come of radically different stocks. In Fig. 2 I have already contrasted Burns' skull with the Bronze Age type. If the projecting occiput of the Scottish skull were pressed forwards and the vault consequently raised, it would fit comfortably within the outline of the Stratford bust (see Fig. 8).

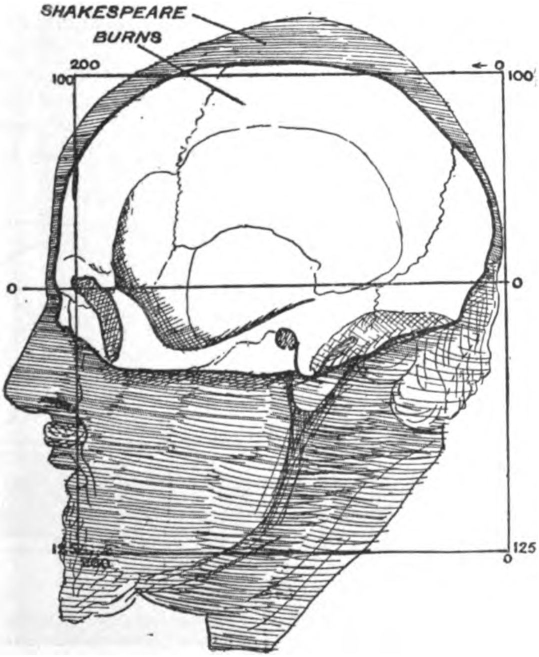


FIG. 8.—Profile of the cranial cast of Robert Burns set within a profile of the bust of Wm. Shakespeare. Both are drawn to the same scale.

The remarkable size of Burns' skull becomes apparent when we contrast it with a skull of ordinary dimensions. In Fig. 9 an instructive comparison is made, the contrasted skulls being examined from behind. Burns' skull is very broad—155·5 millimetres, as contrasted with 137 millimetres—the width of the skull used for comparison. Its height is less remarkable; the vault rises 125 millimetres above the ear passages. In the skull used for comparison the auricular height is 116 millimetres. From these dimensions, it will be seen that the poet's brain was much above the average size. Using the same formula as was employed in estimating the size of Shakespeare's brain, we find that the capacity of Burns' skull must have been about 1730 c.c. In the average Scot, the capacity is 1478 c.c. Burns had a brain at least 200 c.c. above the average of his countrymen. The length of the cast is 206 millimetres; its width, 153·5 millimetres; the width is 74·5 per cent of the length. In type of skull, Burns belonged to the long-headed group.

When a profile view of Burns' skull is compared with a corresponding view of a skull of the same type, but of rather less than average capacity, its remarkable dimensions become apparent (Fig. 10).

There are two features of the poet's skull which are worthy of attention. One is its excessive length. As regards height. there is little difference between the type skull and the poet's. One other feature is the compression, or apparent flattening, of the poet's skull in a vertical direction. The part of the base to which the neck was attached, is shaded in both skulls drawn in Fig. 10. The area for the attachment of the poet's neck is exceptionally flat In life, his head must have appeared to be closely set to the neck. That feature is very apparent in Raeburn's portrait of Burns. It is not so apparent in the Nasmyth portrait, for reasons to be given later.

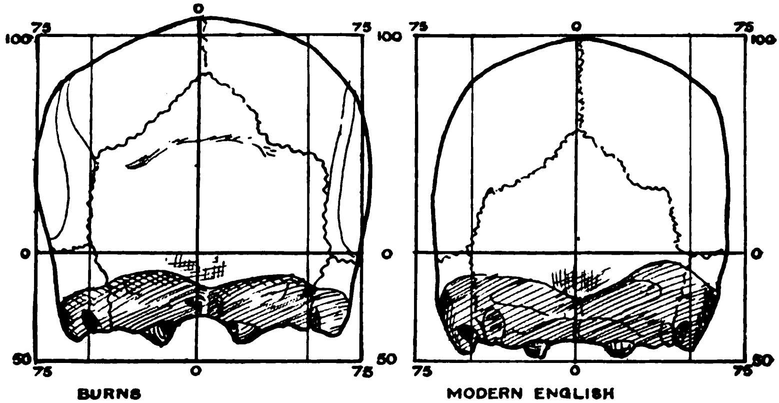
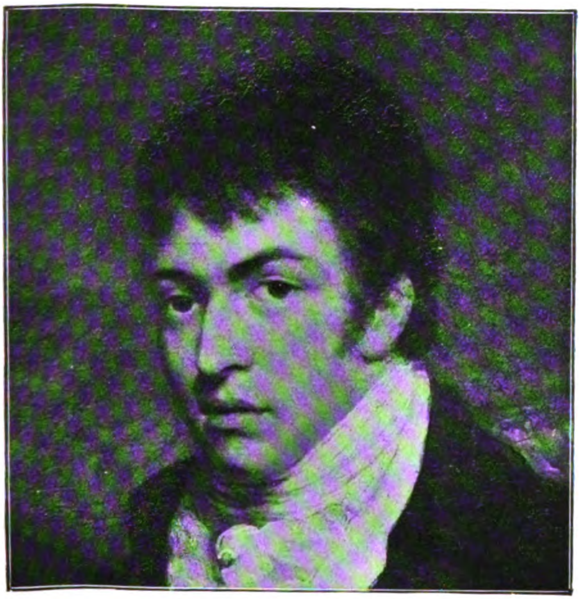
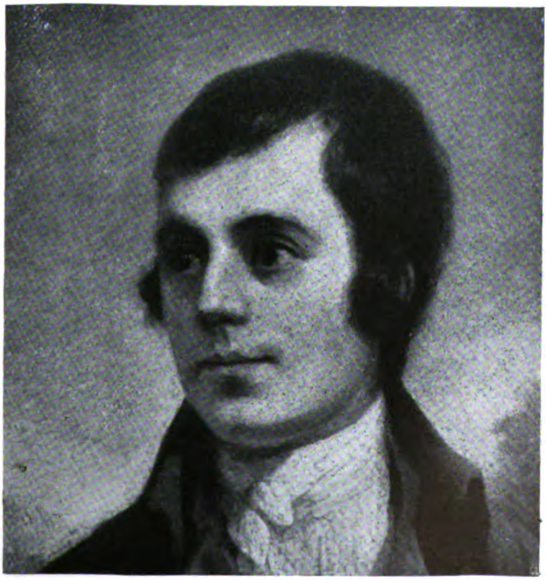


Fig. 9.—Posterior view of the cast of Robert Burns' skull contrasted with a modern skull (capacity 1425 c.c.). Both are drawn to the same scale, and orientated on the same plane.

The one feature which strikes the expert when Burns' skull is viewed from the front, and contrasted with a skull of moderate dimensions, is the size of the orbits (Fig. 11). They are of average width (39 millimetres), but their height or vertical diameter (45 millimetres) is altogether uncommon—11 millimetres more than Sir William Turner found to be the average in skulls of Scotsmen. The eyebrow ridges are strongly marked in contrast to the slight development indicated for those of Shakespeare in the Stratford bust. The face



Burns: Raeburn's Portrait.



Burns: Nasmyth's Portrait.

was slightly above the average in width (135 millimetres), and from the various portraits we may infer it had about the average length—120 millimetres. Thus, while Burns' face was of average size—with long, aquiline nose, deep, capacious eye-sockets and robust eyebrow ridges—his head or cranial case was far above the average size. Sir Walter Scott was of opinion that the portraits of Burns failed to represent the massiveness of his features. The artist probably found that if he gave full value to the head, the face appeared dwarfed.

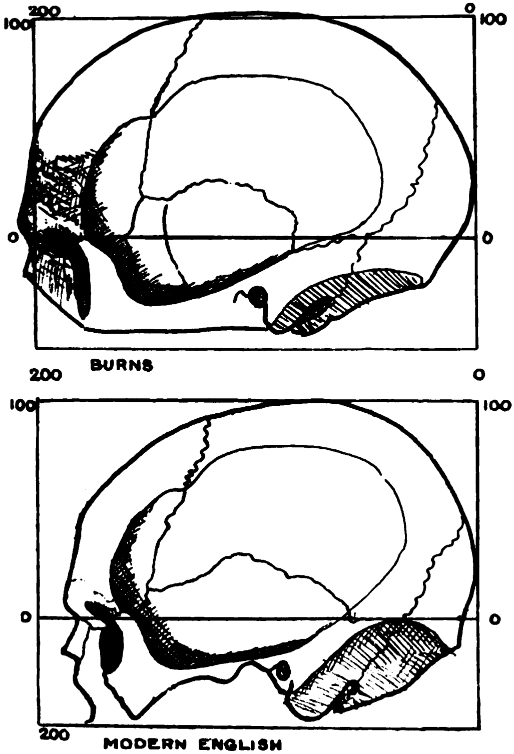


Fig. 10.—Profile of Burns' skull compared with another of the same type, but of average dimensions. Both are oriented on the same plane, and drawn to the same scale.

Burns' skull represents an ancient type. In Fig. 12, I have reproduced a drawing of his skull from above; side by side, I have placed a similar drawing of the Olmo skull. This skull was discovered deeply embedded in a deposit formed in the bottom of an ancient silted up lake, in the North of Italy. The animal remains found in the same deposit belong to an early part of the Pleistocene period. In size and shape the skulls of the very ancient Italian and of the Scottish poet are in close agreement.

It is when we enquire into the racial origin of Burns that the value of the cranial cast as a document becomes evident. As I examined the cast, its resemblance to certain skulls discovered and described by Professor Bryce became manifest. In 1896 and again in 1900, Professor Bryce explored the Cairns of Arran * systematically and thoroughly, and placed the results of his investigations on record. In two cairns—those of Torlin and Clachaig—he found the remains of a number of human individuals; six of the skulls were sufficiently preserved to give us an exact conception of the head form of the people who lived in Arran and interred their dead in megalithic chambered cairns. They were people living in the Stone Age, before bronze was introduced to Scotland. We may safely regard them as belonging to a period which ended at least 2000 years before the time of Christ.

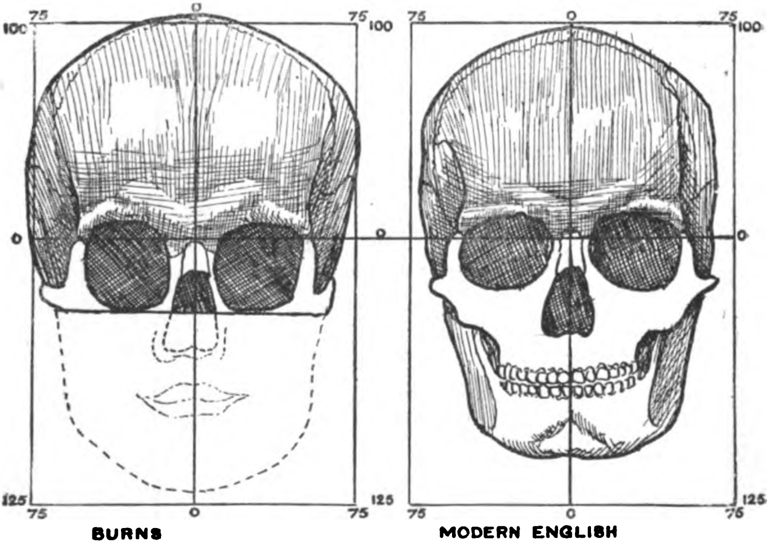


FIG. 11.—Anterior view of the cranial cast of Robert Burns' compared with the same aspect of a skull of moderate dimensions.

Now, the type of skull found by Professor Bryce, in the Cairns

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* See Proc. Soc. Antiq. of Scotland, 1902, p. 79.

of Arran, is just that represented by the skull of Robert Burns. The same type is found in the long barrows of England. Of the six skulls, one was 210 millimetres long, another 201. In Fig. 13, one of these skulls—one of the most complete—from the Cairn at Clachaig is represented. The outline of Burns' skull, drawn to the same scale, is indicated by a stippled line. There can be no doubt as to the identity of types. The poet's skull is fuller and wider than the neolithic ones. In the neolithic period—4000 years or more before our time—the race to which Burns belonged lived in the country round the Firth of Clyde. We find that the ancestor of the poet who settled in Kincardine and took the name of Burness was originally Walter Campbell, of Argyll. His mother, Agnes Brown, was an Ayrshire woman. Burns, then is a direct descendant of the long-headed people who lived in England and Scotland during the neolithic period—at least, during the later part of that period. Where those long-headed people were originally evolved, where they came from, we do not know. They were certainly of the same stock as that race which goes by the name of Iberian. When they reached Britain, they found that country already populated by a long-headed and closely allied race. Thus we have in Shakespeare and Burns—in our two national poets—representatives of two of the most divergent of European stocks. If we use the term "Celt" in the same sense as it is employed on the Continent, then we must call Shakes-

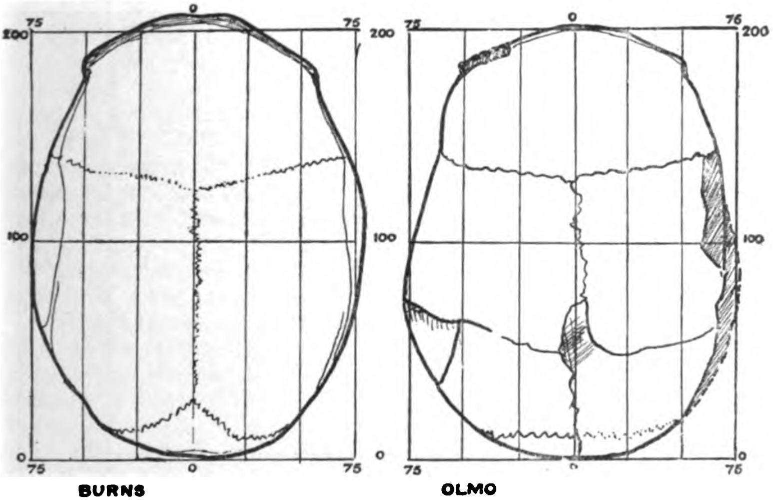

Fig. 12.—View of the cast of Burns' skull seen from above and contrasted with a similar view of the Olmo skull.

peare a Celt, a descendant of the round-headed people who invaded England in the Bronze Age. Burns is from the western fringe—usually called "Celtic," but which in truth is pre-Celtic. He comes of the long-headed stock which inhabited our country before the Bronze Age—probably before the neolithic age dawned. Is it possible that we may explain the extraordinary difference in the working of their brains by the diversity of their racial origin?

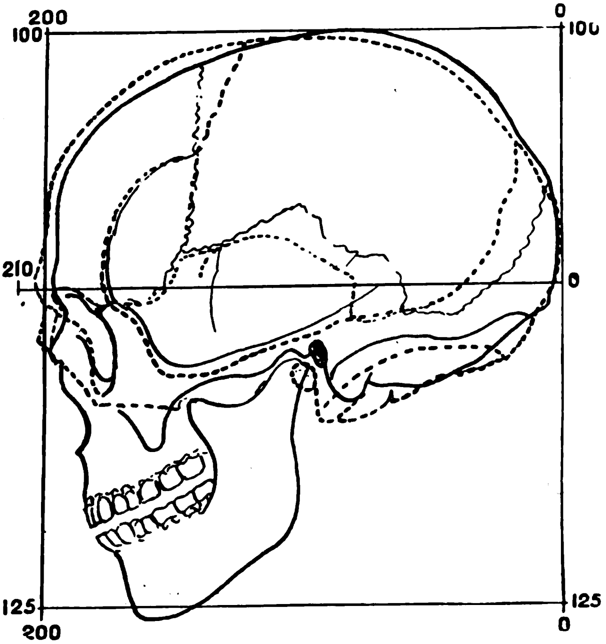

Fig. 13.

Of the Raeburn portrait it is not for me to speak. That the portrait is that of Burns, I have no doubt. The features of the cranial cast are manifest in the portrait. Those who have seen the Raeburn portrait have seen Burns.

[A. K.]

WEEKLY EVENING MEETING,

Friday, February 27, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR W. A. BONE, D.Sc. Ph.D. F.R.S.

Surface Combustion.

DURING his researches upon Flame,* Sir Humphry Davy discovered, in 1817, that the constituents of a combustible mixture will combine slowly below the ignition temperature; this led him to enquire whether, seeing that the temperatures of flames far exceed those at which solids become incandescent, a metallic wire can be maintained at incandescence by the combination of gases at its surface, without actual flame. He thereupon tried the effect of introducing a warm platinum wire into a jar containing a mixture of coal-gas and air rendered non-explosive by an excess of the combustible constituents; the wire immediately became red hot, and continued so until nearly the whole of the oxygen had disappeared.

We will repeat the experiment, in a modified form, with the aid of a platinum crucible and a Bunsen burner. If the platinum crucible be warmed by the flame, and the flow of the explosive mixture through the burner be momentarily arrested, and then immediately renewed, the gases, instead of burning with flame at the burner head, will immediately combine without flame over the platinum surface, causing it to glow.

During the twenty years which followed Davy's discovery, several distinguished chemists (William Henry and Thomas Graham in this country, but more particularly Dulong and Thénard, and independently Döbereiner in France) experimented upon the slow combination of gases at temperatures below the ignition point, in contact with hot solids, whereby it was established (1) that hot solids, and preeminently metals of the platinum group, have the power of inducing gaseous combustion at relatively low temperatures; and (2) that hydrogen is of all combustible gases, the most susceptible to this action.

The mechanism of this induced slow surface combustion formed the subject of a celebrated controversy between Faraday and De la Rive in 1834–5. De la Rive held the view that it consists essentially in a series of rapidly alternating oxidations and reductions of the surface; Faraday, on the other hand, contended that the function of the surface is to condense both the oxygen and the combustible gas, thus producing in the surface layers a condition comparable to that of high pressure. But, owing to lack of crucial experiments, no satisfactory theory of the phenomenon could be evolved, nor, with the exception of the famous "Döbereiner Lamp," was there any practical outcome of this early work. In 1836 interest in the subject suddenly dropped, and was not revived for half a century.

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* Collected Works, vol. vi. p. 8.

Meanwhile, the researches of Deville upon the dissociation of steam and carbon-dioxide at high temperatures led to the notion, which was strongly upheld by the late Frederick Siemens, that inasmuch as incandescent surfaces promote dissociation, they must necessarily hinder combustion. This, of course, is fallacious; we now recognize that if, as Deville proved, an incandescent surface accelerates the dissociation of steam, it must, according to a principle enunciated by Ostwald, of necessity accelerate the combination of oxygen and hydrogen in like degree, provided always that the surface remains chemically unaltered.

A notable demonstration of the possibility of realizing flameless incandescent surface combustion in contact with metals other than those of the platinum group was given by Thomas Fletcher in a lecture at the Manchester Technical School as far back as 1887.* He injected a mixture of gas and air on to a large ball of iron wire, flame being used at first in order to heat the wire to the temperature necessary to induce a continuous surface combustion; on extinguishing the flame, by momentarily stopping the gaseous mixture, the combustion continued without any flame, but with an enormous increase of temperature. Fletcher grasped three important points, namely: (1) that "this invisible flameless combustion is only possible under certain conditions," (2) "that the combustible mixture shall come into absolute contact with a substance at high temperature. . . ," and (3) that "in the absence of a solid substance at a high temperature, it is impossible to cause combustion without flame"; but, so far as I am aware, he did not follow up the matter beyond this point, either in its theoretical aspects or practical applications, and his work had but little influence upon contemporary opinion or practice.

My own investigations upon surface combustion began in 1902 with a systematic attempt to elucidate the factors operative in the slow combination of hydrogen and of carbon monoxide in contact with various hot surfaces (e.g. porcelain, fire-clay, magnesia, platinum, gold, silver, copper and nickel oxides, etc.) at temperatures below 500°.* Into the details of these earlier experiments, which preceded and led up to the technical developments about which I shall speak later, I do not propose to enter; it will be sufficient for my present purpose if I say that it was proved beyond all question

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* Journal of Gas Lighting, 1887, i. p. 168.

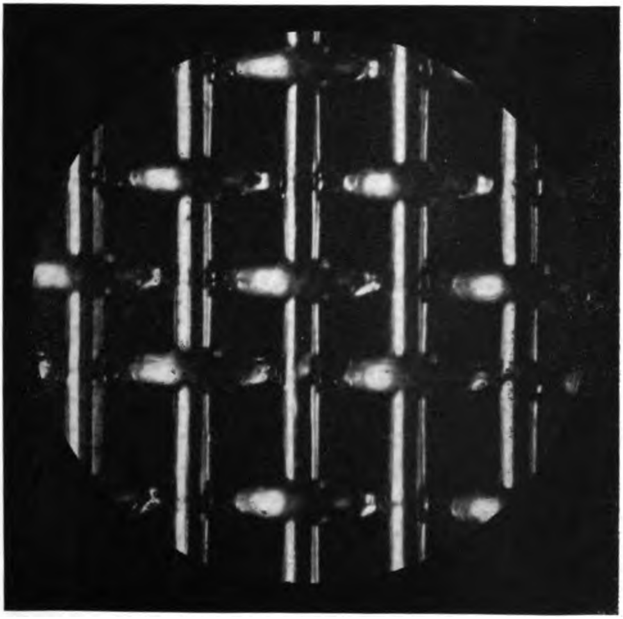

Fig. 1.

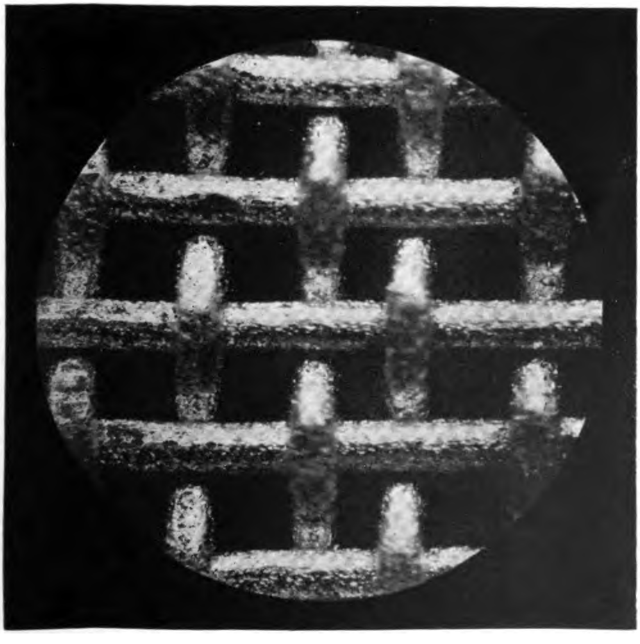

Fig. 2.

(1) that the power of accelerating gaseous combustion is possessed by *all* surfaces at temperatures below the ignition point in varying degrees, dependent upon their chemical characters and physical texture; (2) that such an accelerated surface combustion is dependent upon an absorption of the combustible gas, and probably also of the oxygen, by the surface, whereby it becomes "activated" (probably ionized) by association with the surface; and (3) that the surface itself becomes electrically charged during the process. Finally, certain important differences between homogeneous combustion in ordinary flames and heterogeneous combustion in contact with a hot surface from a chemical point of view were established, so that there can be no longer any doubt as to the reality of the phenomenon.*

The fact that such catalytic combustion depends upon an intimate association of the surface with the combining gases is beautifully illustrated by two photomicrographs of the surface of a silver gauze taken *before* and *after* a long series of experiments in which it was employed as the catalysing medium for the combination of hydrogen and oxygen at 400° C. (848° F.). Fig. 1 shows the original smooth condition of the surface, and Fig. 2 shows the "frosted" effect produced by the deep "pitting" action of the reacting gases during their absorption and "activation" by the surface.

My next contention is that if hot surfaces possess the power of accelerating gaseous combustion at temperatures below, or in the neighbourhood of, the ignition point, the same power must also be manifested in even a greater degree at higher temperatures, and especially so when the surface itself becomes incandescent. Indeed, there are experimental grounds for the belief that not only does the accelerating influence of the surface rapidly increase with the temperature, but also that the differences between the catalysing powers of various surfaces, which at low temperatures are often considerable, diminish with ascending temperatures until at bright incandescence they practically disappear.

Such considerations as I have thus briefly explained convinced me some years ago that if an explosive gaseous mixture be either injected on to or forced through the interstices of a porous refractory incandescent solid under certain conditions, which will be hereafter explained, a greatly accelerated combustion would take place within the interstices or pores, or, in other words, within the boundary layers between the gaseous and solid phases wherever these may be in contact—and the heat developed by this intensified combustion would maintain the surface in a state of incandescence *without any development of flame*, thus realizing the conception of *flameless incandescent*

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* Bone and Wheeler, *Phil. Trans. Roy. Soc.*, 1906 (A. 206, pp. 1–67), also further (unpublished) results (1905–12) in collaboration with Messrs. G. W. Andrew, A. Forshaw, and H. Hartley, which are summarized in *Berichte der Deutschen Chem. Ges.*, 1913.

surface combustion, as a means of greatly increasing the general efficiency of heating operations wherever it can be conveniently applied.

There are critics who, whilst admitting the accelerating influence of an incandescent surface upon gaseous combustion, are sceptical about the process being really flameless. The force of such objections largely disappear when we get into close quarters with the phenomenon, and realize how extremely slow a transaction flame combustion really is when considered in terms of molecular time. Take, for example, the case of such a quick-burning mixture as electrolytic gas ($2H_2 + O_2$). When this is ignited at atmospheric pressure, the flame is initially propagated by conduction with a uniform slow velocity of 20 metres per second, and during this initial period of "*inflammation*," the total duration of chemical change in each successive layer is something like the order of $\frac{1}{50}$ second, an interval of at least one hundred million times as long as the average interval between successive molecular collisions in the gas. Even after "*detonation*" has been set up in the mixture, when the combustion is propagated from layer to layer as a wave of adiabatic compression, at a velocity of 2820 metres per second, the total duration of chemical change is still of the order of $\frac{1}{5000}$ or $\frac{1}{10000}$ second, or about a million times as long as the interval between successive molecular collisions.

To illustrate how a flame may be extinguished by the introduction of an active catalysing surface, I will perform an experiment which was first exhibited here a year ago by my colleague Professor the Hon. R. J. Strutt, who has very kindly lent me his apparatus. Most of you will be familiar with his discovery of the active modification of nitrogen produced by a powerful electric discharge. This bulb (capacity 300 c.c.) is full of rarefied nitrogen, at a pressure of about $\frac{1}{10}$ mm.; and in the side tube lies an oxidized copper wire, which fits it as closely as is consistent with easy sliding.

If the nitrogen in the bulb be subjected to a powerful electrodeless discharge, it is transformed into an active condition; the gas continues to glow brilliantly, owing to the fact that the "active" modification is reverting to the ordinary kind producing a luminosity which may be regarded as a condition analogous with flame. The glow will last for a minute or more during the progress of the chemical reversion, and finally dies out when the latter is completed.

But if, on repeating the experiment, the bulb be tilted so that the oxidized copper wire drops into the glowing gas, the luminosity is instantly extinguished. This shows that the reversion process is so enormously accelerated by the surface that practically the whole of the chemical action is concentrated at the surface, and instantaneously completed there, thus extinguishing the glow.

I think we have, in Professor Strutt's experiment, a close analogy to what I conceive as occurring when an incandescent surface is employed to accelerate ordinary gaseous combustion ; the action is so concentrated at the surface that a substantially "flameless" effect results. I want to emphasize the fact that the incandescent solid plays a specific rôle in this surface combustion ; it is no mere idle looker-on at the surging crowd of reacting molecules which swarm around it. On the contrary, it so galvanizes and incites the dormant affinities between the combustible gas and oxygen, that the stately minuet of ordinary combustion gives place to the wild intoxication of the Venusberg.

The manner in which the surface acts is still perhaps a matter of conjecture, but the fact that it so acts can no longer be disputed. In a discussion which took place at the British Association in 1910, Sir J. J. Thomson insisted that combustion is concerned not only with atoms and molecules, but also with electrons, *i.e.* bodies of much smaller dimensions and moving with very high velocities, and suggested that in reference to the influence of hot surfaces in promoting combustion it was not improbable that the emission of charged particles from the surface was a factor of primary importance. It is known that incandescent surfaces emit enormous streams of electrons travelling with high velocities, and the actions of these surfaces in promoting combustion may ultimately be found to depend on the fact that they bring about the formation of layers of electrified gas in which chemical changes proceed with extraordinarily high velocity.

The New Processes of Incandescent Surface Combustion.

Leaving the theoretical aspects of the subject, I will now describe some of the more important features of two processes of incandescent surface combustion evolved at the works of Messrs. Wilsons and Mathiesons, Ltd., in Leeds under my direction, with the assistance of Mr. C. D. McCourt, in which a homogeneous explosive mixture of gas and air, in the proper proportions for complete combustion (or with air in slight excess thereof), is caused to burn without flame in contact with a granular incandescent solid, whereby a large proportion of the potential energy of the gas is immediately converted into radiant form. The advantages claimed for the new system, now known as the "Bonecourt" system, are : (1) the combustion is greatly accelerated by the incandescent surface, and, if so desired, may be concentrated just where the heat is required ; (2) the combustion is perfect with a minimum excess of air ; (3) the attainment of very high temperatures is possible without the aid of elaborate regenerative devices ; and (4) owing to the large amount of radiant energy developed, transmission of heat from the seat of combustion to the object to be heated is very rapid. These advantages are so uniquely combined in the new system that the resultant heating effect is, for many important purposes not only pre-eminently economical, but also easy of control.

Diaphragm Heating and its Applications.

In the first process the homogeneous mixture of gas and air is allowed to flow under slight pressure through a porous diaphragm of refractory material from a suitable feeding chamber (see Fig. 3), and is caused to burn without flame at the surface of exit, which is thereby maintained in a state of red-hot incandescence. The diaphragm is composed of granules of firebrick, or other material, bound together into a coherent block by suitable means; the porosity of the diaphragm is graded to suit the particular kind of gas for which it is to be used. The diaphragm is mounted in a suitable casing, the space enclosed between the back of the casing

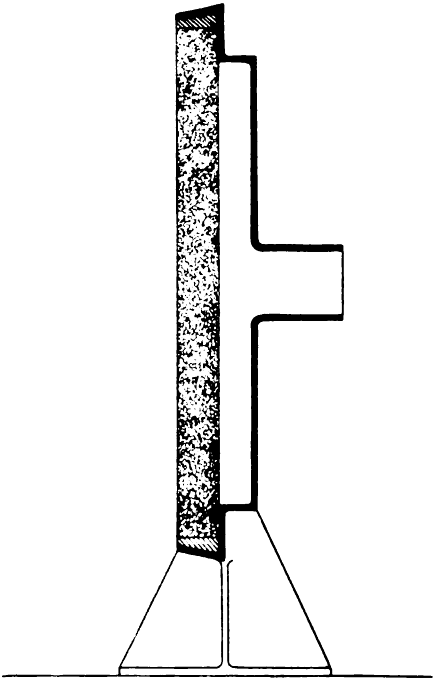

Fig. 3.—Diaphragm, Sectional View.

and the diaphragm constituting a convenient feeding-chamber for the gaseous mixture. Such a mixture may be obtained in either of two ways, namely (1) by means of suitable connections through a Y-piece with separate supplies of low pressure gas and air (2 or 3 inches W.G. is sufficient), or (2) by means of an "injector" arrangement connected with a supply of gas at a pressure of 1 to 2 lb. per sq. inch ; the gas in this case draws in its own air from the atmosphere in sufficient quantity for complete combustion, the proportions of gas and air being easily regulated by a simple device.

We will now start up a diaphragm. Gas is first of all turned on and ignited as it issues at the surface ; air is then gradually added until a fully aerated mixture is obtained. The flame soon becomes non-luminous, and diminishes in size ; a moment later, it retreats on to the surface of the diaphragm, which at once assumes a bluish appearance ; soon, however, the granules at the surface attain an incipient red heat, producing a curious mottled effect ; finally, the whole of the surface layer of granules becomes red-hot, and an accelerated "surface combustion" comes into play. All signs of flame disappear, and there remains an intensely glowing surface throwing out a genial radiant heat which can be steadily maintained for as long as required.

Whilst the diaphragm is in operation before you, I may point out some of the more striking features of the phenomenon which it presents. *Firstly*, the actual combustion is confined within a very thin layer—$\frac{1}{8}$ to $\frac{1}{4}$ inch only—immediately below the surface, and no heat is developed in any other part of the apparatus. Kindly observe that whilst the front of the diaphragm is intensely hot, the back of the apparatus is so cold that I can lay my hand on it. *Secondly*, the combustion of the gas, although confined within such narrow limits, is perfect, for when once the relative proportions of gas and air have been properly adjusted, no trace of unburnt gas escapes from the surface. *Thirdly*, the temperature at the surface of the diaphragm can be instantly varied at will by merely altering the rate of feeding of the gaseous mixture ; there is practically no lag in the temperature response, a circumstance of great importance in operations where a fine regulation of heat is required. *Fourthly*, a plane diaphragm such as this may be used in any position, i.e. at any desired angle between the horizontal and vertical planes. *Fifthly*, the diaphragm method is amenable to a variety of combustible gases —coal or coke oven gas (either undiluted or admixed with water gas), natural gas, petrol-air gas, carburreted water gas are all well suited in cases where unimpeded radiation is required. *Finally*, the incandescence in no way depends upon the external atmosphere. When once the diaphragm has become incandescent, and the proportions of air and gas supplied in the mixing chamber at the back have been properly adjusted, the surface will maintain its incandescence unimpaired, even in an atmosphere of carbon dioxide.

I need hardly point out to you the many obvious purposes, domestic and industrial, to which "diaphragm heating" may be applied. In the domestic line, the boiling of water, grilling, roasting and toasting are at once suggested, and although the best existing types of gas fires are thoroughly hygienic and efficient, I think that the diaphragm may come in for the heating of apartments; at any rate experiments are being carried out in that direction.

I have recently been told of a large restaurant in London where they are grilling by means of an electric radiator fixed horizontally above the work so that the radiant energy is directed downwards on to it. It has been claimed that the value of the recovered fat, which in the ordinary method of grilling drops into the fire and is consumed, has more than paid for the electricity used. With a diaphragm, similar advantages can be secured with gas at a considerably less cost; for comparing gas of net calorific value 500 B.Th.Us. per cub. ft. at 2*s*. 6*d*. per 1000 cub. ft. with electricity at 3*d*. per unit, the initial cost of a given amount of energy is about 14 times greater in the form of electricity than in the form of gas.

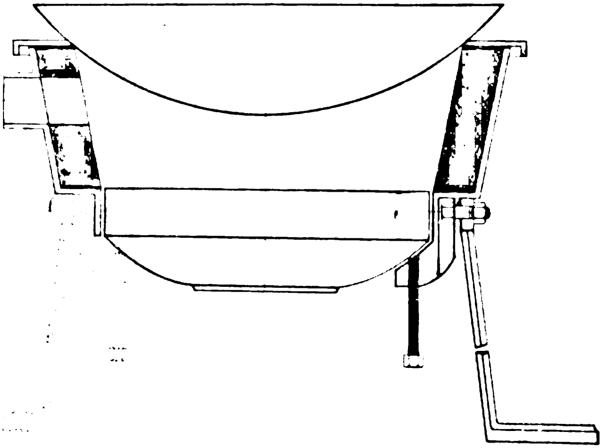

Fig. 4.—Diaphragm for Boiling Sugar Solutions, Etc.

Turning now to the industrial field, I had the satisfaction of visiting not long ago a large confectionery factory where the diaphragms have been employed for more than a year for the boiling and concentrating sugar solutions. The solution is boiled in a copper pan over a 13-in. diameter circular diaphragm (see Fig. 4); the supply of gaseous mixture is controlled by one lever which operates in a single movement the gas and air cocks. The ignition is effected automatically by means of a small pilot light. Each boiler does 10 to 12 heats per diem, each lasting twenty minutes, and some of the diaphragms have been in continuous daily use for about a year. I saw in the factory a battery of thirteen such boiling pans, all in constant use, and was informed that the gas consumption was not more than half what it formerly used to be when the pans were heated over ordinary atmospheric flame burners. The secret of this higher efficiency lies in the fact that gas flames are a very unsatisfactory means for boiling liquids in metallic vessels, owing to the non-conducting layer of relatively cool gas which forms between the flame and the undersurface of the pan; but with a diaphragm the large percentage of radiant heat is quickly absorbed by the vessel and transferred to the liquid with high efficiency.

Moreover, in evaporating liquids it is possible to use an overhead diaphragm, directing the radiation downward on to the surface of the liquid, thus eliminating altogether the influence of the containing vessel. In this way only the upper layers of the liquid are actually heated up to the working temperature, whilst the evaporation is extraordinarily rapid and efficient. Given a sufficiently cheap high-grade gas there would appear to be endless possibilities for diaphragm heating in the industrial field.

Incandescent Surface Combustion in a Bed of Refractory Granular Material.

The second process is applicable to all kinds of gaseous or vaporized fuels; it consists essentially in injecting, through a suitable orifice at a speed greater than the velocity of back-firing, an explosive mixture of gas (or vapour) and air in their combining proportions into a bed of incandescent granular refractory material which is disposed around or in proximity to the body to be heated.

I can perhaps best describe the process by the aid of two diagrams showing its applications to the heating of crucible and muffle furnaces. Fig. 5 shows the process as applied to a crucible furnace. The crucible is surrounded by a bed of refractory incandescent granular material. The mixture of gas and air is injected at a high velocity through a narrow orifice in the base of the furnace, and as it impinges upon the incandescent bed, combustion is instantaneously completed without flame.

The seat of this active surface combustion is in the lowest part of the bed; the burnt gases, rising through the upper layers, rapidly impart their heat to the bed, maintaining it in a high degree of incandescence. Fig. 6 shows a similar arrangement for the heating of a muffle furnace which needs no further explanation.

It is obvious that this process is capable of adaptation to all kinds of furnace operations, as, for example, to the heating of crucibles, muffles, retorts, and to annealing and forging furnaces generally. Moreover, it is not essential that the bed of refractory material should be very deep; indeed a quite shallow bed suffices to complete the combustion. Neither is it necessary that the bed shall be disposed *around* the vessel or chamber to be heated; for if contact with the burnt products is not objectionable, a shallow bed may be arranged *within* the heating chamber itself (Fig. 7); or the refractory material may be equally well packed into tubes, or the like, traversing the substance or medium to be heated. The last-named modification is, as we shall see later, specially important in relation to steam-raising in multitubular boilers.

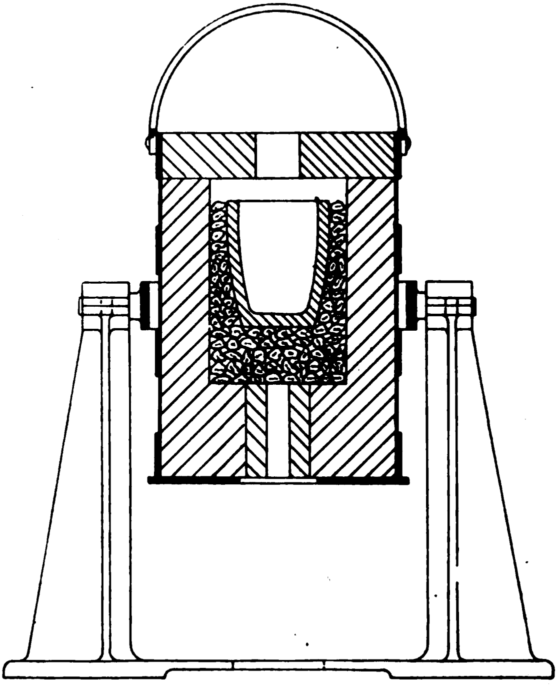

Fig. 5.

By means of this process much higher temperatures are attainable with a given gas than by the ordinary methods of flame combustion without a regenerative system, and, as a matter of fact, we have found that with any gas of high calorific intensity (such as coal gas, water gas, or natural gas) the upper practicable temperature limit is determined rather by the refractoriness of the material composing the chamber to be heated (i.e. the muffle or crucible) than by the possibilities of the actual combustion itself. When I tell you that in a crucible fired by coal gas on this system we have melted Seger-cone No. 39, which according to the latest determination of the German Reichsanstahlt melts at 1880° C. (3416° F.), and also that we can easily melt platinum, you will appreciate the possibilities of the method in regard to high temperatures with gas-fired furnaces.

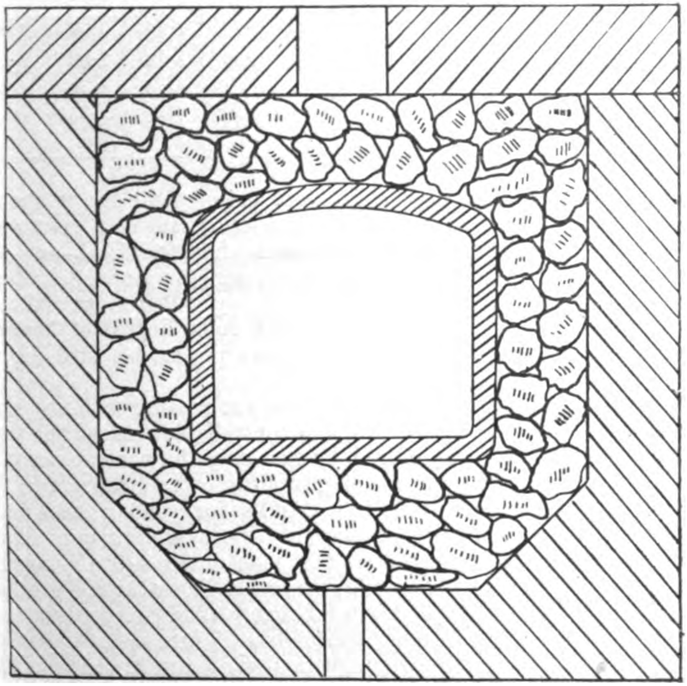

Fig. 6.

The maximum temperature obtainable, without "regenerative" appliances, with any particular gas will obviously depend upon the relative heat capacities of their products of combustion for a given heat development in the bed. In this connection, it is of interest to compare the principal gaseous fuels available for industrial operations in the order of their calorific intensities, as follows :—

THE COMBUSTION OF TYPICAL GASEOUS FUELS.

—	Per Cubic Foot of Gas Burnt			Per 100 B.Th.U's. *net* developed on Combustion			
	Net B.Th.U's	Vol. of Air Required	Vol. of Products	Vol. of Air Required	Vol. of Combining Mixtures	Vol. of Products	Relative Heat Capacities of Products
Blue Water Gas... ...	290	2·290	2·82	0·79	1·14	0·97	0·95
Coal or Coke Oven Gas	500	4·656	5·36	0·93	1·13	1·08	1·00
Producer Gas	140	1·166	2·00	0·84	1·50	1·43	1·38
Blast Furnace Gas ...	100	0·716	1·58	0·72	1·72	1·58	1·52

Now, whilst with coal gas, coke oven gas or water gas, it should be easily possible, without regeneration, to obtain in a refractory granular bed temperatures of up to at least 2000° C. (or say 3630° F.), with a low-grade producer gas, such as Mond gas, about 1500° C. (or say 2730° F.), would probably be the approximate maximum without regeneration. But with some degree of heat recuperation, which in large furnaces is quite practicable, still higher temperatures would be attainable.

Moreover, within the maximum limits, the heating with any kind of gas is very uniform, economical, and controllable; indeed the temperature for a given furnace fired by gas of uniform composition is chiefly controlled by the amount of gas burnt, or in other words, the rate of heat development in the bed. And as for economy, tests made about three years ago in Leeds under my supervision upon medium-sized muffle furnaces convinced me that to maintain a given temperature between say 800 and 1400° C., a properly constructed "surface combustion furnace" would require between 70 and 40 per cent only of the gas used in an ordinary flame-heated furnace of the same dimensions, and that the advantage in favour of "surface combustion" would increase rapidly with the working temperature.

This was confirmed by independent competitive trials carried out in New York shortly after my lectures there in the autumn of 1911, in which our surface combustion furnaces were pitted against the best American types; it was proved that in order to maintain a temperature of 1400° C. we practically halved their gas consumption, whilst to maintain 800° C. our consumption was about 0·7 of theirs.

Anyone who understands furnace design and construction will appreciate the many difficulties which are necessarily encountered in applying a new and revolutionary idea of this sort to the firing of

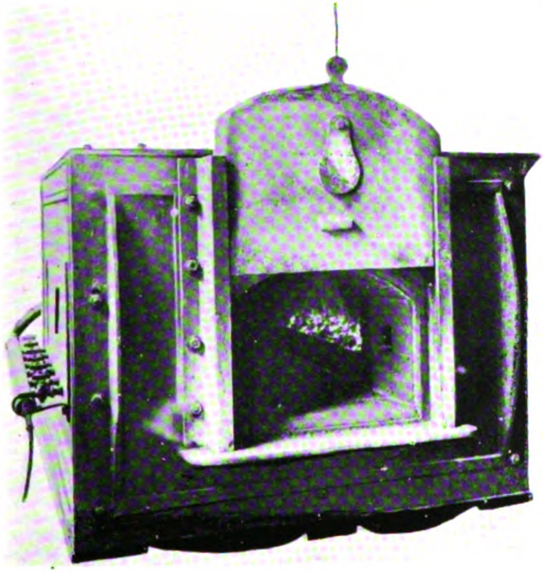

Fig. 7.

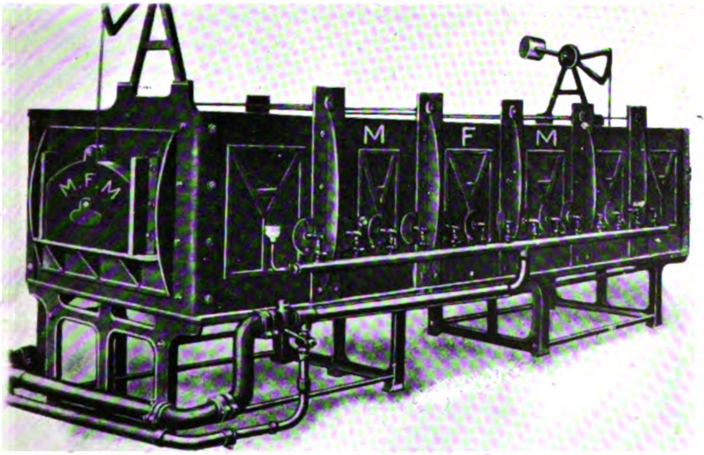

Fig. 8.

large industrial furnaces. Indeed, the new conditions of heating are so radically different from the old, that a new experience has to be built up. There are problems connected with the proper feeding and distribution of gaseous mixture, with the dimensions and arrangement of the combustion beds, and with the refractiveness of the furnace walls, which have to be solved. Nevertheless, judging from reports that reach me from time to time, substantial progress has been made in the direction of gas-fired crucible furnaces for brass and aluminium melting, annealing furnaces, and forging furnaces, the latter fired with producer gas with regeneration of gas and air. Some large annealing furnaces (for example, Fig. 8) are now running satisfactorily in industrial establishments on both Mond gas and town gas, and I understand that brass melting can be carried out in 50 to 60 lb. charges in tilting crucible furnaces with the remarkably low consumption of about 1·75 cub. ft. per pound of metal melted and poured.

Surface Combustion as Applied to Steam Raising.

I now come to an important application of the new process to the raising of steam in multitubular boilers; not that the application of surface combustion is limited to boilers of the multitubular type, but because our investigations have so far been principally made with these.

It is well known that the gas-firing of steam boilers has not been very successful from the point of view either of thermal efficiency or of rate of evaporation. In this country the gases available for steam-raising purposes on a large scale are principally (1) blast furnace gas, of which there are large surpluses available in all iron-smelting areas; (2) the surplus gas obtainable during the manufacture of coke in by-product ovens; and (3) producer gas of various compositions, but more particularly that manufactured under ammonia recovery conditions. In the United States, and some other oil-producing countries, natural gas of high calorific power is also available. All these gases have been found to be amenable to the system I am about to describe.

It has been estimated by a prominent British blast furnace engineer that the thermal efficiency of the best type of water-tube boiler, fired by blast furnace gas, does not exceed about 55 per cent., whilst in the case of boilers fired by coke oven gas the average thermal efficiency probably does not much exceed 65 to 70 per cent. But on applying the principle of surface combustion to the gas-firing of multitubular boilers we have been able to obtain results with coal gas corresponding to the transmission of nearly 95 per cent of the *net* calorific value of the gas to the water in the boiler.

Our first experiments in Leeds were made with a single steel tube

3 feet in length and 3 inches in diameter, packed with fragments of granular refractory material, meshed to a proper size, and fitted at one end with a fire-clay plug, through which was bored a circular hole, ¾ inch in diameter, for the admission of the explosive mixture of gas and air at a speed greater than that of back firing (Fig. 9). The tube was fitted into an open trough, such as you see here on the table, in which water could be evaporated at atmospheric pressure. After the lecture my assistants will demonstrate the boiling of water in this apparatus.

Such a tube may be appropriately termed the fundamental unit of our boiler system, because boilers of almost any size may be constructed merely by multiplying the single tube, and as each tube is, so to speak, an independent fire or unit, the efficiency of the whole is that of the single tube, or in other words, the efficiency of the whole boiler is independent of the number of tubes fired.

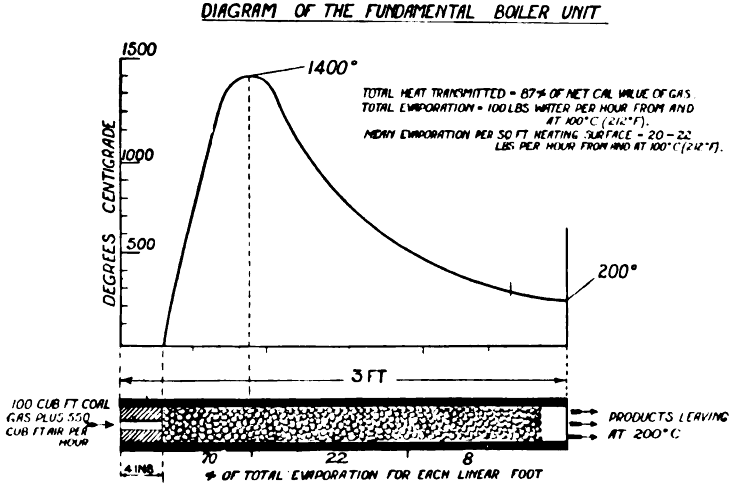
DIAGRAM OF THE FUNDAMENTAL BOILER UNIT
1500
1400°
TOTAL HEAT TRANSMITTED = 87% OF NET CAL VALUE OF GAS.
TOTAL EVAPORATION = 100 LBS WATER PER HOUR FROM AND AT 100°C (212°F).
MEAN EVAPORATION PER SQ FT HEATING SURFACE = 20 – 22 LBS PER HOUR FROM AND AT 100°C (212°F).
1000
DEGREES CENTIGRADE
500
200°
3 FT
100 CUB FT COAL GAS PLUS 550 CUB FT AIR PER HOUR
PRODUCTS LEAVING AT 200°C
4 INS
70
22
8
% OF TOTAL EVAPORATION FOR EACH LINEAR FOOT

Fig. 9.—Diagram of the Fundamental Unit.

Experimenting with such a tube, it was found possible to burn completely a mixture of 100 cubic feet of coal gas plus 550 cubic feet of air per hour, and to evaporate about 100 lb. of water from and at 100° C. (212° F.) per hour (20 to 22 lb. per square foot of heating surface), the products leaving the further end of the tube at practically 200° C. This meant the transmission to the water of 88 per cent. of the net heat developed by the combustion, and an evaporation per square foot of heating surface nearly twice that of an express locomotive boiler. The combustion of the gas was completed within 4 or 5 inches of the point where it entered the tube. Of the total evaporation, no less than 70 per cent occurred over the first linear foot of the tube, 22 per cent over the second foot, and only 8 per cent. over the last foot This points to a very effective "radiation" transmission from the incandescent granular material in the first third of the tube, where the zone of active combustion is located, although it should be remarked that the *loci* of actual contact between the incandescent material and the walls of the tube are so rapidly cooled by the transmission of heat to the water on the other side that they never attain a temperature even approaching red heat. The granular material in the remaining two-thirds of the tube serves to baffle the hot products of combustion, and to make them repeatedly impinge with high velocity against the walls of the tube, thus materially accelerating their cooling, and either preventing or minimising the formation of the feebly-conducting stationary film of relatively cold gases which in ordinary boiler practice clings to the tube walls, seriously impairing the heat transmission.

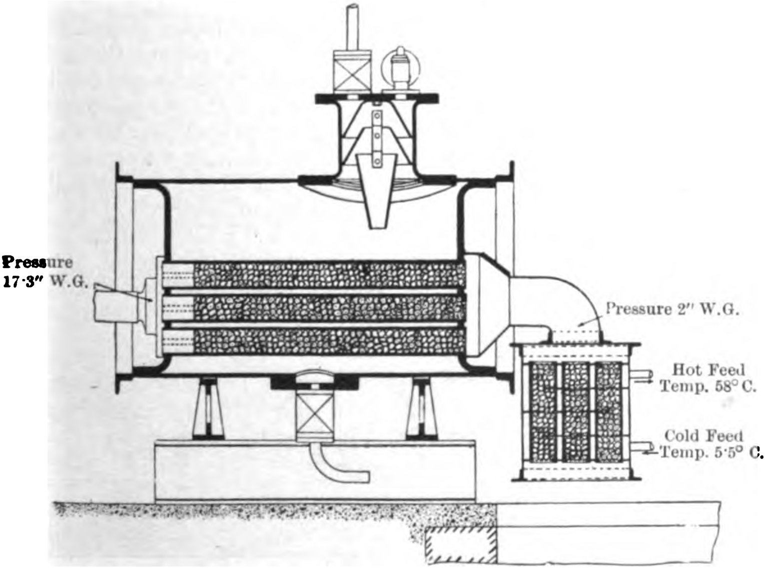

Fig. 10.

Having thus satisfied ourselves of the efficiency of the fundamental unit as an evaporator, we proceeded to construct our first experimental boiler, made up of ten tubes, each 3 ft. long and 3 in. in diameter, fixed horizontally in a cylindrical steel shell capable of withstanding a pressure of over 200 lb. per square inch. This boiler is shown diagrammatically in Fig. 10 (section showing three tubes only), whilst Fig. 11 is a photograph of it (back view showing feed water heater). The gaseous mixture was forced through the tubes under pressure from a special feeding chamber attached to the front plate of the boiler; the products of combustion, after leaving the boiler, passed through a small feed-water heater containing nine tubes, each 1 ft. long and 3 in. in diameter, filled with granular material to facilitate the exchange of heat.

This combination of boiler and feed-water heater proved phenomenally successful in every way; in a trial run at Leeds on December 8, 1910, with coal gas of net calorific value 562 B.Th.Us. per cubic foot at N.T.P., and evaporating at the rate of 21·6 lb. of water from and at 212° F. per sq. ft. of heating surface per hour, the ratio of the heat utilized to the net heat developed in the tubes was 0·943. But this remarkable result was eclipsed in another independent trial carried out by German Engineers in London on January 8, 1913, when with coal gas of net calorific value 510 B.Th.Us. per cubic foot at N.T.P., and evaporating at the enormously high rate of 33·9 lb. from and at 212° F. per square foot of heating surface, the ratio of the heat utilized to the net heat developed by combustion was 0·933. The details of these tests are shown below:—

——	I. Dec. 8, 1910	II. Jan. 8, 1913
1. Net cal. value of gas: B. Th. Us. per cub. ft at N. T. P.	562	510
2. Rate of evaporation: Lbs. of water per sq. ft. heating surface per hour (from and at 212° Fahr.)	21·6	33·9
3. Pressure of gaseous mixture entering the tubes: Inches W. G.	17·3	40·9
4. Pressure of products entering the feed water heater: Inches W. G.	2·0	5·14
5. Steam gauge pressure: Lbs. above atmospheric ...	100	103
6. Boiling point of the water: Fahr. Degrees	338°	340°
7. Temperature products leaving boiler: Fahr. Degrees	446°	534°
8. Temperature products leaving feed water heater: Fahr. Degrees	203°	289°
9. Net heat supplied to the boiler per hour: B. Th. Us.	559,800	728,333
10. Net heat transmitted per hour: B. Th. Us.	527,800	680,000
Efficiency Ratio	0·943	0·933

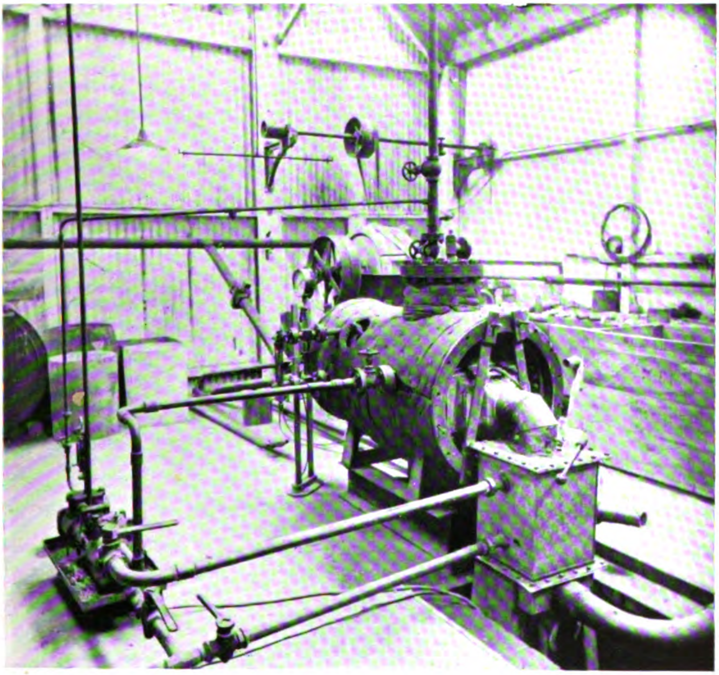

FIG. 11.

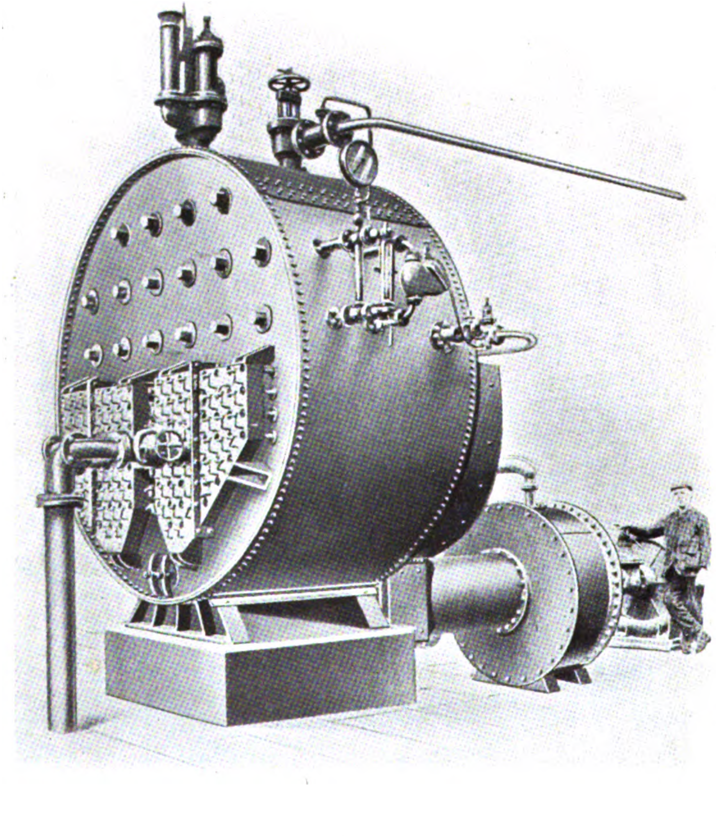

Fig. 12.

The 110-Tube Boiler at the Skinningrove Ironworks.

Six months' continuous experience with our first experimental unit gave us great confidence in its reliability, so that when in the early months of 1911 we received an inquiry from the Skinningrove Iron Co., Ltd., for a boiler of about ten times the capacity of the experimental unit, to be fired by means of the surplus gas from their new Otto by-product coking-plant, we had no hesitation in accepting a commission to instal our first large boiler there, under a strict guarantee as to its output and efficiency. This boiler (Fig. 12) was built by Messrs. Richardsons and Westgarth, Ltd., of Middlesbrough, to the design of Mr. Michael Longridge, to whom I had been indebted for much valuable advice and encouragement during the earlier experiments. It consists of a cylindrical drum 10 feet in diameter and 4 feet from front to back, traversed by 110 steel tubes, each of 3 inches internal diameter, which are packed with fragments of suitable refractory granular material. To the front of the boiler is attached a specially designed feeding chamber which delivers washed coke oven gas at 60° to 70° Fahr., and under a pressure of 1 to 2 inches W.G., to each of the 110 combustion tubes; this gas, together with a regulated proportion of air from the outside atmosphere, is drawn, under suction from a fan, through a short mixing tube, into each of the said combustion tubes, where it is burnt

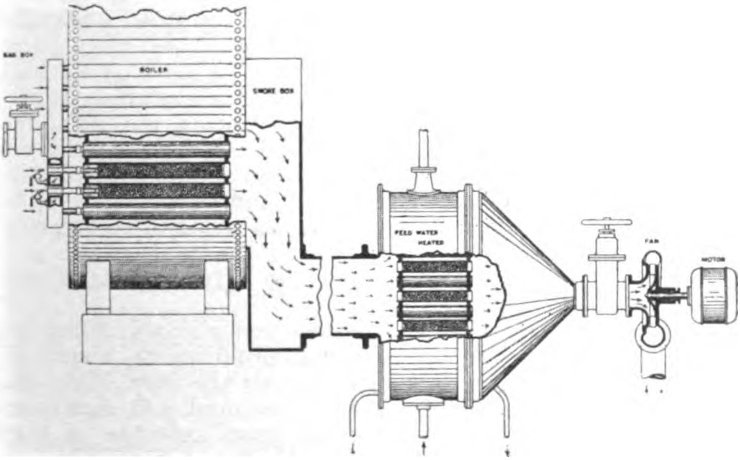

Fig. 13.

without flame in contact with the incandescent granular material. The products of combustion having traversed the 4-foot length of packed tube, pass outwards into a semicircular chamber at the back of the boiler, and thence through a duct to the tubular feed-waste heater. The fan which is attached just beyond this feed-water heater and is driven by a three phase A.C. electric motor, sucks out the cooled products, at a temperature of 100° C. (212° F.) or under, and discharges them, through a short vertical duct, into the atmosphere. The arrangement of the whole plant is shown in the accompanying diagram (Fig. 13).

The plant was successfully started up on November 7th, 1911, for a month's trial run—day and night continuously—after which it was opened up for an official inspection by the representative of a Boiler Insurance Company. Everything worked without a hitch during this trial; steam was generated at 100 lbs. gauge pressure, from a feed-water of about four degrees of hardness, whilst the average temperature of the waste gases leaving the feed-water heater was reduced to 80° C. (say 175° Fahr.), a sure indication of the high thermal efficiency of the plant. When, at the conclusion of the month's trial, the boiler was opened up for inspection, the combustion tubes were found to be in good condition and free from scale; indeed, owing to the extremely high rate of evaporation, the scaling troubles experienced with other types of multi-tubular boilers appear to be completely obviated, the scale being automatically and continuously shed from the tube in thin films (about $\frac{1}{30}$ in. thick) as fast as it is formed; a very important advantage, as anyone who is plagued by scaling troubles will appreciate.

After this official inspection, the plant, having amply fulfilled our guarantee, was taken over by the Skinningrove Iron Co., and it was at once re-started under the direction of Mr. E. Bury, M.Sc., the Manager of the Coke Oven Plant, who maintained it in continuous commission for three months until the great coal-strike of 1912 brought everything to a standstill.

At the end of July, 1912, after altogether five months actual running, exhaustive trials were made under the personal superintendence of an eminent American steam engineer who had crossed the Atlantic with a commission to investigate the boiler; he subsequently reported the following results of a ten-hours' steam trial on July 29th, which speak for themselves.

RESULTS OF STEAM TRIAL, JULY 29, 1912.

Duration of test in hours	10
Mean steam gas pressure, lbs. per sq. in. above atmospheric	97·2
Total gas burnt, cub. ft. at N.T.P.	101,853

Net cal. value of gas, B. Th. Us. per cub. ft. at N.T.P.	510·5*
Total net heat supplied, B. Th. Us.	52,003,996
Temperature of products leaving boiler, Fahr. Degrees	386° (196° C.)
Temperature of products leaving feed-water heater, Fahr. Degrees	202° (94·6° C.)
Total water evaporated, lbs. from and at 212° Fahr.	49,824
Evaporation in lbs. per sq. ft. of heating surface per hour	14·1
Per cent. Dryness of steam	99·3
Total heat utilized, B. Th. Us.	48,208,399

$$\text{Ratio} \quad \frac{\text{Heat utilized}}{\text{Net heat supplied}} = 0{\cdot}927 \dagger$$

$$\left.\begin{matrix}\text{Power taken by fan} \\ [\text{Suction} = 20 \text{ in. W.G.}]\end{matrix}\right\} = 6097 \text{ watts} = \text{(say) } 8{\cdot}2 \text{ H.P.}$$

The heat balance of the test is as follows, and for purposes of comparison I will give the heat balance for a trial of a marine boiler fired with a good steam coal of net calorific value 13,800 B.Th.Us. per lb. (volatile matter = 16·1 per cent).

——		Gas-Fired Surface Combustion Boiler	Coal-Fired Marine Boiler
		Per Cent.	Per Cent.
Heat utilized		92·7‡	75·1
Heat lost	in burnt gases	3·0	18·1
	in unburnt gases	nil	2·8
	by radiation, etc.	4·3	4·0
		100·0	100·0
Rate of Evaporation in lbs. per sq. ft. heating surface per hour...		14·1	4·3

The engineer who superintended these trials was very anxious to find out whether, in view of the intensity of the combustion and the high rate of heat transmission, the boiler tubes after five months continuous running day and night would show any signs of deterioration. Consequently, one of the boiler tubes, selected by him, was cut out and subjected to severe mechanical tests, similar tests being simultaneously made with a new and unused boiler tube of same dimensions. The results proved conclusively that the five months' service had not in any way impaired the mechanical properties of the boiler tubes, thus falsifying the confident predictions of several critics that the tubes would be speedily burnt through:—

---

* *An analysis of a typical sample of the gas gave :—*

$CO_2 = 2{\cdot}5$; $CO = 6{\cdot}5$; $C_nH_{2n} = 3{\cdot}2$; $CH_4 = 28{\cdot}0$;
$H_2 = 48{\cdot}0$; N_2, etc. $= 11{\cdot}3$ per cent.

† It should be stated that the boiler was *not lagged* during the trial, otherwise this figure would have been about 0·94.

‡ Deducting the steam equivalent of the power consumed in driving the fan on the Skinningrove boiler, the "*over-all efficiency*" of the plant is *practically 90 per cent.* To complete the comparison, the coal-fired boiler should be similarly debited with the power (whether human or mechanical) expended in stoking.

——	New Tube	Old Tube after 5 Months' Service	
		Front End of Boiler	Back end of Boiler
Ultimate tensile strength, tons per per sq. in.	25·3–27·1	27·9–29·6	29·7–31·6
Elastic limit	11·9–14·4	20·05	19·3–20·5
Per cent elongation on 8 in. length	23–24	19–21	18–23
Per cent area contraction	55·6–58·8	61·3–63·1	53·4–61·3

The new tube was slightly thinner than the old one, and therefore the better comparison would be between the results obtained for the specimens taken from the *front* and *back* ends of the old tube, because, whereas the specimen from the front end had surrounded the intensely heated "zone of combustion," that from the back end had never been subjected to a higher temperature than that of the water in the boiler (say 168° C. or 335° F.). The results of the tests show practically no difference between the mechanical strengths of the two specimens. Perhaps the best testimonial to the success of this initial installation of a large boiler on the new principle is the fact that the Skinningrove Iron Co. have put down a second unit for their new battery of coke ovens, which is now being started up, and I expect, within a few weeks at most, to see the two boilers working side by side.

Within the last few months the firm of Krupps have put down a boiler in connection with one of their coking plants in the Ruhr district of Westphalia, from the plans of the Skinningrove plant. This boiler has been running successfully since October last, and about three weeks ago underwent its official steam trials, which were carried out by the Bergbauliche Verein. Pending the official publication of the results in the German technical press, I am precluded from giving any details to-night, but an unofficial test on October 30th, 1913, showed a total evaporation of 6750 lb. of water "from and at 212° Fahr." per hour, with an efficiency ratio of

0·925; and I am informed that the results of the recent official tests have confirmed these satisfactory figures.

As this is the first occasion on which I have been able to refer in public to the successful inauguration of these inventions on a commercial scale in Germany, and in anticipation of the publication of the results of the official German trial, I wish, with your kind permission, to mention a matter which, although it may appear personal, is nevertheless bound up with the credit of British scientific invention.

This boiler, invented and developed as it was in Leeds, by a group of British chemists, all University trained men, aided by the advice of Mr. Michael Longridge and by the enterprise of my friends of the Skinningrove Iron Co., may truly be claimed as an "all-British" invention, if ever there was one. Nevertheless, it has recently been described in the German technical press as the "Bone Schnabel," or the "Schnabel Bone" boiler, instead of the "Bone-court" boiler, as it should be. It may be necessary at some future time, to deal more fully with this aspect of the matter, but to-night I will content myself with a protest against any attempt to claim or represent this boiler as, in part, a German invention.

I have perhaps said enough already about the boiler and its working to convince you that it combines high thermal efficiency and concentration of power, in a unique degree, and perhaps I may be permitted to summarize the other important advantages which may be claimed for it. *Firstly*, from the constructional point of view, nothing could be simpler or more compact than a cylindrical shell only 4 feet long by 10 feet in diameter, traversed by straight tubes, supported on a casting, and requiring neither elaborate brickwork setting nor expensive chimney flues and stack. *Secondly*, it has a further advantage over all multitubular boilers in that the front plate can never be heated beyond the temperature of the water, however much the firing may be forced, a circumstance which, coupled with the extremely short length of the tubes, implies an absence of strain and greatly reduces the risk of leaky joints. *Thirdly*, the high rate of mean evaporation obviates scaling troubles, and the very steep evaporation gradient along each tube causes a considerable natural circulation of water in the boiler, a factor of great importance from the point of view of good and efficient working; in this connection I may remind you that under normal working conditions we obtain a *mean evaporation* of 20 lbs. per square foot of heating surface per hour, and can, if need be, force this up to 35 lbs.; of this total evaporation, 70 per cent occurs over the first *third* length of the tube, 22 per cent over the *second* third, and only 8 per cent over the last third. *Fourthly*, inasmuch as each tube of the boiler is, so to speak, an independent combustion unit, capable of being shut off or lit up without affecting the others, and as it only takes five minutes after lighting up a cold tube to attain its maximum steam output, it is obvious that not only is such a boiler highly responsive to rapid variations in the load, but also it works with equal efficiency at both small and big loads ; indeed, within very wide limits, its efficiency is practically independent of the load.

I have endeavoured, within the hour, to trace the history of an idea from its inception in the laboratory through its experimental stages right up to its successful technical applications. Modern chemistry originated in the primeval instinct of "fire-worship," and even a century after Davy's work, the science of combustion is not yet a completed record. On the theoretical side, there are still obscure points concerning the mechanism of "surface combustion" for future investigation, and it will probably take many years to realize the wide range of industrial possibilities in gas-firing already opened up, not to speak of the equally important problem of "oil-firing." The number of applications of "surface combustion" which have been pressed upon our attention from without is positively embarrassing, and the task of fulfilling all expectations in the near future is indeed formidable. I hope, however, I have succeeded in convincing you that a sound foundation has been laid, upon which may be reared an enduring edifice.

[W. A. B.]

GENERAL MONTHLY MEETING,

Monday, March 2, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Sir James Key Caird, Bart., LL.D. F.Z.S.
Thomas William Dewar, Esq., M.D. M.B. C.M.
Frederick George Donnan, Esq., M.A. Ph.D. F.R.S.
Allan Hair, Esq., M.R.C.S. D.P.H.
Mrs. Eric Hambro.
Charles Edward Hearson, Esq.
John Gregory Jones, Esq.
John Edward Kingsbury, Esq., M.I.E.E.
Captain Charles Morley Knight.
Vincent Warren Low, Esq., F.R.C.S.
Mrs. Penrose-Thackwell.
William Jackson Pope, Esq., M.A. LL.D. F.R.S.

were elected Members of the Royal Institution.

The **PRESENTS** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Agricultural Journal of India, Vol. IX. No. 1. 8vo. 1914.
Agricultural Research Institute, Pusa: Bulletin, Nos. 35-37. 8vo. 1913.
Kodaikanal Observatory Bulletin, Nos. 34-35. 4to. 1913.
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  Amateur Photographer for February, 1914. 8vo.
  American Journal of Science for February, 1914. 8vo.
  Athenæum for February, 1914. 4to.
  Author for February, 1914. 8vo.
  Chemical News for February, 1914. 4to.
  Chemist and Druggist for February, 1914. 8vo.
  Concrete for February, 1914. 8vo.
  Dyer and Calico Printer for February, 1914. 4to.
  Electrical Engineering for February, 1914. 4to.
  Electrical Industries for February, 1914. 4to.
  Electrical Review for February, 1914. 4to.
  Electrical Times for February, 1914. 4to.
  Electricity for February, 1914. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 6, 1914.

J. H. Balfour Browne, Esq., K.C. D.L. J.P. LL.D., Manager and Vice-President, in the Chair.

The Rev. Canon J. O. Hannay, M.A.
("George A. Birmingham").

The Stage Irishman.

[Abstract.]

The reception of Bernard Shaw's "John Bull" in Dublin was for a while doubtful. The audience was inclined to demonstrate against Tim Haffigan, supposing him to be Shaw's presentation of an Irishman. Popular demonstrations, the expressions of the feelings of a democracy in literature and art, are as a rule, singularly foolish. Haffigan is in reality intended as a caricature of the stage Irishman. His language, drunkenness, and so forth, are what we expect from the stage Irishman. We Irish object strongly to the stage Irishman. He is a caricature of our countrymen. The English, Scottish and French, apparently do not object to being caricatured. The Irish, and perhaps the Americans, do. The reason of this.

In order that a caricature should have any vitality it must correspond to something real. Where are we to find the reality from which this caricature is drawn? Nothing like it is to be found in the ancient Irish literature. The Jacobite Irishman does not belong to the type. It is in the eighteenth century that we first find the prototype of the stage Irishman. It is in the eighteenth century also that he first appears in literature. The rapid development of the prototype of the stage Irishman in the early nineteenth century, and reasons for this.

Charles Lever is the literary godfather of the stage Irish type. Lever exaggerated existing realities. The influence of the gentry upon their dependents, and the development of the comic Irish peasant. The disappearance of this kind of Irishman after the famine. The new purposeful seriousness of Irish life in our time. The disappearance of fun in Ireland. The coming into being of a new kind of stage Irishman, quite different and hardly so pleasant as the old type that Shaw satirised.

[J. O. H.]

WEEKLY EVENING MEETING,

Friday, March 13, 1914.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Walter R. Lawrence, Bart., G.C.I.E.

An Indian State.

[Abstract.]

A former Lecture delivered at the Royal Institution dealt with Kashmir, its people, products, races, and religions, but did not touch on its constitution and political status.

The subject of the Indian States, rarely mentioned in England, and little known in India, save to the few whose work brings them into direct contact with the States.

Reticence on the subject; but as the States comprise two-fifths of the whole of India, it is right that the English public should know something about them. Their political importance.

Their peculiar character. No parallel exists elsewhere.

Variations; but tendency to conform to a single type.

Points in common. Devotion to the King-Emperor. Personal rule of the Chiefs; their control over legislation.

No external relations. Status of subordinate isolation. Protected from external dangers and against serious internal troubles. Secure and perpetual.

Influence of Lord Dufferin and Lord Curzon on the Indian States.

Conference of Chiefs: first step towards modification of policy of isolation.

Life in an Indian State. The Capital and the Palace. The Chief an Indian ruling Indians. The devotion of the people to the Chief. Factions and parties; the respect for ceremony and social order.

Life in the villages. Its quietude. Occasional appearance of Dacoits. Absence of crime. No police necessary in Kashmir.

British India: The splendid work of the British. Up to the last ten years one policy. The policy of the British District. The old system not appreciated by the educated Indians. Present generation has never known the troubled times their fathers knew.

Education has bred discontent. Impatient dislike of a strong

Bureaucratic Government. The people understood a strong government, but they do not understand a Bureaucratic Government tempered by popular institutions. They want one thing or the other.

The large number of low-paid Indian Officials a danger to our rule. Discontent of educated classes may spread to the remote millions. Little scope for the educated classes.

Long residence in Indian States has enabled lecturer to compare the two systems of Government. The British system, and the system of the Chiefs. People are happier and more contented in the Indian States than they are in British India. Reasons for this.

Belief in the great future of India, but that future is endangered by recent experiments. Safety may be found by adopting the system of the Indian States and applying it to British India.

Explanation of proposal.

[W. R. L.]

WEEKLY EVENING MEETING,

Friday, March 20, 1914.

Alexander Siemens, Esq., M.Inst.C.E. M.I.E.E., Secretary
and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, O.M. P.C. LL.D. D.Sc. F.R.S.
M.R.I., Honorary Professor of Natural Philosophy,
Royal Institution.

Fluid Motions.

The subject of this lecture has received the attention of several generations of mathematicians and experimenters. Over a part of the field their labours have been rewarded with a considerable degree of success. In all that concerns small vibrations, whether of air, as in sound, or of water, as in waves and tides, we have a large body of systematized knowledge, though in the case of the tides the question is seriously complicated by the fact that the rotation of the globe is actual and not merely relative to the sun and moon, as well as by the irregular outlines and depths of the various oceans. And even when the disturbance constituting the vibration is not small, some progress has been made, as in the theory of sound waves in one dimension, and of the tidal *bores*, which are such a remarkable feature of certain estuaries and rivers.

The general equations of fluid motions, when friction or viscosity is neglected, were laid down in quite early days by Euler and Lagrange, and in a sense they should contain the whole theory. But, as Whewell remarked, it soon appeared that these equations by themselves take us a surprisingly little way, and much mathematical and physical talent had to be expended before the truths hidden in them could be brought to light and exhibited in a practical shape. What was still more disconcerting, some of the general propositions so arrived at were found to be in flagrant contradiction with observation, even in cases where at first sight it would not seem that viscosity was likely to be important. Thus a solid body, submerged to a sufficient depth, should experience no resistance to its motion through water. On this principle the screw of a submerged boat would be useless, but, on the other hand, its services would not be needed. It is little wonder that practical men should declare that theoretical hydrodynamics has nothing at all to do with real fluids. Later we will return to some of these difficulties, not yet fully surmounted, but for the moment I will call your attention to simple phenomena of which theory can give a satisfactory account.

Considerable simplification attends the supposition that the motion is always the same at the same place—is *steady*, as we say—and fortunately this covers many problems of importance. Consider the flow of water along a pipe whose section varies. If the section were uniform, the pressure would vary along the length only in consequence of friction, which now we are neglecting. In the proposed pipe how will the pressure vary? I will not prophesy as to a Royal Institution audience, but I believe that most unsophisticated people suppose that a contracted place would give rise to an *increased* pressure. As was known to the initiated long ago, nothing can be further from this fact. The experiment is easily tried, either with air or water, so soon as we are provided with the right sort of tube. A suitable shape is shown in Fig. 1, but it is rather troublesome to construct in metal. W. Froude found paraffin-wax the most convenient material for ship models, and I have followed him in the experiment now shown. A brass tube is filled with candle-wax and bored out to the desired shape, as is easily done with templates of tin plate. When I blow through, a *suction* is developed at the narrows, as is witnessed by the rise of liquid in a manometer connected laterally.

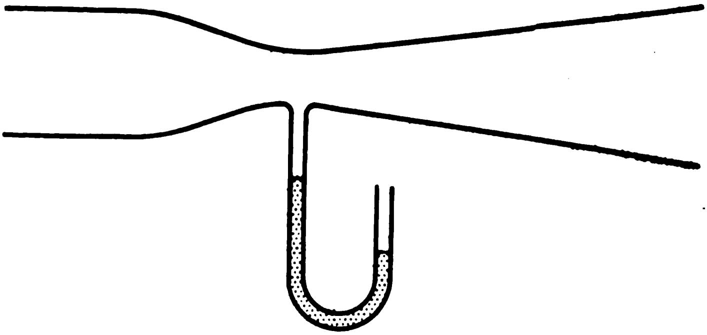

Fig. 1.

In the laboratory, where dry air from an acoustic bellows or a gas-holder is available, I have employed successfully tubes built up of cardboard, for a circular cross-section is not necessary. Three or more precisely similar pieces, cut for example to the shape shown in Fig. 2 and joined together closely along the edges, give the right kind of tube, and may be made air-tight with pasted paper or with sealing-wax. Perhaps a square section requiring four pieces is best. It is worth while to remark that there is no stretching of the cardboard, each side being merely *bent* in one dimension. A model is before you, and a study of it forms a simple and useful exercise in solid geometry.

Another form of the experiment is perhaps better known, though rather more difficult to think about. A tube (Fig. 3) ends in a flange. If I blow through the tube, a card presented to the flange is drawn up pretty closely, instead of being blown away as might be expected. When we consider the matter, we recognize that the channel between the flange and the card through which the air flows after leaving the tube is really an expanding one, and thus that the inner part may fairly be considered as a contracted place. The suction here developed holds the card up.

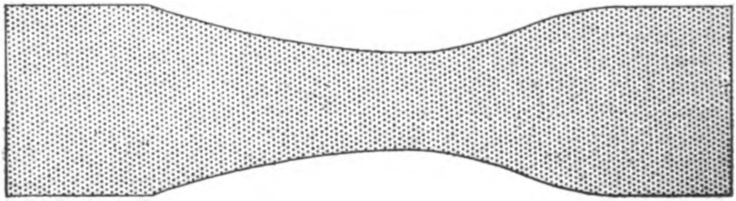

Fig. 2.

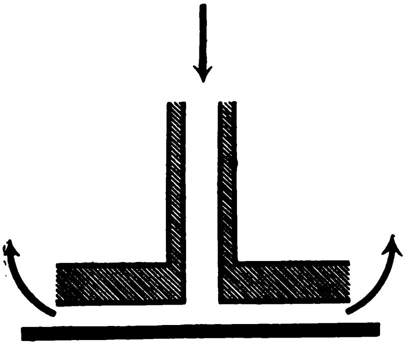

Fig. 3.

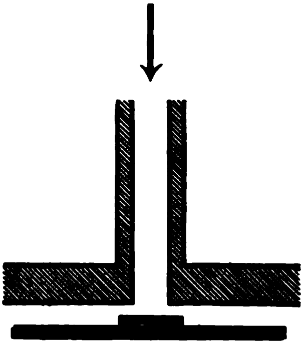

Fig. 4

A slight modification enhances the effect. It is obvious that immediately opposite the tube there will be pressure upon the card and not suction. To neutralize this a sort of cap is provided, attached to the flange, upon which the objectionable pressure is taken (Fig. 4). By blowing smartly from the mouth through this little apparatus it is easy to lift and hold up a penny for a short time.

The facts then are plain enough, but what is the explanation? It is really quite simple. In steady motion the quantity of fluid per second passing any section of the tube is everywhere the same. If the fluid be incompressible, and air in these experiments behaves pretty much as if it were, this means that the product of the velocity and area of cross-section is constant, so that at a narrow place the velocity of flow is necessarily increased. And when we enquire how the additional velocity in passing from a wider to a narrower place is to be acquired, we are compelled to recognize that it can only be in consequence of a fall of pressure. The suction at narrows is the only result consistent with the great principle of conservation of energy; but it remains rather an inversion of ordinary ideas that we should have to deduce the forces from this motion, rather than the motion from the forces.

The application of the principle is not always quite straightforward. Consider a tube of slightly conical form, open at both ends, and suppose that we direct upon the narrower end a jet of air from a tube having the same (narrower) section (Fig 5). We might expect this jet to enter the conical tube without much complication. But if we examine more closely a difficulty arises. The stream in the conical tube would have different velocities at the two ends, and therefore different pressures. The pressures at the ends could not both be atmospheric. Since at any rate the pressure at the wider delivery end must be very nearly atmospheric, that at the narrower end must be decidedly below that standard. The course of the events at the inlet is not so simple as supposed, and the apparent contradiction is evaded by an inflow of air from outside, in addition to the jet, which assumes at entry a narrower section.

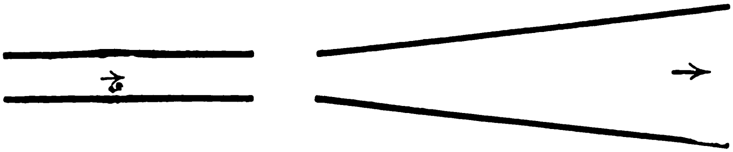

Fig. 5.

If the space surrounding the free jet is enclosed (Fig. 6), suction is there developed, and ultimately when the motion has become steady the jet enters the conical tube without contraction. A model shows the effect, and the principle is employed in a well-known laboratory instrument arranged for working off the water-mains.

I have hitherto dealt with air rather than water, not only because air makes no mess, but also because it is easier to ignore gravitation. But there is another and more difficult question. You will have noticed that in our expanding tubes the section changes only gradually. What happens when the expansion is more sudden—in the extreme case when the diameter of a previously uniform tube suddenly becomes infinite? (Fig. 3 without card.) Ordinary experience teaches that in such a case the flow does not follow the walls round the corner, but shoots across as a jet, which for a time preserves its individuality and something like its original section. Since the velocity is not lost, the pressure which would replace it is not developed. It is instructive to compare this case with another, experimented on by Savart * and W. Froude,† in which a free jet is projected through a short cone, or a mere hole in a thin wall, into a vessel under a higher pressure. The apparatus consists of two precisely similar vessels with apertures, in which the fluid (water) may be at different levels (Fig. 7, copied from Froude). Savart found that not a single drop of liquid was spilt so long as the pressure in the recipient vessel did not exceed one-sixth of that under which the jet issues. And Froude reports that so long as the head in the discharge cistern is maintained at a moderate height above that in the recipient cistern, the whole of the stream enters the recipient orifice, and there is "no waste, except the small sprinkling which is occasioned by inexactness of aim, and by want of exact circularity in the orifices." I am disposed to attach more importance to the small spill, at any rate when the conoids are absent or very short. For if there is no spill, the jet (it would seem) might as well be completely enclosed; and then it would propagate itself into the recipient cistern without sudden expansion and consequent recovery of pressure. In fact, the pressure at the narrows would never fall below that of the recipient cistern, and the discharge would be correspondingly lessened. When a decided spill occurs, Froude explains it as due to the retardation by friction of the outer layers, which are thus unable to force themselves against the pressure in front.

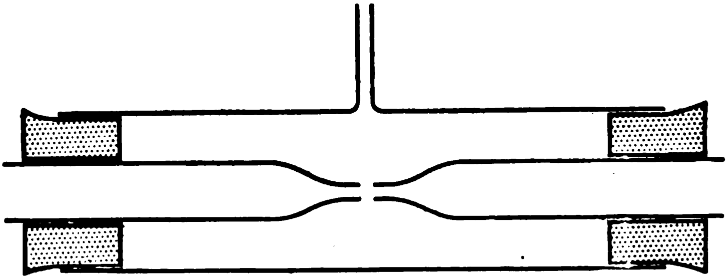

FIG. 6.

Evidently it is the behaviour of these outer layers, especially at

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* Ann. de Chimie, lv. p. 257, 1833.
† Nature, vol. iii. p. 93, 1875.

narrow places, which determines the character of the flow in a large variety of cases. They are held back, as Froude pointed out, by friction acting from the walls; but, on the other hand, when they lag they are pulled forward by layers farther in which still retain their velocity. If the latter prevail, the motion in the end may not be very different from what would occur in the absence of friction; otherwise an entirely altered motion may ensue. The situation as regards the rest of the fluid is much easier when the layers upon which the friction tells most are allowed to escape. This happens in instruments of the injector class, but I have sometimes wondered whether full advantage is taken of it. The long gradually expanding cones are overdone, perhaps, and the friction which they entail must have a bad effect.

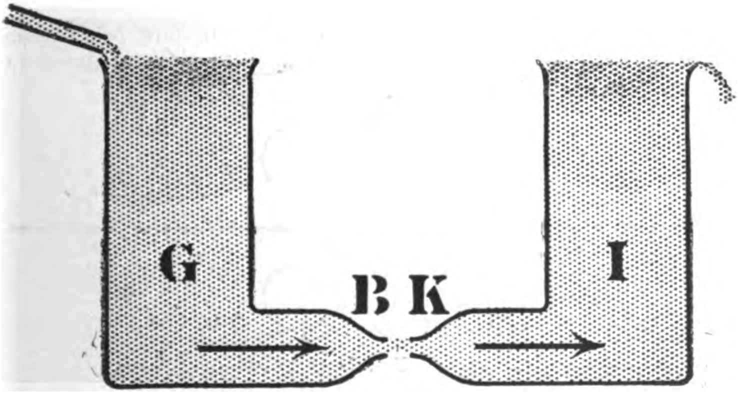

Fig. 7.

Similar considerations enter when we discuss the passage of a solid body through a large mass of fluid otherwise at rest, as in the case of an airship or submarine boat. I say a submarine, because when a ship moves upon the surface of the water the formation of waves constitutes a complication, and one of great importance when the speed is high. In order that the water in its relative motion may close in properly behind, the after-part of the ship must be suitably shaped, fine lines being more necessary at the stern than at the bow, as fish found out before men interested themselves in the problem. In a well-designed ship the whole resistance (apart from wave-making) may be ascribed to *skin friction*, of the same nature as that which is encountered when the ship is replaced by a thin plane moving edgeways.

At the other extreme we may consider the motion of a thin disk or blade flatways through the water. Here the actual motion differs altogether from that prescribed by the classical hydro-dynamics, according to which the character of the motion should be the same behind as in front. The liquid refuses to close in behind, and a region of more or less "dead water" is developed, entailing a greatly increased resistance. To meet this Helmholtz, Kirchhoff, and their followers have given calculations in which the fluid behind is supposed to move strictly with the advancing solid, and to be separated from the remainder of the mass by a surface at which a finite slip takes place. Although some difficulties remain, there can be no doubt that this theory constitutes a great advance. But the surface of separation is unstable, and in consequence of fluid friction it soon loses its sharpness, breaking up into more or less periodic eddies, described in some detail by Mallock (Fig. 8). It is these eddies which cause the whistling of the wind in trees and the more musical notes of the æolian harp.

The obstacle to the closing-in of the lines of flow behind the disk is doubtless, as before, the layer of liquid in close proximity to the disk, which at the edge has insufficient velocity for what is required of it. It would be an interesting experiment to try what would be the effect of allowing a small "spill." For this purpose the disk or blade would be made double, with a suction applied to the narrow interspace. Relieved of the slowly moving layer, the liquid might then be able to close in behind, and success would be witnessed by a greatly diminished resistance.

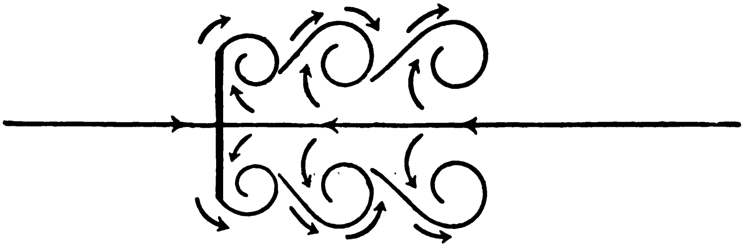

FIG. 8.

When a tolerably fair-shaped body moves through fluid, the relative velocity is greatest at the maximum section of the solid which is the minimum section for the fluid, and consequently the pressure is there least. Thus the water-level is depressed at and near the midship section of an advancing steamer, as is very evident in travelling along a canal. On the same principle may be explained the stability of a ball sustained on a vertical jet as in a well-known toy (shown). If the ball deviate to one side, the jet in bending round the surface develops a suction pulling the ball back. As Mr. Lanchester has remarked, the effect is aided by the rotation of the ball. That a convex surface is attracted by a jet playing obliquely upon it was demonstrated by T. Young more than 100 years ago by means of a model, of which a copy is before you (Fig. 9).

It has been impossible in dealing with experiments to keep quite clear of friction, but I wish now for a moment to revert to the ideal fluid of hydro-dynamics, in which pressure and inertia alone come into account. The possible motions of such a fluid fall into two great classes—those which do and those which do not involve *rotation*. What exactly is meant by rotation is best explained after the manner of Stokes. If we imagine any spherical portion of the fluid in its motion to be suddenly solidified, the resulting solid may be found to be rotating. If so, the original fluid is considered to possess rotation. If a mass of fluid moves irrotationally, no spherical portion would revolve on solidification. The importance of the distinction depends mainly upon the theorem, due to Lagrange and Cauchy, that the irrotational character is permanent, so that any portion of fluid at any time destitute of rotation will always remain so. Under this condition fluid motion is comparatively simple, and has been well studied. Unfortunately many of the results are very unpractical.

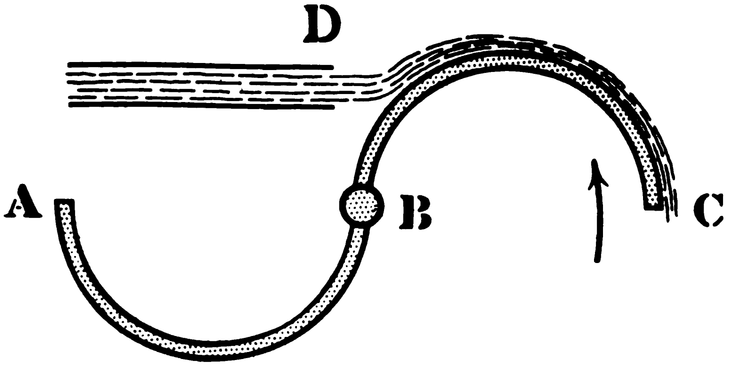

FIG. 9.

A plate, bent into the form A, B, C, turning on centre B, is impelled by a stream of air D in the direction shown.

As regards the other class of motions, the first great step was taken in 1858, by Helmholtz, who gave the theory of the vortex-ring. In a perfect fluid a vortex-ring has a certain permanence and individuality, which so much impressed Kelvin that he made it the foundation of a speculation as to the nature of matter. To him we owe also many further developments in pure theory.

On the experimental side, the first description of vortex-rings that I have come across is that by W. B. Rogers,* who instances their production during the bursting of bubbles of phosphuretted hydrogen, or the escape of smoke from cannon and from the lips of expert tobacconists. For private observation nothing is simpler than Helmholtz's method of drawing a partially immersed spoon along the surface, for example, of a cup of tea. Here half a ring only is developed, and the places where it meets the surface are shown as dimples, indicative of diminished pressure. The experiment, made on a larger scale, is now projected upon the screen, the surface of the liquid and its motion being made more evident by powder of lycopodium or sulphur scattered over it. In this case the ring is generated by the motion of a half-immersed circular disk, withdrawn after a travel of two or three inches. In a modified experiment the disk is replaced by a circular or semi-circular *aperture* cut in a larger plate, the level of the water coinciding with the horizontal diameter of the aperture. It may be noticed that while the first forward motion of the plate occasions a ring behind, the stoppage of the plate gives rise to a second ring in front. As was observed by Reusch,† the same thing occurs in the more usual method of projecting smoke-rings from a box; but in order to see it the box must be transparent.

In a lecture given here in 1877, Reynolds showed that a Helmholtz ring can push the parent disk before it, so that for a time there appears to be little resistance in its motion.

For an explanation of the origin of these rings we must appeal to friction, for in a perfect fluid no rotation can develop. It is easy to recognize that friction against the wall in which the aperture is perforated, or against the face of the disk in the other form of experiment, will start a rotation which, in a viscous fluid, such as air or water actually is, propagates itself to a finite distance inwards. But although a general explanation is easy, many of the details remain obscure.

It is apparent that in dealing with a large and interesting class of fluid motions we cannot go far without including fluid friction, or viscosity as it is generally called, in order to distinguish it from the very different sort of friction encountered by solids, unless well lubricated. In order to define it, we may consider the simplest case where fluid is included between two parallel walls, at unit distance apart, which move steadily, each in its own plane, with velocities which differ by unity. On the supposition that the fluid also moves in plane strata, the viscosity is measured by the tangential force per unit of area exercised by each stratum upon its neighbours. When we are concerned with internal motions only, we have to do rather with the so-called "kinematic viscosity," found by dividing the quantity above defined by the density of the fluid. On this system the viscosity of water is much *less* than that of air.

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* Amer. J. Sci., vol. xxvi. p. 246, 1858.

† Pogg. Ann., vol. cx. p. 309, 1860.

Viscosity varies with temperature; and it is well to remember that the viscosity of air increases while that of water decreases as the temperature rises. Also that the viscosity of water may be greatly increased by admixture with alcohol. I used these methods in 1879 during investigations respecting the influence of viscosity upon the behaviour of such fluid jets as are sensitive to sound and vibration.

Experimentally the simplest case of motion in which viscosity is paramount is the flow of fluid through capillary tubes. The laws of such motion are simple, and were well investigated by Poiseuille. This is the method employed in practice to determine viscosities. The apparatus before you is arranged to show the diminution of viscosity with rising temperature. In the cold the flow of water through the capillary tube is slow, and it requires sixty seconds to fill a small measuring vessel. When, however, the tube is heated by passing steam through the jacket surrounding it, the flow under the same head is much increased, and the measure is filled in twenty-six seconds. Another case of great practical importance, where viscosity is the leading consideration, relates to lubrication. In admirably conducted experiments Tower showed that the solid surfaces moving over one another should be separated by a complete film of oil, and that when this is attended to there is no wear. On this basis a fairly complete theory of lubrication has been developed, mainly by O. Reynolds. But the capillary nature of the fluid also enters to some extent, and it is not yet certain that the whole character of a lubricant can be expressed even in terms of both surface tension and viscosity.

It appears that in the extreme cases, when viscosity can be neglected and again when it is paramount, we are able to give a pretty good account of what passes. It is in the intermediate region, where both inertia and viscosity are of influence, that the difficulty is greatest. But even here we are not wholly without guidance. There is a general law, called the law of dynamical similarity, which is often of great service. In the past this law has been unaccountably neglected, and not only in the present field. It allows us to infer what will happen upon one scale of operations from what has been observed at another. On the present occasion I must limit myself to viscous fluids, for which the law of similarity was laid down in all its completeness by Stokes as long ago as 1850. It appears that similar motions may take place provided a certain condition be satisfied, viz. that the product of the linear dimension and the velocity, divided by the kinematic viscosity of the fluid, remain unchanged. Geometrical similarity is pre-supposed. An example will make this clearer. If we are dealing with a single fluid, say air under given conditions, the kinematic viscosity remains of course the same. When a solid sphere moves uniformly through air, the character of the motion of the fluid round it may depend upon the size of the sphere and upon the velocity with which it travels. But we may infer that the motions remain *similar*, if only the product of diameter and velocity be given. Thus, if we know the motion for a particular diameter and velocity of the sphere, we can infer what it will be when the velocity is halved and the diameter doubled. The fluid velocities also will everywhere be halved at the *corresponding* places. M. Eiffel found that for any sphere there is a velocity which may be regarded as critical, i.e. a velocity at which the law of resistance changes its character somewhat suddenly. It follows from the rule that these critical velocities should be inversely proportional to the diameters of the spheres, a conclusion in pretty good agreement with M. Eiffel's observations.* But the principle is at least equally important in effecting a comparison between different fluids. If we know what happens on a certain scale and at a certain velocity in *water*, we can infer what will happen in *air* on any other scale, provided the velocity is chosen suitably. It is assumed here that the compressibility of the air does not come into account, an assumption which is admissible so long as the velocities are small in comparison with that of sound.

But although the principle of similarity is well established on the theoretical side and has met with some confirmation in experiment, there has been much hesitation in applying it, due perhaps to certain discrepancies with observation which stand recorded. And there is another reason. It is rather difficult to understand how viscosity can play so large a part as it seems to do, especially when we introduce numbers, which make it appear that the viscosity of air, or water, is very small in relation to the other data occurring in practice. In order to remove these doubts it is very desirable to experiment with different viscosities, but this is not easy to do on a moderately large scale, as in the wind channels used for aeronautical purposes. I am therefore desirous of bringing before you some observations that I have recently made with very simple apparatus.

When liquid flows from one reservoir to another through a channel in which there is a contracted place, we can compare what we may call the *head* or driving pressure, i.e. the difference of the pressures in the two reservoirs, with the *suction*, i.e. the difference between the pressure in the recipient vessel and that lesser pressure to be found at the narrow place. The ratio of head to suction is a purely numerical quantity, and according to the principle of similarity it should for a given channel remain unchanged, provided the velocity be taken proportional to the kinematic viscosity of the fluid. The use of the same material channel throughout has the advantage that no question can arise as to geometrical similarity, which in principle should extend to any roughnesses upon the surface, while the necessary changes of velocity are easily attained by altering the head and those of viscosity by altering the temperature.

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* Comptes Rendus, Dec. 30, 1912; Jan. 13, 1913.

The apparatus consisted of two aspirator bottles (Fig. 10) containing water and connected below by a passage bored in a cylinder of lead, 7 cm. long, fitted water-tight with rubber corks. The form of channel actually employed is shown in Fig. 11. On the upstream side it contracts pretty suddenly from full bore (8 mm.) to the narrowest place, where the diameter is 2·75 mm. On the downstream side the expansion takes place in four or five steps, corresponding to the drills available. It had at first been intended to use a smooth curve, but preliminary trials showed that this was unnecessary, and the expansion by steps has the advantage of bringing before the mind the dragging action of the jets upon the thin layers of fluid between them and the walls. The three pressures concerned are indicated on manometer tubes as shown, and the two differences of level representing head and suction can be taken off with compasses and referred to a millimetre scale. In starting an observation the water is drawn up in the discharge vessel, as far as may be required, with the aid of an air-pump. The rubber cork at the top of the discharge vessel necessary for this purpose is not shown.

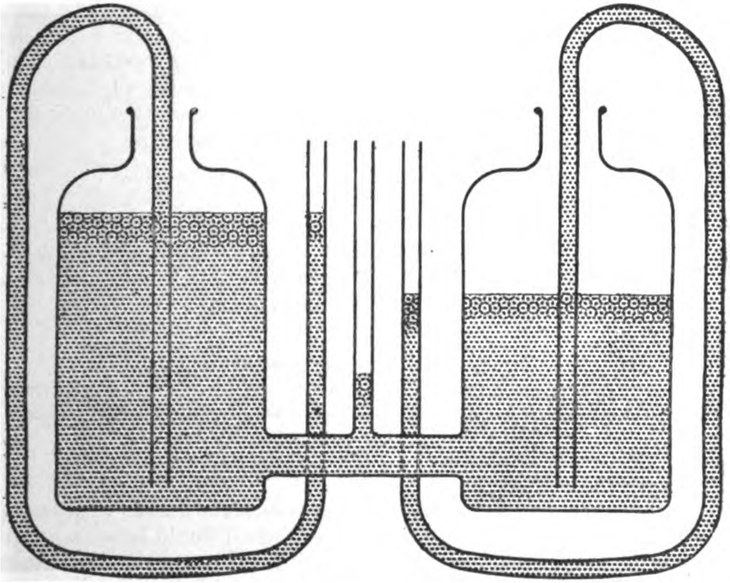


FIG. 10.

As the head falls during the flow of the water, the ratio of head to suction increases. For most of the observations I contented myself with recording the head for which the ratio of head to suction was exactly 2 : 1, as indicated by proportional compasses. Thus on January 23, when the temperature of the water was 9° C., the 2 : 1 ratio occurred on four trials at 120, 130, 123, 126, mean 125 mm. head. The temperature was then raised with precaution by pouring in warm water with passages backwards and forwards. The occurrence of the 2 : 1 ratio was now much retarded, the mean head being only 35 mm., corresponding to a mean temperature of 37° C. The ratio of head to suction is thus dependent upon the head or velocity, but when the velocity is altered the original ratio may be recovered if at the same time we make a suitable alteration of viscosity.

And the required alteration of viscosity is about what might have been expected. From Landolt's tables I find that for 9° C. the viscosity of water is ·01368, while for 37° C. it is ·00704. The ratio of viscosities is accordingly 1·943. The ratio of heads is 125 : 35. The ratio of *velocities* is the square-root of this or 1·890, in sufficiently good agreement with the ratio of viscosities.

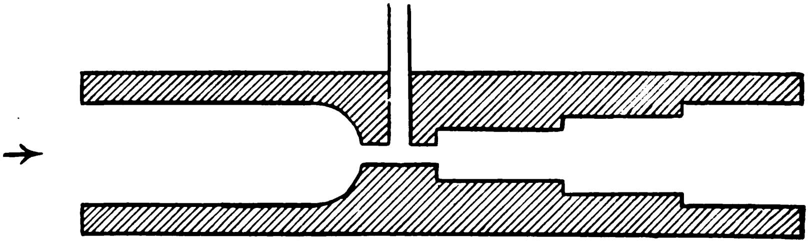


Fig. 11.

In some other trials the ratio of heads *exceeded* a little the ratio of viscosities. It is not pretended that the method would be an accurate one for the comparison of viscosities. The change in the ratio of head to suction is rather slow, and the measurement is usually somewhat prejudiced by unsteadiness in the suction manometer. Possibly better results would be obtained in more elaborate observations by several persons, the head and suction being recorded separately and referred to a time scale so as to facilitate interpolation. But as they stand the results suffice for my purpose, showing directly and conclusively the influence of viscosity as compensating a change in the velocity.

In conclusion, I must touch briefly upon a part of the subject where theory is still at fault, and I will limit myself to the simplest case of all—the uniform shearing motion of a viscous fluid between two parallel walls, one of which is at rest, while the other moves tangentially with uniform velocity. It is easy to prove that a uniform shearing motion of the fluid satisfies the dynamical equations, but the question remains: Is this motion stable? Does a small departure from the simple motion tend of itself to die out? In the case where the viscosity is relatively great, observation suggests an affirmative answer; and O. Reynolds, whose illness and comparatively early death were so great a loss to science, was able to deduce the same conclusion from theory. Reynolds' method has been improved, more especially by Professor Orr of Dublin. The simple motion is thoroughly stable if the viscosity exceed a certain specified value relative to the velocity of the moving plane and the distance between the planes; while if the viscosity is less than this, it is possible to propose a kind of departure from the original motion which will increase *for a time*. It is on this side of the question that there is a deficiency. When the viscosity is very small, observation appears to show that the simple motion is unstable, and we ought to be able to derive this result from theory. But even if we omit viscosity altogether, it does not appear possible to prove instability *à priori*, at least so long as we regard the walls as mathematically plane. We must confess that at the present we are unable to give a satisfactory account of skin-friction, in order to overcome which millions of horse-power are expended in our ships. Even in the older subjects there are plenty of problems left!

[R.]

WEEKLY EVENING MEETING,

Friday, March 27, 1914.

THE RIGHT HON. SIR JAMES STIRLING, P.C. M.A. LL.D. F.R.S., Vice-President, in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S. *M.R.I.*

Improvements in Long-Distance Telephony.

GREAT advances have been made of late years both in the scientific theory and in the practical appliances of telephony. To a few, but only to a few, of these improvements can your attention be invited during the present hour.

When we speak to an ordinary telephone transmitter, the voice causes rapid changes of air pressure against a thin metal disc called the diaphragm. This disc is bent inwards or outwards to a small extent in a manner which has a certain correspondence with the changes of air pressure.

These motions of the diaphragm are caused to compress more or less some granulations of hard carbon contained in a shallow receptacle behind it, and this again varies the electrical resistance in a circuit of which the carbon forms a part.

These changes in conductivity in turn produce variations of a nearly similar character in an electric current flowing in the line wire.

At the receiving end of the line this current flows through an induction coil and creates a secondary current, and the latter, circulating in the coils wound on the polar extensions of the electromagnet of the Bell telephone receiver, sets in motion its diaphragm and so reproduces approximately the changes of air pressure, and therefore the sound made against the transmitter.

Between speaker's voice and listener's ear there are therefore many transformations of energy, and at each of these there is not only some dissipation of energy, but also an imperfect translation of the complex motions or currents.

The transmitter diaphragm does not exactly copy by its motion the changes of air pressure made by the voice near it. The current into the line does not vary precisely in accordance with the motions of the diaphragm. The current at the receiving end does not accurately resemble that at the sending end, neither does the receiver translate back these received current variations into changes of air pressure or motion without error.

Apart altogether from the complications involved in working an Exchange System into which a human, and therefore a fallible, element enters, there are special difficulties in telephonic transmission which are introduced by the line or cable. It is to these that we shall devote our attention chiefly to-night, neglecting the imperfections of the transmitter and receiver themselves as translating devices.

This does not imply that the latter are unimportant, for indeed there is still great room for invention to overcome their present defects, but since the line or cable is the most costly element in long-distance telephony any improvement in its speech-transmitting powers is of the utmost importance.

If time permit, a few words may be said on the inventions which may enable the line in some cases to be entirely abolished.

It will be useful to mention first the means at our disposal for studying the changes of air pressure made near the transmitter and those of the current into the line.

If we speak or sing to an ordinary phonograph, the delicate chisel attached to the diaphragm of the mouth-piece cuts a groove in the plastic cylinder or disc, the bottom of which is very irregular, but is a copy more or less of the movements of the diaphragm.

I have attached to a diaphragm, in place of the chisel, a small lever which moves a little mirror, and I throw on to the mirror a ray of light. We can thus give to the reflected ray motions corresponding to those of the diaphragm. If the ray falls back on a steadily revolving mirror, consisting of an octagonal frame having its sides covered with looking-glass, and thence is reflected to a screen, the moving ray makes a line of light which is unbroken and nearly straight if the diaphragm is not in motion. When, however, we speak or sing to this artificial ear, or phonautograph, the movement of the small mirror causes a deflection of the ray, and moulds the line of light into a shape which renders the character of the air pressure changes visible to the eye.

If we sing to it a pure musical sound, the line crinkles itself into a smooth undulation called a simple harmonic curve, or curve of sines. If we act on it a more complex sound, such as that of a harmonium reed, we find the curve becoming rippled with other subsidiary undulations which give it an irregular outline, the irregularities being repeated at equal intervals. If we speak to the diaphragm a sentence we have a non-repetitive line, and if that sentence has in it certain consonantal sounds, particularly explosive consonants, such as *b* or *p*, the amplitude of the undulations becomes very large at some points. Thus the words—

> "To be or not to be, that is the question.
> Whether 'tis nobler in the mind to suffer
> The slings and arrows of outrageous fortune,
> Or to take arms against a sea of troubles,
> And by opposing end them ?"

reveal themselves on our line of light by casting it into a shape which suggests an outline of the Alps with its peaks and passes.

The loudness of the sound is represented by large amplitude in the curve, and in the case of a musical note the pitch of the sound by the distance from one hump to the next, this wave length being large for deep notes and short for high ones on the musical scale.

This curve, whatever may be its nature, is called the *wave form* of the sound, and the three qualities possessed by every sound—viz. loudness, pitch, and quality or timbre—are represented in the curve by the amplitude of height of the curve, by its wave length or distance in which it repeats itself, and by the shape of the curve whether regular or irregular.

Since the transmitting telephone is a device for translating aerial vibrations into electrical vibrations, we need another appliance for investigating the wave form of the electrical current, which is created in the cable, and this we have in that valuable instrument called the oscillograph. In the form given to it by Mr. Duddell it consists of a narrow loop of wire or two loops very tightly strained in the field of a powerful magnet. To the loop is attached a little mirror, and from this a ray of light is reflected, first on to a rocking mirror and then on to the screen. When an electric current passes up one wire of the loop and down the other it compels the wires to move in opposite directions through the magnetic field, and tilts the attached mirror to one side or the other. The ray is thus deflected, say, vertically by the changes of current in the loop and horizontally by the vibration of the rocking mirror. The combination of the two motions makes the spot of light on the screen describe a wavy curve, which represents to the eye the wave form of the current.

We have thus the means of delineating by the changing altitude or ordinate of a curve these invisible motions of the air particles or of the electrons in the wire which in reality are longitudinal—that is, in the direction of the motion of the wave.

In the next place another prefatory explanation should be advanced for the benefit of those who are not mathematicians.

If we draw under one another any number of simple harmonic curves having wave-lengths in the ratio of 1, 2, 3, etc., and various amplitudes, it is quite an easy matter to place these constituent curves one under the other in certain relative positions, and to plot a new curve the ordinate of which at every point is the sum of the respective ordinates of the conponent curves for the same abscissa. We thus derive a complex periodic curve. On the other hand, the inverse problem can also be solved. We can take any single-valued periodic curve however irregular and find out the constituent harmonic curves of which it is built up.

Nearly a century ago (1822) Jean Baptiste Joseph Fourier, in his classical work on the "Analytical Theory of Heat," which has been aptly termed a mathematical poem, showed that any periodic curve which is single-valued or not self-cutting can be built up by adding together the ordinates of a number of sine curves having respectively wave lengths in the ratio 1, 2, 3, etc., and of suitable amplitude and phase difference.

In virtue of the properties of certain mathematical functions, the process of analysing a curve into its component harmonics is easily reduced to arithmetic and can be even carried out by a machine. Hence any complex periodic curve depicting an aerial or an electrical vibration can be resolved into the sum of a number of simple harmonic constituents.

In the case of a complex electrical or aerial vibration this analysis is not merely a mathematical fiction, but can be performed physically, in the sense that we can separate out and extricate the harmonic constituents from such a complex electric or aerial vibration by means of devices called resonators.

Thus I have in my laboratory at University College a dynamo machine which gives an alternating or periodic electric current of a complex form, having a frequency of 1000. I can, however, by suitable electric resonators extract from it practically three electric currents of simple harmonic form which have frequencies respectively of 1000, 3000 and 5000. This process is analogous to that in which a complex musical sound, such as a chord, can be analysed into its component simple tones by acoustic resonators, and the ear enabled to pick out and hear the components separately when the complex sound is created.

Hence, when we apply to the end of a telephonic cable such a very complex electromotive force as that produced by speaking to a telephone transmitter attached to it, we are entitled to assume that the cable is being acted upon simultaneously by a number of simple harmonic electromotive forces having frequencies in the ratio of 1, 2, 3, 4, etc., these harmonic constituents having various amplitudes and phase differences.

We have, in the next place, to consider how the cable acts towards these alternating electromotive forces or vibratory currents of different frequencies.

Every electric line, whether overhead, aerial, underground or submarine cable, possesses four qualities, which are called its primary constants. In virtue of two of these it stores up energy in two forms, electrostatic and electromagnetic, and in virtue of the other two it dissipates these energies as heat.

The conservative qualities are called its capacity (C) and its inductance (L), and its dissipative ones are called its conductor resistance (R) and its dielectric leakance (S).

Every telephonic wire or cable is a sort of Leyden jar. It consists of a copper or bronze wire covered with gutta-percha, paper, or other insulator. This again is covered with a metal sheath or coating.

The insulator or dielectric, when acted upon by electromotive force, stores up energy electrostatically because it possesses capacity, but it also dissipates it because no dielectric is altogether free from conductance or leakance. When a current flows through the copper conductor it creates around it a magnetic field which represents stored energy, but at the same time some energy is being dissipated as heat because the conductor has electrical resistance.

From this double storage of energy it follows that if the electromotive force applied to the end of a cable is alternating or periodic in nature, the current is propagated along the cable as a wave motion. This means that the current at various points in the cable is not flowing in the same direction equally at the same instant, but ebbs and flows from point to point so that it is not high tide, so to speak, at all points at the same moment, but the high tide or maximum current takes place progressively along the cable.

A wave of current is therefore propagated along the cable, and the nearest distance between two places at which the current is a maximum or zero at the same instant is called the wave length.

The waves die away in amplitude or attenuate as they run along the cable because their energy is gradually frittered away by the heat they produce in the wire, and also by the similar action in the insulator.

These statements can be best illustrated by making use of the visible vibrations of flexible strings to represent the invisible oscillation of currents.

We have on the shaft of a small electric motor a pulley which has a crank pin in it driving a crank shaft, the other end of this crank shaft being attached to a rocking lever. A long cotton cord is fastened to a loop on the crank shaft, so that when the motor revolves one end of the cord has a circular but irrotational motion given to it. If the string is stretched horizontally by a weight passing over a pulley we have a medium along which we can propagate waves. The string possesses elasticity and also density or mass, and in virtue of this can store up energy in two forms, potential and kinetic, and have waves of displacement propagated along it. When one end of it is revolved uniformly by the motor, the rotation given to the end of the stretched string, therefore, travels along it as a wave of displacement. The speed with which this wave travels is directly proportional to the square root of the tension or pull on the string, and inversely as the square root of the weight of the string per unit of length.

Tyndall describes in his "Lectures on Sound" (Lect. III.) an experiment in which a cord fixed at one end by a swivel had its other end rotated by a whirling table so as to produce on it stationary waves. The use of an electric motor is, however, a considerable improvement on any method depending on hand rotation.

If the length of the string is properly adjusted, the repeated waves produced by the revolution of the motor run up the string and are reflected at the fixed end, thus meeting the outgoing waves and making stationary waves on the string. By adjusting the tension we can alter their velocity, and therefore the wave length or double the distance from node to node. The number of vibrations of the cord is given by a counter which records the speed of the motor. The motion travels along the string a distance equal to one wave length in the time of one complete vibration. We can with this apparatus experimentally prove all the above statements. Taking a string of known density or weight per inch or centimetre, and stretched by a known weight or tension, we can find the square root of the quotient of tension by density, which gives us the velocity of the wave, and compare this number with the measured product of wave length and frequency, which also gives us the wave velocity.

The two numbers are found to be the same. If we employ a string one-half of which is single and the other half four-fold, we find on vibrating it that the waves are double as long on the part which is single as they are on the part which is four-fold, thus proving that the velocity varies inversely as the square root of the density. By varying the speed of the motor whilst keeping the tension of the string constant, we can obtain stationary waves of various wave lengths, and prove that the velocity of the wave along the string is the same for all wave lengths. (See Fig. 1.)

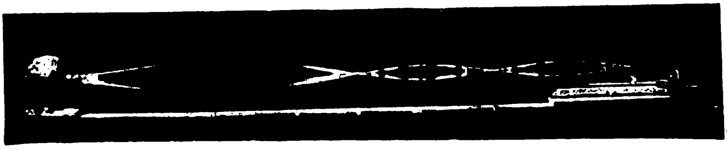
In the case of this vibrating string, the mass per unit of length corresponds to the inductance of an electric cable, and the tension applied to the string to the inverse or reciprocal of the capacity of the cable, but if the string is very flexible, there is no quality which corresponds to the electric resistance of the cable, or to its dielectric leakance. We must therefore turn for assistance to another case of wave motion.

If we attach to a horizontal steel wire a number of slips of metal all soldered transversely to the wire, and if we give the end slip a sudden displacement so as to twist the steel wire one way or the opposite, a wave of twist, or a torsional wave, will run along it.

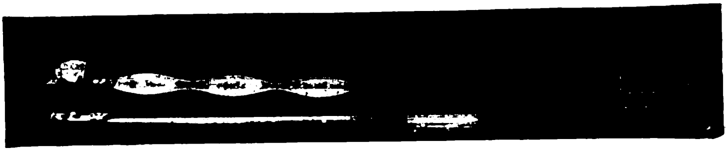
In this case the torsional elasticity of the wire corresponds to the tension in the case of the stretched string, and the mass of the system per unit of length to the density of the string. This latter corresponds also to the inductance of our telegraph cable, and the torsional elasticity to the reciprocal of the cable capacity per unit of length. If we suppose this arrangement to be immersed in water so that the oscillatory motion of each transverse strip is resisted, the water produces a decay or attenuation in the waves transmitted along it which corresponds to the action of the electrical resistance of the conductor in attenuating electric waves running along it. Moreover, it is clear, since fluid friction increases with velocity, the attenuation of the waves propagated along this arrangement would be greater for waves of short wave lengths or high frequency than for long waves of low frequency.

The point to notice is that by making these transverse strips heavier we can give them a greater store of energy, and thus render the effect of the fluid friction in retarding them less marked sensible. Again, if we suppose the steel wire to be imperfectly elastic and to possess in some degree a quality like pitch or wax of yielding to a slow steady stress, but not to a rapidly alternating one, it will be

I.—STANDING WAVES ON UNLOADED STRING.



II.—STANDING WAVES ON STRING HALF LOADED, HALF UNLOADED.



III.—REFLECTION OF WAVE BY LOAD AT CENTRE OF STRING.

IV.—STANDING WAVES ON STRING HALF SINGLE, HALF FOURFOLD.

V.—STANDING WAVES ON LOADED STRING, WAVE-LENGTH COVERS ABOUT THREE LOADS.

FIG. 1.

clear that slow vibrations or long waves of tension would be attenuated more in travelling up such a strip than quick vibrations or short waves. Such a quality of imperfect elasticity in the wire could correspond to conductivity or leakance in the dielection of a cable.

Hence, we may sum up all our statements with regard to an electric cable by saying that in virtue of its two energy-storing qualities—viz. the inductance and capacity—we can have waves of electric current or potential propagated along it just as we can have waves of twist or displacement propagated along a steel strip or a flexible thread. Also in consequence of its two energy-dissipating powers of conductor resistance and dielectric leakance, these electric waves along the cable are attenuated or weakened as they run along it by reason of the gradual dissipation of the energy.

We can quite easily show the production of stationary electric waves on a wire by applying to the end of it a high frequency alternating electromotive force, which in every way correspond to the stationary waves we have produced on the string.

If, however, we employ a straight wire the wave lengths are in general so large that it is difficult to bring them with the space of a room. We can, however, compress them by coiling the wire in a close spiral round an ebonite rod. The high frequency electromotive forces are created by connecting one end of this helix to some oscillatory electric circuit composed of a condenser, an inductive circuit, and a spark gap. By adjusting this frequency to certain values we can create stationary electric waves on the helix and the loops, and nodes can be rendered visible by holding at various positions a neon vacuum-tube, or by the electric glow on a fine wire stretched parallel to the helix.

In the case of the helix here used in this experiment the velocity of the electric waves along it is 1250 miles per second, and varies inversely as the square root of the product of the capacity and inductance of the helix per unit of length. We have, however, little or no variation of velocity or attenuation of the waves with wave length, because there is only very little dissipation of energy by the resistance of the wire.

I wish, however, to make evident to you that in an ordinary submarine cable this is not the case.

For this purpose I shall make use of an artificial cable constructed on Dr. Muirhead's plan. In this appliance, the conductor, which represents the wire of the cable, is a strip of tinfoil cut into a zigzag shape and attached to a sheet of paraffined paper, whilst on the other side of this paper is another complete sheet of tinfoil. This arrangement has therefore capacity, resistance, and inductance like a real cable. By employing a sufficient number of these sheets we can imitate the electrical structure of any number of miles of submarine cable. The artificial cable in front of me is equivalent to 100 miles of ordinary deep-sea cable, and I have the ends of the sections brought out so that I can attach a receiver to any point on the line.

I shall apply to this artificial cable a periodic or alternating electromotive force, the wave form of which is very irregular. This can be produced by a suitable alternator and transformer, by means of which an alternating current of the required irregular wave form can be created.

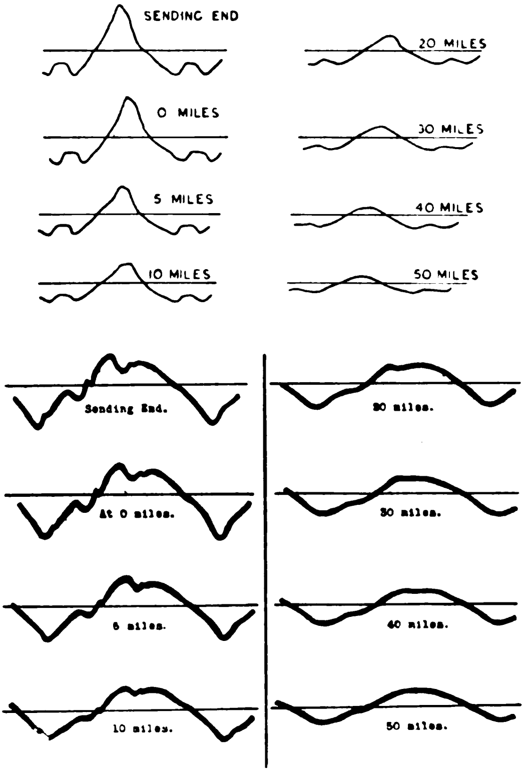

SENDING END
20 MILES
0 MILES
30 MILES
5 MILES
40 MILES
10 MILES
50 MILES
Sending End.
20 miles.
At 0 miles.
30 miles.
5 miles.
40 miles.
10 miles.
50 miles.

Fig. 2.—Two Examples of the Distortion of an Electric Wave in travelling along a Cable.

Capacity of cable = 0·3018 mfds. per mile.
Resistance of cable = 7·235 ohms per mile.

We shall then employ a Duddell oscillograph to throw upon the screen a representation, in the form of a line of light, of the wave form of this electromotive force. The oscillograph contains two wires, and one of these is employed as above to depict the wave form of the electromotive force at the sending of the cable. The other wire can be employed to pick up and depict the wave form at any required distance, 10 miles, 20 miles, and so on up to the full length of 100 miles.

If, then, we compare the wave form of the current at the receiving and sending ends when only a very short length of the cable intervenes, we find them practically the same. If, however, we test the wave form at 20 or 30 miles distance we find it somewhat different, and as we go farther and farther along the cable we see the wave form not only become attenuated, that is, reduced in amplitude, but smoothed out so that it has lost all the little irregularities of the curve at the sending end. (See Fig. 2.)

It is obvious that this is due to the harmonics of higher frequency being more attenuated by the cable than the lower ones or fundamentals.

This change in the wave form is called the *distortion*. If we translate this fact into its equivalent in sound, it means that the action of the cable is not merely to reduce the loudness of the sound, but also to take away its quality, or those characteristics which give it intelligible meaning, because these depend upon the presence of the higher harmonics.

A sound, therefore, at the sending end which has some distinctive vowel-sound character becomes by the action of the cable so emasculated that at the receiving end it is recognizable as no vowel in particular.

This distortion is well shown by a series of photographs taken with the oscillograph, showing the wave form at the beginning and at the end of a cable of certain typical sounds. These oscillograms were taken by Mr. B. S. Cohen in the Research Laboratory of the General Post Office, and I am indebted to him for the loan of them.

You will see how strikingly different the wave form of the current coming out of the cable is from that entering it when certain vowel sounds or syllables are uttered to a transmitter connected to a receiver through 30 miles or so of standard cable. (See Fig. 3.)

The problem presented was then to find the practical remedy for this attenuation or decay of the amplitude of the waves, and especially to cure the unequal attenuation of the short and long waves which, according to Heaviside's theory, is the principal source of this distortion.

It was long ago noticed that a leak or want of insulation in a telephone cable will sometimes actually improve the clearness of speech transmitted through it. Hence, one remedy for distortion proposed by Professor Silvanus Thompson in 1891 was to put upon the cable at intervals artificial leaks of a particular kind called inductive shunts. It is, however, necessary to preserve a certain loudness in the sound as well as clearness in order that speech may be intelligible, and it was the doubt whether this method when tried on long cables would preserve the necessary wave amplitude that perhaps hindered its trial on a large scale.

Mr. Oliver Heaviside had, however, pointed out in 1887 that the wave distortion in cables could be cured by giving to the cable increased inductance; and he proved mathematically that if the four line constants of the cable per mile were so adjusted as to magnitude that the product of the line inductance and insulator conductance was equal to the product of line resistance and capacity, then this cable would possess the peculiar property that vibrations or electric waves of all wave lengths would travel along it with equal speed and be attenuated at an equal rate or in equal proportions.

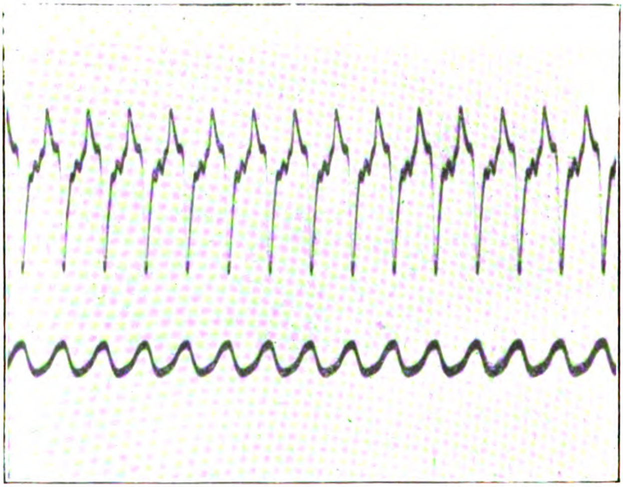


FIG. 3.—DISTORTION OF VOWEL SOUND "OO" BY TRANSMISSION ALONG 30 MILES OF STANDARD TELEPHONE CABLE.

The upper curve shows the wave form of the current at the sending end, and the lower curve the distorted form at the receiving end of the cable.

Hence such a cable would allow a wave form to be propagated along it without distortion.

In all ordinary cables the product of the line resistance and line capacity is too large to fulfil Heaviside's condition, and hence we must give the cable greater inductance to bring about an approximation to the distortionless condition.

This addition of inductance is called *loading* the cable.

Heaviside merely gave a general instruction that loading would be beneficial, but the exact details how to do it were left to be discovered.

In the years 1899 and 1900, Professor Pupin, of Columbia College, New York, published two very important papers, in which he discussed the result of inserting into the cable at intervals loading coils of insulated wire wound on an iron wire core so as to give them high inductance.

The important contribution he made to the subject was to show that if these coils were at such a distance apart that there were 8 or 10 loading coils in the span of a single electric wave length on that cable, corresponding to the mean speech frequency (800 per second), they acted just as if their inductance were evenly distributed along the cable.

Under these conditions they do not cause much loss of wave amplitude by reflection. If, however, the loads are farther apart so that only two or three loads are comprised in a wave length, there would be great loss of wave amplitude by reflections at the loading coils.

The mechanical equivalent of this can be very well shown with the vibrating string apparatus I have just exhibited to you. If we place on a string a heavy glass bead somewhere about the middle and then set the string vibrating, we shall see quite easily that the wave is reflected at the bead and only a small portion of it transmitted. In the first section of the cord there are vigorous vibrations, but on the other side of the load they are very feeble. (See III, Fig. 1.)

If we put on the string several beads, and adjust the tension and frequency of the vibrations so that the half wave length extends rather more than the distance which separates two adjacent beads, we shall notice that a reflection of the wave takes place at each bead which rapidly attenuates the wave (see V, Fig. 1). Precisely the same thing takes place in the electrical case. Hence in practice it is found that the wave length on the cable must extend over 8 or 9 loading coils at least, these coils being placed at equal distances.

The principal result of this loading when so carried out is to make a remarkable reduction in the attenuation of the waves, and to render less unequal the attenuation of waves of different wave lengths.

In the case of an unloaded telephone cable the shortest waves travel fastest and attenuate most rapidly. Hence, if we start a complex oscillation on the cable, the shorter harmonics in the wave form are weeded out soonest and die away fastest. It is as if a crowd of men, youths and boys start on a race. The youngsters, with nimble limbs, race ahead at first, but they have not the staying power of the men and are soon played out, whilst the strong men stay in and survive to the end.

Two very important characteristics of a cable are its *attenuation constant* and *wave length constant*.

The reciprocal of the attenuation constant is the distance in which the wave amplitude diminishes to 0·367 of its amplitude at the sending end. This last fraction is the reciprocal of the base of the Napierian logarithms. Hence, if the attenuation constant is 0·1, then in 10 miles run along the cable the wave amplitude will be reduced to about one-third of that at the sending end. The wave length constant is a number the reciprocal of which multiplied by 6·28 gives the wave length or distance from crest to crest of the waves. The wave velocity is the product of the wave length and frequency. These numbers can all be calculated from the four primary constants of the cable.

If we take any cable, such as the cable used in telephony, as a standard of comparison, which has a resistance of 88 ohms, an inductance of 1 millihenry, and a capacity of 0·05 of a microfarad, all per loop mile (that is, a mile run of lead and return), and if we calculate the above quantities for it for various frequencies, we can set them out as in Table I. You will see that as the frequency increases, the attenuation and the wave velocity both increase, whilst the wave length diminishes.

This shows that there is no particular velocity of electricity along a wire. It depends upon the wire and upon the speed of the oscillations.

The average frequency which we have to consider in telephony is about 800, although some oscillations, such as those of the higher harmonics, are quicker, and some fundamental tones are lower. This frequency has, however, been selected as the mean or standard frequency for telephonic speech. It corresponds nearly to a note on the musical scale represented by the upper G in the treble clef.

TABLE I.—ATTENUATION CONSTANTS, WAVE LENGTH, AND WAVE VELOCITIES FOR VARIOUS FREQUENCIES ALONG A STANDARD TELEPHONE CABLE HAVING RESISTANCE OF 88 OHMS, INDUCTANCE 1 MILLIHENRY, AND CAPACITY OF 0·05 MICROFARAD, ALL PER LOOP MILE.

Frequency	Attenuation Constant	Wave Length Constant	Wave Length in Miles	Wave Velocity in Miles per Second
100	0·0370	0·0373	168·0	16,800
1,000	0·1134	0·1218	51·5800	51,580
10,000	0·2667	0·5181	12·1300	121,300
100,000	0·3095	4·4530	1·4110	141,100
1,000,000	0·3110	44·3700	0·1414	141,400

For this frequency (800) and for the standard cable, the attenuation constant is about 0·1, which means that in 10 miles the wave amplitude has decreased to 0·367 of its value at the sending end. The wave length is 58·1 miles and wave velocity 46,480 miles per second. The speech limit on this table is about 47 miles, and there is considerable distortion of the waves. If, however, we insert in the run of this cable, every 2 miles or so, inductance coils consisting of rings of iron wire wound over with many turns of insulated copper wire, and include this copper wire in the telephone circuit, we shall give to the cable much more inductance per mile. (See Fig. 4.)

These coils are called *loading coils*, and they are usually made to have an inductance of about 100 or 200 millihenrys. Suppose that such coils are inserted every mile in the run of a standard cable. Then without increasing much the capacity or resistance of the line, the inductance per mile can be made 100 times greater.

The result of this is to reduce the attenuation constant to about one-third of that of the unloaded cable, the wave length to about 18 miles, and the velocity to about 13,280 miles per second for the frequency of 800.

Also, this loading reduces the inequality in the attenuation and velocity of waves of different wave lengths, and hence prevents that extinguishing of the higher harmonics relatively to the lower, which is the chief cause of the distortion of the wave form. If these loading coils are not put farther apart in this cable than 2 miles or so, a wave length of 18 miles will still extend over 9 or 10 coils, and the loss of wave amplitude by reflection at each coil will not be great. This proposal of Pupin to locate an added inductance in separate coils, placed at intervals in the run of the cable and at such a distance that 9 or 10 coils are covered by one wave length, proved to be a very effective method of carrying into effect Heaviside's recommendations.

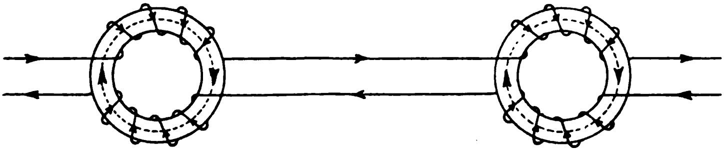


FIG. 4.—DIAGRAM SHOWING INSERTION OF LOADING OR PUPIN COILS INTO A TELEPHONE CIRCUIT.

This loading has resulted in a great improvement in those long distance telephone lines to which it has been applied. You may ask for some common-sense explanation of this improvement. It is really due to the greater energy which the added inductance imparts to the current waves. If, for instance, two ships of equal size and shape, but unequal weight, were being launched down ways of equal inclination into the water and allowed to run out freely as far as they would go until their energy was exhausted, the heavier ship having the greater store of energy would go farthest because it has more energy to expend in overcoming water friction. So also is it with the electric waves in the cable. We have to consider in telephone engineering the three cases of aerial or overhead lines, underground cables, and submarine cables, and it may be well at this point to say a few words as to the manner in which these three kinds of lines are now loaded, or "Pupinized" as the French call it.

In the case of overhead lines the inductance coils are included in iron boxes, which can be attached to the telegraph posts at the proper intervals. (See Fig. 5.)

For underground lines they are put in iron boxes in pits and the coils for a large number of lines can be placed in the same pit. (See Fig. 6.)

The type of cable now used for urban and underground work is a cable called a dry core cable, in which each wire is insulated by twisting loosely round it a strip of dry paper, and numerous pairs of such wires are enclosed in a lead sheath drawn over them. The insulation of each wire is thus really air and paper, and the advantage of this is much smaller value of the capacity compared with a wire insulated with gutta-percha or indiarubber.

The conduits in which these cables are run have to be kept dry by passing dry air through them.

The problem of loading a submarine cable with Pupin coils presented greater difficulties both in construction and in laying. These difficulties were overcome with great success after much preliminary research by the scientific staff of Messrs. Siemens Bros., and in the Engineering Department of the General Post Office when under the able control of Major W. A. J. O'Meara as Engineer-in-Chief.

Two such loaded cables have now been laid across the English Channel from England to France, and to Belgium for the British Government, and also another type of loaded cable has been laid by the French Administration close to one of the above cables between France and England. A coil loaded cable has also been laid by Messrs. Siemens Bros. for the General Post Office across the Irish Channel, under the direction of the present Engineer-in-Chief, Mr. W. Slingo.

The problems to be solved in successfully making and laying a loaded submarine telephone cable were partly electrical and partly mechanical, and required the greatest experience and knowledge to grapple with them.

As regards the cable laid in 1910 between England and France from Abbots Cliff to Grisnez, the full details of all the previous researches and of the structure and laying of this cable have been described in an admirable and most valuable paper by Major W. A. J. O'Meara, C.M.G., at that time Engineer-in-Chief to the General Post

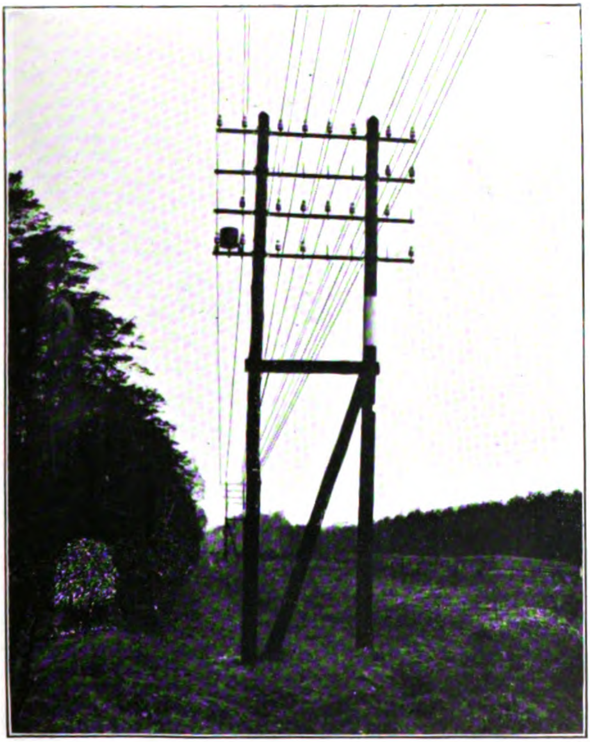


FIG. 5.—LOADING COIL PLACED ON THE CROSS ARM OF A TELEGRAPH POST, AS USED IN LOADED AERIAL TELEPHONE LINES.

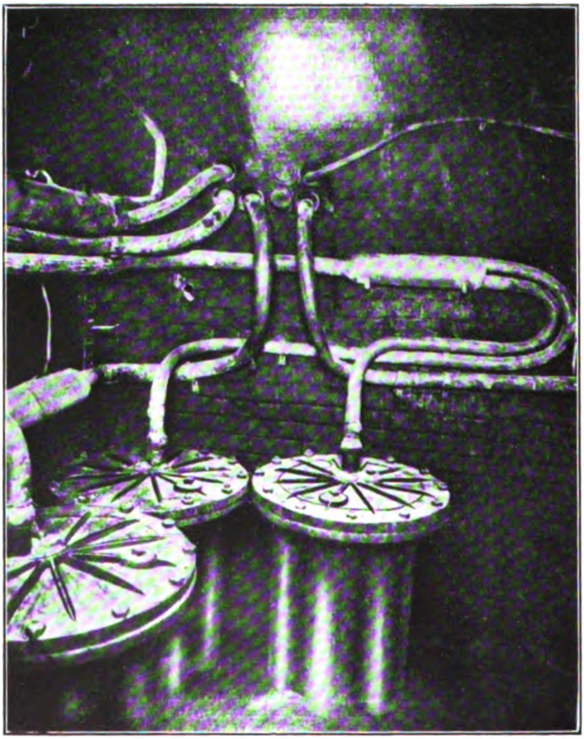


Fig. 6.—Loading Coils placed in a Pit, as used in Loaded Underground Telephone Cables.

Office, read to the Institution of Electrical Engineers about three years ago.*

This cable has a length of 21 nautical miles, and contains four copper conductors forming two separate circuits, these being so arranged that no mutual inductance, and therefore no "cross talk," exists between them. The copper is insulated with gutta-percha and armoured, as usual in the case of deep-sea cables. At intervals of 1 nautical mile iron-cored inductance coils are inserted in each line, these coils each having a resistance of 6 ohms and the inductance of 0·1 henry at 750 periods per second. Each iron-wire core has on it two windings which are placed respectively in the circuit of the two wires forming the lead and return of each circuit.

These coils are of such form that when insulated over they merely present a certain extra thickening of the cable for a length of about 30 inches, and with some extra care presents no insuperable difficulty in either laying the cable or picking it up.

The construction of the core of this 1910 Anglo-French telephone cable is such that if no loading coils were introduced, the attenuation constant at a frequency of 750 would be nearly $\frac{1}{20}$ or 0·0520. This means that in a run of twenty miles the amplitude of the current would be reduced to 36·7 per cent of that at the sending end. As loaded with the coils of 100 millihenrys inductance placed one nautical mile apart, the attenuation constant is actually only $\frac{1}{60}$ or 0·0166, which means that the current would be reduced in the above ratio by travelling sixty miles along the cable.

In the case of this 1910 Anglo-French telephone cable, special measurements showed that the attenuation constant for different frequencies between 700 and 1700, varied almost proportionately to the frequency. This shows that, although Heaviside's condition for complete non-distortion is not fulfilled, yet the diminution in the attenuation which is obtained by loading resulting in greater loudness in the sound, is a very great improvement over the unloaded cable.

A second loaded cable was laid in 1911 by the British Post Office across the Channel between England and Belgium, running from St. Margaret's Bay near Dover, to La Panne in Belgium. This cable, which has a length of forty-eight nautical miles, was also manufactured by Messrs. Siemens Bros., and is of the same general construction as the Anglo-French (1910) cable in that it contains two complete metallic circuits each separately loaded. It is noteworthy, however, that the guttapercha used in the insulation of the Anglo-Belgian cable is telephonically much superior to that in the Anglo-French cable.

The explanation of this matter is as follows:—Every dielectric or insulator, such as paper, guttapercha, etc., possesses some small degree of true conductivity for alternating electric currents. This conductivity, as shown by experiments made in my laboratory at University College a year or two ago, increases with the frequency of the oscillations and with the temperature.

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* See Journal of the Proceedings of the Inst. Elect. Eng. vol. xlvi. The lecturer was indebted to Major O'Meara for the loan of slides for this discourse illustrating the structure and laying of this 1910 Anglo-French loaded telephone cable.

In the case of telephone cables, and especially loaded cables, the attenuation depends, amongst other things, upon the ratio of the dielectric conductivity (S) to the capacity (C) of the cable per unit of length.

This ratio of S/C is rather large for guttapercha at ordinary temperature and for a frequency of 800 to 1000. Hence the importance of it. For guttapercha such as was used in the Anglo-French cable, the value of S/C was about 120. Before making the Anglo-Belgian cable, Messrs. Siemens discovered a method of preparing, or treating it so as to reduce the value of S/C to 12 for that cable. This is an enormous improvement on previous materials. The exact nature of this mode of preparation has not been divulged. It may be in part at least a most careful desiccation of the guttapercha, as I have found that a very small amount of moisture has the effect of greatly increasing this ratio for a dielectric.

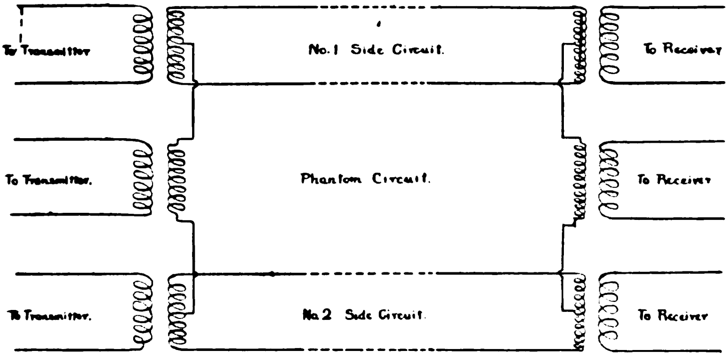

Fig. 7.—Arrangement of Side and Phantom Circuits for Phantom Working in Duplex Circuits.

The Anglo-Belgian cable, like the Anglo-French cable, is a coil-loaded cable, but is so constructed that by a simultaneous use of the two circuits in it, it is possible to provide a third, or so-called phantom circuit, obtained as follows :—

The cable has in it two complete metallic circuits, each of which can be used separately as an independent telephone circuit. The circuits are connected at the two ends through induction coils, or transformers, to their respective transmitters and receivers. If, however, we use the two wires of one circuit as a lead, and the two wires of the other as a return, we can make a third circuit, or phantom circuit, and telephone along it quite independently of what is going on along the other two main circuits. (See Fig. 7.)

Moreover, by a very ingenious arrangement it has been found possible to load the phantom circuit by inductance coils, which are so wound that whilst they add inductance to the phantom they do not alter that of the component circuits, which last have, however, their own independent loading coils. (See Fig. 8.)

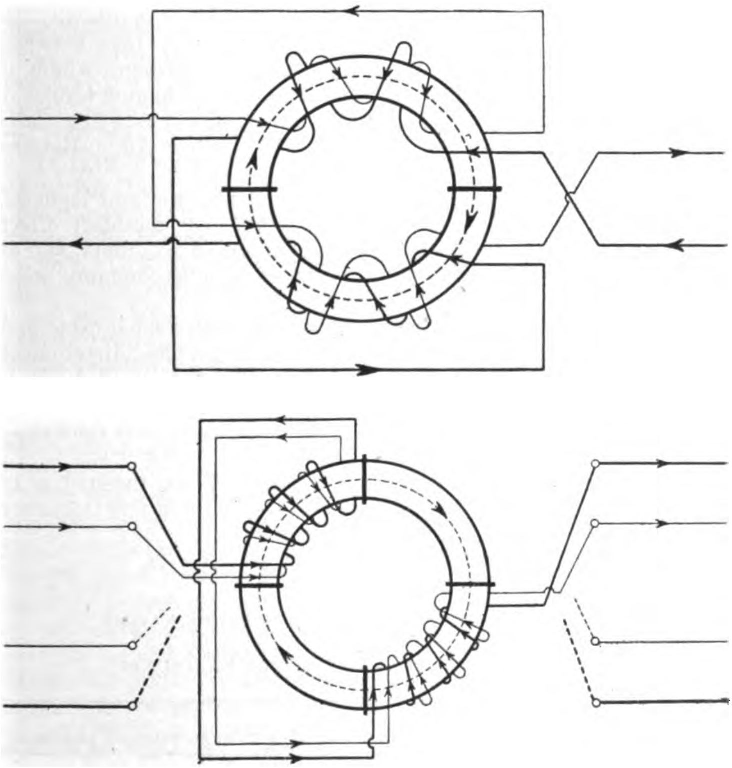

Fig. 8.

The upper diagram represents the mode of winding each loading placed in the side circuits of a pair circuit cable. The lower diagram represents the mode of carrying each side circuit pair of conductors round a loading coil so as to load the phantom circuit. The winding of one side circuit only is shown.

In this way a great economy of copper is obtained under a sort of arithmetic in which 2 and 2 do not make 4 but make 6.

In fact the phantom circuit is better than either of the circuits of which it is composed; for one reason because each conductor of the phantom has double the section of copper compared with the component physical circuits. This alone tends to reduce the attenuation constant. It is, therefore, often possible to speak farther through the phantom circuit than through the component or side circuits.

These cross-Channel loaded cables have very greatly facilitated telephonic communication between England and the Continent. A telephonic service is now given by the General Post Office between London and Geneva, and other cities in the Continent, which is conducted through one or other of the two loaded Channel Cables.

Telephonic conversation has also been conducted experimentally between Berlin and London through the loaded Anglo-Belgian Cable.

Another loaded submarine cable between England and Holland, is I believe, in course of construction, which will establish direct telephonic communication between London and Amsterdam, and at the same time, render possible inter-telephonic communication between England and Northern Germany.

The latest submarine telephone cable is the coil-loaded cable laid last year (1913) across the Irish Channel under the direction of Mr. W. Slingo, the present Engineer-in-Chief of the General Post Office. This cable, 64 nautical miles long, is a two-circuit cable arranged for phantom working, and is the longest loaded submarine cable actually working.

At intervals of one nautical mile Pupin coils are inserted, as in the Anglo-Belgian Cable. The attenuation constant at 800 frequency is 0·015.

The details of all these cables are given in Table II.

TABLE II.—Loaded Submarine Telephone Cables.

All values are per nautical mile of loop at 800 frequency.

Cable . . .	Anglo-French Coil loaded (1910)	Anglo-Belgian Coil loaded (1911) Side circuit	Anglo-Belgian Phantom Circuit	Anglo-Irish Coil loaded (1913)
Length in nauts . . .	21	48	48	64
Coil-spacing in nauts . .	1	1	1	1
Coil Resistance in ohms .	6·6	11·5	4·6	6·8
Total Resistance in ohms	20·9	25·7	11·7	21·0
Capacity in mfds. . . .	0·138	0·162	0·314	0·166
Inductance in henrys . .	0·1	0·1	0·05	0·1
Ratio S/C	120	12	12	15
Attenuation Constant. .	0·017	0·018	0·0173	0·015
Total Attenuation . . .	0·36	0·86	0·83	0·96
Conductor Weight per naut	160 lb.	160 lb.	320 lb.	160 lb.

Then a word or two must be said on another type of loading which is called *continuous loading.* It is really a reversion to Heaviside's original idea of adding inductance all along the cable. If a stranded copper cable has a soft iron wire uniformly and closely wound on it as a spiral, this iron increases the inductance of the wire to a certain extent. It also somewhat increases the capacity and the resistance of the cable, but nevertheless reduces the attenuation constant. This type of loaded cable is sometimes called a Krarup cable after the Danish Engineer who first employed it for practical telephony. A good deal of discussion has taken place amongst telephonists as to the relative advantages of coil or Pupin loading and uniform or Krarup loading.

To help to settle some of these questions the French Government laid across the English Channel in 1912 a continuously loaded or Krarup cable, which was manufactured by the Telegraph Construction and Maintenance Company.

This Anglo-French uniformly loaded cable has an effective resistance of 8 54 ohms at 1000 frequency, a wire-to-wire capacity of 0·176 mfds., an inductance of 0·0135 henry, and an attenuation constant 0·0185. The total attenuation is 0·39, the value of S/C is 109, and the conductor weighs 300 lb. to the nautical mile.

The copper core in this uniformly loaded cable has twice the weight per mile, viz. 300 lb., of either of the G.P.O. coil-loaded cables, and it has an attenuation constant slightly greater than those of the latter.

The performance of these four cables and the resulting experience as to the cost of maintenance and repairs, will furnish most valuable data to guide the design for future loaded deep-sea telephone cables. Important information has also been gained to enable telephonic engineers to predict the performance of various types of coil-loaded or continuously-loaded cables.

With respect to uniformly-loaded cables it should also be mentioned that Messrs. Henley's Telegraph Works Company have made and laid several uniformly-loaded submarine cables.

In 1907 they supplied 25 kilometres of such 4-core cable insulated with guttapercha to the Royal Danish Administration made to the specification of the late Mr. Krarup. The copper strand was wound over uniformly with a single layer of soft iron wire 0·55 mm. in diameter. This cable was laid in the Faroe Islands early in 1913 ; they made and laid on the Pacific Coast a similarly uniformly loaded cable 31 nautical miles in length. The loading is a single layer of iron wire 0·3 mm. (0·012 in.) in diameter. This last cable is laid from Nanainio in Vancouver's Island to the mainland in British Columbia.

It is said to be the longest uniformly loaded cable yet put down. It is a two circuit cable and is arranged for phantom working. The attenuation constant at 800 frequency is 0·019.

When such loaded cables are connected at their ends to land lines which are not loaded there is a certain loss of amplitude by the reflection which occurs at the junction where the two lines of different impedance join. This effect is exactly similar to that which occurs when a ray of light passes from one medium to another of different refractive index.

We can illustrate this phenomenon by use of our vibrating string. If we put a single heavy bead in the centre of our string and vibrate one end of it with the motor so as to make waves travel along it, you see that strong waves exist on one half of the string but very feeble ones on the other. When the waves meet the load they are partly transmitted and partly reflected, just as when a ray of light meets a sheet of glass. (See II, Fig. 1.)

If we put several beads upon the string at distances comparable with the half wave-length, then reflection takes place at each bead and the waves die away in amplitude very quickly.

If, however, we put beads of different sizes, beginning with a heavy bead in the centre and beads decreasing in mass on either side of it so as to make a tapered load, then the wave started at one end of the coil passes through the loads with much less loss by reflection.

The same thing happens in the case of the passage of a ray of light from one medium to another of different refractive index.

If the refractive index is graduated very evenly there will be refraction of the wave but less reflection.

We meet with identically the same phenomenon in the case of coil loaded telephone cables. It is necessary to taper off the loading by increased spacing or gradual reduction of the inductance of the coils to reduce the losses by reflection when the waves pass from the coil loaded cables to the unloaded terminal wires.

Some interesting curves were given by Dr. Hammond V. Hayes in a paper before the Electrical Congress at St. Louis showing the effect on the attenuation of a telephone line when not loaded, when loaded, and when loaded with taper. These diagrams show well the effect of tapering the loads in the increase of received current which takes place.

Of late years great attention has been paid by advanced telephonists to the theory of the propagation of electric waves and speech-currents along wires. We are no longer content with the crude empirical rules which served the earlier telephonic engineers.

Oliver Heaviside, Pupin, Kennelly, Campbell, Breisig and others have provided the mathematical tools for practical workers to use, and the predetermination of the results of any given cable construction is now a matter of definite calculation, and not, as of old, mere guess-work. When the attenuation constant of a cable is known, the product of the constant and the length in miles of the cable gives us a number called the total attenuation. Intelligible speech cannot be transmitted if the total attenuation of a line exceeds 4·5 or 4·8.

For good speech it should not exceed 2·5 or 3·5.

The extreme limit corresponds to about 47 miles of standard telephone cable. It may be remarked, however, that the total attenuation is not the only criterion of good speech transmission, although it is an important one.

In the case of the Anglo-French loaded (1910) cable the total attenuation for that cable alone is only about 0·33. Hence, as pointed out by Major O'Meara in his valuable paper before the Institution of Electrical Engineers when describing the structure and laying of this cable,* if this cable were continued by heavy overland lines of copper wire weighing 800 lb. to the mile, speech would be possible between London and Astrakhan on the Persian Gulf. Accordingly, if a cable of this type about 200 miles in length were laid under sea, speech would certainly be possible through it. Good speech would be obtained through about 150 miles.

Unless therefore a still greater reduction in attenuation constant is practicable, we are a long way yet from telephony conducted through an Atlantic cable or even telephony through a cable 1000 miles in length.

As regards underground cables or aerial lines the case is very different. It is possible to lay underground dry core or paper insulated cables sheathed in lead, and to load them as heavily as required by coils at intervals of a few miles. Also we are not so much limited in the weight of the copper wire per mile as in the case of submarine cables, whilst the coils can be of any reasonable size.

It is in this manner that great improvements in the speech transmitting power of overhead and underground lines are being made by our British General Post Office and by the postal and telegraphic authorities of other countries, such as France, Germany, Italy, and the United States, by suitable loading.

The longest loaded aerial telephone line at present in use is that from New York to Denver, Colorado, U.S.A., a distance of over 2000 miles. Before the introduction of loaded lines this range was beyond economical telephonic transmission. This loaded line is an aerial line constructed of a copper wire No. 8 B.W.G., weighing 435 lb. to the mile. Similar lines connect all the large cities in the Eastern States. The circuits are complete or double metallic circuits, the lines being twisted to prevent cross talk. They are associated in pairs so that phantom circuits can be formed. The constants of each circuit when unloaded are 4·14 ohms, 0·0091 microfarad and 0·0034 henry, all per loop mile. They are loaded by inserting every 8 miles loading coils each having an effective resistance of 6·5 ohms and an inductance of 0·265 henry.

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* See Major W. A. J. O'Meara, Submarine Cables for Long Distance Telephone Circuits, Journ. Inst. Elect. Eng., vol. xlvii., p. 309, 1911.

The average effective resistance per mile thus becomes 5 ohms and the inductance 0·0365 henry.

A simple calculation then shows that the attenuation constant of this circuit therefore is 0·00125, or about $\frac{1}{800}$. In other words, the wave runs 800 miles before its amplitude is reduced to 36·7 per cent of its initial value.

The total attenuation for 2000 miles is therefore about 2·5, which admits of excellent speech transmission. In fact, so good is the speech transmitted from New York to Denver that on first trying it many persons could not believe they were speaking to correspondents 2000 miles away.

I am indebted for the above information to Mr. J. J. Carty, the able Chief Engineer of the American Telephone and Telegraph Company, under whom the work has been carried out. This remarkable achievement has brought within reach of possibility an additional feat which is a carefully cherished ambition of Mr. Vail, the President of the above company, and of his technical staff, viz. to connect by a loaded telephone line New York and San Francisco, a distance of over 3000 miles, and establish direct speech transmission between the two cities. Another very long loaded aerial line second only in length to the New York and Denver line is that now just completed between Berlin and Rome, a distance of about 2080 kilometres, or 1300 miles. This line runs from Berlin to Frankfort, thence to Bale, and to Brieg at the entrance of the Simplon tunnel. A short length of uniformly loaded telephone cable is laid through the tunnel to Iselle on the Italian side, and an aerial line then proceeds from Iselle to Milan and thence to Rome. The conductor consists of a pair of phosphor bronze wires 4·5 mm. in diameter, weighing about 500 lb. per mile. The lead and return wires are kept 42 cm. apart, and loading coils inserted every 10 kilometres or 6¼ miles. These coils have an effective resistance of 5 ohms and an inductance of 0·2 henry. The line has thus an effective resistance of 2·9 ohms, inductance 0·022 henry, and capacity of 0·0056 microfarad per kilometre. The attenuation constant is thus about 0·0011 per kilometre, and the whole line has a total attenuation of about 2·2. There is no doubt excellent speech transmission will therefore be obtained. I am indebted for the above information to Dr. G. di Pirro, who has been in control of the scientific side of the work for the Italian Government, and to Dr. F. Breisig, who in like manner is the chief of the telegraphic and telephonic research department of the Imperial German Post Office.

Then, turning to our own country, it is possible by the kindness of Mr. W. Slingo, the present Engineer-in-Chief of the British General Post Office, to put you in possession of information as to the present state of loaded telephone lines in Great Britain. The Post Office has now over 30,000 miles of loaded telephone circuits, aerial and cable, using 12,448 loading coils. There are also 45,645 miles of circuits being loaded with 17,767 coils. Hence the G.P.O. will soon have over 75,000 miles of loaded telephone lines in Great Britain containing 30,000 loading coils, not including the submarine cables above mentioned. The longest British aerial loaded lines are the two Trunk circuits running from London to Leeds, the details of which are given in Table III, and taken by permission from a recent paper by Mr. J. G. Hill,* one of the Post Office Engineers, who has given in that paper full information of the experiments conducted by the G.P.O. on these lines.

The details of the loading of these long aerial lines, including the Berlin-Frankfurt line in Germany, are given in Table III.

As regards loaded underground cables, Mr. Slingo informs me that the longest working underground loaded cable in Great Britain is that from Hull to Newcastle via Leeds, which is 154 miles long. The conductors of the Hull-Leeds and Leeds-Stockton sections weigh 70 lb. per mile. They are laid up in multiple twin formation.

The inductance added is 54 millihenries per mile, and wire-to-wire capacity is 0·065 microfarads per mile. The loading coils are spaced 2·5 miles apart. The conductors of the Stockton-Newcastle section (34 miles) weigh 100 lb. per mile. The total attenuation over the whole length is 2·32.

TABLE III.—Loaded Aerial Land Lines.

All values are per mile or per kilometre at 800 frequency.

Line . .	New York and Denver	Berlin and Rome	Berlin and Frankfurt	London (St. Albans) and Leeds Trunks	
				No. 6	No. 7
Length	2000 miles	2082 km.	584 km.	189 miles	189 miles
Coil Spacing . . .	8 miles	10 km.	5 km.	8 miles	12 miles
Coil Resistance . .	6·5 ohms	5	8·7	6·6	4·0
Total Resistance . .	4·95 ohms	2·9	11·18	7·58	7·08
Capacity in mfds. .	0·0091	0·0056	0·0055	0·0098	0·0098
Inductance in henrys	0·0365	0·022	0·0461	0·037	0·0173
Attenuation Constant	0·0013	0·0011	0·0019	0·00283	0·00372
Total Attenuation .	2·6	2·2	1·12	0·55	0·72
Conductor	Copper	Phosphor Bronze	Bronze	Copper	Copper
Weight or Size . .	435 lb. to mile	4·5 mm. diam.	2·5 mm. diam.	300 lb. to mile	300 lb. to mile

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* Mr. J. G. Hill, "The Loading of Aerial Lines and the Constants of Loaded Circuits." *The Electrician*, vol. lxxii., p. 602, January 16, 1914.

The G.P.O. are now also laying an underground air-space paper coil-loaded cable 3 in. in diameter containing fifty circuits, between London and Liverpool via Birmingham, a total distance of 200 miles. Mr. Slingo informs me this cable contains the following loops :—

2 circuits of 300 lb. per mile.
14 circuits of 200 lb. per mile.
12 circuits of 150 lb. per mile.
24 circuits of 100 lb. per mile.

These circuits are double metallic circuits arranged as multiple-twin circuits in a lead sheath. The wire-to-wire capacity of all loops is 0·065 microfarad per mile. These cables are all loaded with coils spaced 2·5 miles apart, and the added inductance is 0·055 henrys per mile of loop. The ratio S/C is 20. The thicker wires have coils of lower resistance than the thinner wires. This cable, when completed, will afford improved telephonic communication between London, Birmingham, and Liverpool, which will be quite independent of storm or weather.

In the United States a still longer coil loaded telephone cable has been laid between Boston and Washington, passing through New York, Philadelphia, and Baltimore, a total of 475 miles. This cable is a multiple cable, and includes seven circuits of No. 10 wires, which can be phantomized in pairs. The Pupin coils are inserted every 1·4 mile. The phantom circuits between New York and Washington have a transmission efficiency equal to thirteen miles of standard cable, and between Boston and Washington equal to twenty-six miles.

Up to 1912 in the United States, telephone circuits of a total length of 103,000 miles had been equipped with loading coils, and a great improvement effected in the range and transmission efficiency.

To sum up: we can say from an inspection of the figures given above that it is now possible by suitably loading a telephone line to increase its telephonic range two or three times. The chief difficulty which is encountered in the loading of aerial lines is the variable leakance over the insulators, which may under some conditions reduce the effect of loading to nil. Broadly speaking, however, the introduction of loading has made possible telephony by overhead lines to a distance of 2000 miles, and this may, perhaps, in the near future be increased to 3000 miles. It has rendered possible commercial telephony by underground cables up to a distance of about 400 miles, and this may be increased to 500 miles before long. It has enabled submarine telephone cables to be laid for distances of sixty to seventy miles, and it is within possibility to increase this to 150, or perhaps 200 miles.

We may ask in conclusion what prospect exists of abolishing the line altogether, with its troublesome attenuation and distortion of wave form.

The treatment of this subject demands a lecture in itself and not merely a few concluding sentences at the end of one. Suffice it to say that telephony conducted without interconnecting wires has been achieved over distances of several hundred miles as an experimental feat, but cannot yet be said to have entered into the field of practical everyday service. A brief outline of the method is as follows.

At the transmitting station we establish all the arrangements now used for producing continuous long electric waves of rather less than 10 miles in wave length, and preferably about 4 or 5 miles. These waves are radiated from an antenna wire and must be perfectly continuous, that is, emitted without break or interruption; or, at least, if they are produced by spark discharges, the spark frequency must be about 20,000 per second. At the receiving station there are the usual arrangements for receiving by telephone signals as in radio-telegraphy.

The only difference is that at the sending station in the base of the antenna is inserted a microphone of such current-carrying capacity as to bear 5 to 20 amperes or more passing through it. This microphone varies the resistance in the antenna circuit when speech is made to it. This in turn causes a fluctuation in the amplitude of the stream of continuous electric waves being emitted. It, so to speak, makes waves on waves, and at the receiving station the telephone is influenced by these secondary and slower modulations of wave amplitudes.

The frequency of the steady stream of waves emitted by the sending station must be such that it is above the limits of audition, that is, more than 20,000 per second, and preferably about 40,000. An unbroken wave train of this frequency cannot affect the telephone directly. If, however, the amplitude of the electric waves radiated is varied in accordance with the lower frequency of speech, these undulations of amplitude in the high frequency wave do affect the telephone in the receiver, and the speech uttered to the transmitting microphone is heard.

The continuous wave-train can be produced either by a high frequency alternator, such as that of Goldschmidt, or by the continuous-wave disc generator of Mr. Marconi, or by some form of Poulsen or Moretti arc generator.

The chief difficulty is that of constructing a microphone which shall be able to carry the large oscillatory currents which flow up and down the antenna.

Professor Majorana and Dr. Vanni in Italy have devised ingenious liquid microphones. A multiple carbon microphone is commonly used, and Dubilier has devised a water-cooled arrangement called a relay microphone.

The Vanni microphone is constructed as follows:—A jet of water, which may be made slightly conducting by the addition of acid or salts, emerges from an ebonite mouthpiece and falls between two inclined metal plates A and B. (See Fig. 9.) One of these plates is fixed

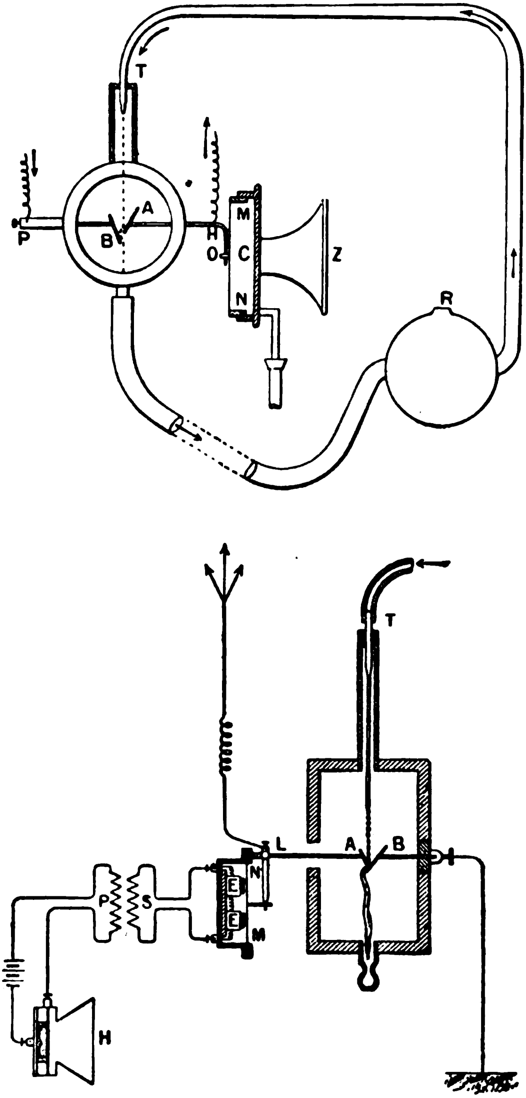
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FIG. 9.—VANNI'S LIQUID MICROPHONE.

and the other is connected to the diaphragm of a telephone. The liquid falls on to one plate and bounces off on to the other. and makes an electrical connection between them. The movement of the diaphragm sets up oscillations in the liquid column and also varies the distance between the plates, and therefore alters the thickness of the connecting film of liquid, and hence its electrical resistance. If one of these plates is connected to a transmitting antenna and the other to the earth we have the means of varying the resistance in the antenna circuit, and hence the amplitude of the emitted electric waves so as to modulate them in accordance with the wave form of the speaking voice.

At the receiving end the arrangements are not different from those used in wireless telegraphy employing telephonic reception. The receiving antenna is coupled to a condenser circuit, which has in it a crystal or Fleming valve receiver and a telephone.

The valve rectifies the continuous waves, but the uniform current produced does not *per se* affect the telephone. If, however, modulations of amplitude are produced in the radiated waves by speaking to the transmitter at the sending station, the receiving telephone is affected by these variations of amplitude, and responds only to the waves on the waves or variations in the amplitude of the continuous waves. The receiving operator then hears the words uttered to the distant transmitter.

The limiting factor is therefore the current-carrying power of the speaking microphone or transmitter.

Using his liquid transmitter, Professor Vanni has achieved the feat of speaking successfully from Rome to the Island of Ponza (120 km.), to Maddalena (260 km.), to Palermo (420 km.), to Vittoria (600 km.), and finally to Tripoli (1000 km.).

The timbre or quality of the voice is said to be reproduced with great accuracy. Since all electric waves travel at the same speed through space and attenuate proportionately, there is little or none of that distortion to which we have referred in the case of telephony along wires.

Time will not permit of reference to the details of all the experiments on this subject made by Fessenden and others in the United States, Poulsen in Denmark, Ruhmer, Goldschmidt, and the Telefunken Company in Germany, M.M. Colin and Jeance in France, and Marconi, Majorana and Vanni in Italy, and Ditcham in England. Suffice it to say that this lineless telephony has been successfully conducted up to 500 or 600 miles distance.

The chief difficulty in obtaining greater ranges is that of constructing a voice-varied resistance or so-called microphone transmitter which shall be able to carry large currents up to 50 or 100 amperes and vary these currents faithfully in accordance with the wave form of the speaking voice.

Wireless telephony across the Atlantic will be achieved when it is thus possible to modulate power to the extent of 100 or 150 kilowatts.

A very promising method of varying the amplitude of the emitted waves appears to be by the insertion of a speaking microphone in the exciting circuit of a high frequency alternator, such as that of Goldschmidt. It will be thus possible to modulate in accordance with the wave form of articulate speech a large antenna current without actually passing that current through any form of microphone. The modulated current is then merely a small current of a few amperes.

Summing up the position, we can say that lineless telephony up to 500 or 600 miles is now possible, and there appears to be no insuperable obstacle to prevent it from being conducted across the Atlantic ocean. If this were ever done, and the New York to San Francisco line telephony accomplished, it might be possible, with one repetition of a message, to speak articulately from London to places in California. Long before this, however, is achieved, wireless telephony will doubtless have come into use for moderate distances between ships and ship and shore. Mr. Marconi has recently directed much attention to this subject with the object of improving the speech transmission and simplifying the apparatus. He has invented for this purpose new and effective transmitters and receivers, and has conducted demonstrations between ships of the Italian Navy.

It would be an advantage to possess a simple and easily worked form of wireless telephone apparatus that could be installed on ships for intercommunication, so that ships' officers and others who have not had the special training necessary to read Morse signals by ear could on necessity communicate with distant vessels by actual speech, and call for help in the hour of need.

From this brief statement on a small portion of this subject it will be seen that we have by no means come to the end of invention in connection with telephony.

Achievement has been great since Alexander Graham Bell gave us 38 years ago the exquisitely simple receiver which bears his name, and at once made possible commercial telephony, when coupled with the invention of the microphone and carbon transmitter made by Hughes and Edison and other great pioneers.

Let no one, however, think that the field is exhausted. The chief problems have been solved, but those who come to the task with adequate powers of scientific investigation or original invention, still find in the various departments of practical telephony an abundant opportunity to enlarge our knowledge or improve a means of communication, which, even if it contributes at times to test our patience, has yet become so essential to our modern life.

[J. A. F.]

GENERAL MONTHLY MEETING,

Monday, April 6, 1914.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Miss A. R. Atkinson,
Lady Esmonde,
Mrs. H. J. F. Simson,
Francis William Goodenough, Esq.

were elected Members of the Royal Institution.

The Chairman announced that the Managers, at their Meeting held this day, had, in accordance with the Actonian Trust Deed, awarded the Actonian Prize of One Hundred Guineas to Professor Charles Scott Sherrington, M.A. M.D. D.Sc. LL.D. F.R.S., Waynflete Professor of Physiology in the University of Oxford, for his work entitled "The Integrative Action of the Nervous System."

The following Lecture Arrangements After Easter 1914 were announced:—

Walter Wahl, Esq., Ph.D. (Helsingfors) *M.R.I.* Two Lectures on Problems of Physical Chemistry: 1. Study of Matter at High Pressures; 2. Structure of Matter at Low Temperatures. (Experimentally Illustrated.) On *Tuesdays*, April 21, 28.

Professor William Bateson, M.A. D.Sc. F.R.S., Fullerian Professor of Physiology, Royal Institution. Two Lectures on 1. Double Flowers; 2. The Present State of Evolutionary Theory. On *Tuesdays*, May 5, 12.

Professor D'Arcy W. Thompson, C.B. D.Litt. M.A., Professor of Natural History, Dundee. Two Lectures on Natural History in the Classics; 1. The Natural History of the Poets: Homer, Virgil and Aristophanes; 2. The Natural History of Aristotle and of Pliny. On *Tuesdays*, May 19, 26.

Professor A. Fowler, F.R.S., Sec. R.A.S. Astrophysics Department, Imperial College of Science and Technology. Two Lectures on Celestial Spectroscopy: Experimental Investigations in connection with the Spectra of the Sun, Stars and Comets. On *Tuesdays*, June 2, 9.

The Very Rev. William Ralph Inge, D.D., Dean of St. Paul's. Three Lectures on The Last Chapter of Greek Philosophy: Plotinus as Philosopher, Religious Teacher and Mystic. On *Thursdays*, April 30, May 7, and *Monday*, May 11.

Professor Svante Arrhenius, D.S.C. Hon. F.R.S. Hon. F.C.S. *Hon. M.R.I.*, Director, Nobel Institute of Physical Chemistry. Three Lectures on Identity of Laws: in General: and Biological Chemistry. (The Tyndall Lectures) On *Thursdays*, May 14, 21, 28.

Professor Silvanus P. Thompson, B.A. D.Sc. LL.D. F.R.S. *M.R.I.*, Principal and Professor of Physics, City and Guilds Technical College, Finsbury. Two Lectures on Faraday and the Foundations of Electrical Engineering. On *Thursdays*, June 4, 11.

Thomas E. Stanton, Esq., D.Sc. M.Inst.C.E., Superintendent, Engineering Department, National Physical Laboratory. Two Lectures on Similarity of Motion in Fluids: 1. The Theory of Similarity of Motion in Fluids and the Experimental Proof of its Existence; 2. The General Law of Surface Friction in Fluid Motion. On *Saturdays*, April 25, May 2.

Professor Charles J. Patten, M.A. M.D. Sc.D. F.R.A.I., Professor of Anatomy, Sheffield. Two Lectures on Bird-Migration. On *Saturdays*, May 9, 16.

Professor John W. Gregory, D.Sc. F.R.S., Professor of Geology, Glasgow. Two Lectures on Fiords and their Origin: 1. The Nature and Distribution of Fiords; 2. Fiords and Earth Movements. On *Saturdays*, May 23, 30.

Sigismund Goetze, Esq., *M.R.I.* Two Lectures on Studies on Expression in Art: 1. Origin and Development; 2. Right Expression in Modern Conditions. On *Saturdays*, June 6, 13.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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Athenæum for March 1914. 4to.
Author for March–April, 1914. 8vo.
Canadian Mail for March 1914. 4to.
Chemical News for March 1914. 4to.
Chemist and Druggist for March 1914. 8vo.
Church Gazette for March 1914. 8vo.
Concrete for March–April, 1914. 8vo.
Dyer and Calico Printer for March 1914. 4to.
Electrical Engineering for March 1914. 4to.
Electrical Industries for March 1914. 4to.
Electrical Review for March 1914. 4to.
Electrical Times for March 1914. 4to.
Electricity for March 1914. 8vo.
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Engineering for March 1914. fol.
Ferro-Concrete for March 1914. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 24, 1914.

Alexander Siemens, M.Inst.C.E. M.I.E.E., Secretary and Vice-President, in the Chair.

Frank Watson Dyson, M.A. LL.D. F.R.S. F.R.A.S.
(The Astronomer Royal).

The Stars around the North Pole.

1. My purpose to-night is to put before you some of the methods employed to determine the distances of the stars. A knowledge of the distances of the stars from us is of fundamental importance in any attempt to describe the stellar universe. It is required before answers can be given to questions on the average distances of stars from one another, their brightness compared with the sun, and the extent to which they reach in space. There are not more than 100 or 150 stars whose distances have been measured with any degree of accuracy. Although this number is being steadily increased, it is only the stars which are comparatively near to the sun which can be treated individually. For the greater number we have to be content with average values, which apply to groups of stars, and I propose to show you how these may be obtained.

2. A map or a photograph of the stars gives us only their bearings, that is to say their directions as seen from the earth. It gives no information whatever about their distances. One star may be a hundred times as far away as its neighbour on the map. But if two maps are made, separated by a sufficient interval of time, some differences will be found in the relative positions of the stars. These indicate movements either of the stars themselves or of the point from which they are viewed. But the movements which are observed are merely changes of angular position. We cannot tell directly from them either the actual velocities or distances of the stars, but only the ratio between these quantities. It is, however, from the geometrical study of these small angular motions, supplemented by the information obtained from the spectroscope as to the velocities of stars in the line of sight, that our knowledge of their distances is derived.

3. The problem is in many ways analogous to one which has been completely solved. In the early days of astronomy the movements of the wandering stars or planets were noted. The essential characteristics of the movements were next embodied in geometrical formulæ by the Greeks. In the course of time Copernicus showed

that these formulæ could be most simply interpreted on the assumption that the earth revolved round the sun. His purely geometrical arguments were, it is true, powerfully reinforced by the revelations of Galileo's telescope. Nevertheless, the planetary system as formulated by Copernicus and Kepler resulted from the observation of the angular movements of the planets, and the attempt to give them the simplest possible geometrical interpretation.

Further study of the planetary system has been guided and controlled by the law of gravitation. But the observational data on which our very complete knowledge of the solar system is based—the distances, sizes, and movements of all its members—are a long series of measures of the angular movements as seen from the earth. Linear measurements are only required to obtain the form and dimensions of the earth itself, and thus supply a base line to determine the scale of the system.

4. The fixed stars present us with a very similar problem. From the study of their small angular movements, supplemented by spectroscopic observations, it is required to construct as far as possible a model of the stellar universe. Such a model would give for each star—

(*a*) Its actual position in space, measured along three axes with the sun as origin.

(*b*) The velocity in kilometres per second in each of these directions.

(*c*) The brightness or luminosity, taking the sun as unit.

(*d*) The mass.

(*e*) The size.

(*f*) The physical and chemical constitution.

Of these elements, the mass is at present only determinable for double stars, and the size for eclipsing variables. The physical and chemical constitution are known from spectroscopic observations for a considerable number of stars. But the distance and absolute brightness can be found only for a limited number of the nearer stars. Average results can, however, be obtained for the more distant stars which tell us—

(*a*) The number within certain limits of distance from the sun.

(*b*) The mean velocities of these stars, and what percentage are moving with given velocities, say, for example, between 10 and 20 kilometres per second.

(*c*) Whether these velocities are irregular or show anything in the nature of streaming in particular directions.

(*d*) What proportion of the stars are comparable with the sun in intrinsic brightness, and what proportion are ten times or one-tenth as bright; and so on.

Such a description of the stellar system is, to a large extent, within the powers of astronomers, and we nurse the perhaps extravagant hope that generalizations will be discovered which will lead to the formulation of dynamical laws on the constitution of the stellar universe.

5. I have chosen a small area round the pole as a sample, because this part of the sky has been observed more fully than any other of equal extent. It forms a small cap extending to a distance of 9° from the pole, and covering about $\frac{1}{160}$ of the whole sky. In the years 1855–6 Carrington, an English amateur astronomer, well known from his observations of sun-spots, using a very small transit instrument, observed the positions of all the stars in this part of the sky from the brightest down to very faint stars between the tenth and eleventh magnitudes. [An eleventh magnitude star is exactly 100 times as faint as one of the sixth magnitude, and a sixth magnitude star is the faintest star which can be seen with the naked eye.] He thus constructed a catalogue, giving with great accuracy the positions of 3700 stars for the year 1855. About the year 1900 these stars were re-observed at Greenwich by a combination of visual and photographic observations. By comparison with the positions as given in Carrington's catalogue, the angular movement of each of these 3700 stars in 45 years is determined. These angular movements, or proper motions as they are technically called, are the data available for obtaining the actual positions and movements of the stars in space. We have to solve the geometrical problem of making these stars stand out in three dimensions, so that we may see them as we see a picture in a stereoscope.

6. Now the proper motions of stars are very small. The star of largest proper motion moves only 870″ a century. An idea of the smallness of this motion may be obtained from the fact that it would take two centuries to move a distance equal to the apparent diameter of the sun or moon. It would take four centuries to move across the small cluster of the Pleiades. There is no star among those near the North Pole with a proper motion so great as one-tenth of this. The following table gives an abstract of the proper motions of the 3700 stars under consideration:—

TABLE I.

Limits of Proper Motion	No. of Stars
> 40″ a century . . .	2
20″–40″ ,, ...	39
10″–20″ ,, ...	134
5″–10″ ,, ...	574
3″–5″ ,, ...	977
0″–3″ ,, ...	2000

It is clear that the stars with large proper motions must either be moving fast or must be comparatively near. These are the alternatives, but for an individual star it is impossible to decide between them.

7. The table given in § 6 shows how largely the proper motions of stars vary in amount. They differ just as widely in direction. Some signs of regularity in the directions were first detected by Sir William Herschel, who found that the movement of seven quick-moving stars, situated in different parts of the sky, were approximately directed to one point. He observed that this would result if the proper motions arose not from the movement of the stars themselves but from that of the point of observation in an opposite direction, and concluded that the solar system was moving towards a point in the constellation Hercules. This conclusion was not universally admitted for some time, but researches by Argelander, Airy, Bessel, and others demonstrated a regular drift among the stars, such as would arise if on their otherwise irregular movements were superposed this common motion. A large number of researches have been made on the exact direction of the sun's motion, and it is now established with some certainty that it is towards a point in right ascension 18^h and declination 35° N., not far in direction from the bright star Vega. The speed of the sun's motion through space has been determined by spectroscopic observations. On the average, stars near Vega appear to be approaching us, stars in the opposite direction to be receding from us. In this way, Prof. Campbell has found from the observed velocities of 1500 stars that the solar system is moving at the rate of 19·5 kilometres a second.

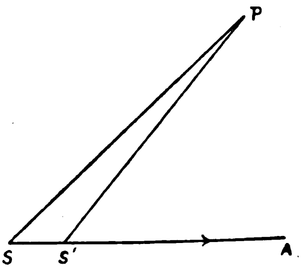


Fig. 1.

8. The fact that the sun is moving with a velocity of 19·5 kilometres a second in a known direction supplies us immediately with a means of determining the average distances of groups of stars. This velocity carries the sun forward in a century a distance equal to 412 times the sun's distance from the earth. If at the beginning of the century the sun is at S, and at the end has moved to S^1, the angular distance of a star situated at P and having no motion of its own will have increased from ASP to AS^1P. The difference of these angles, which is the proper motion of the star, is SPS^1. Thus in the triangle SPS^1 all the angles are known and the small base SS^1; therefore the side SP—the distance of the star from the sun—can be deduced. The principle is the same as that used in ordinary surveying, the base line being provided by the motion of the sun. But we cannot say that any individual star is at rest. If, however, we take a sufficiently large group of stars it is legitimate to suppose that in the average the peculiar movements of the separate

stars are eliminated, and that the mean distance of the group can be correctly inferred.

9. During the last twenty or thirty years the proper motions of many stars have been determined by comparison of modern with earlier observations. Particularly, the reduction by Dr. Auwers of Bradley's observations made in 1755, led to the accurate determination of the angular movements of the brighter stars. The proper motions of fainter stars have been found by comparison of recent observations with those made in the first half of the nineteenth century. These have all been utilized to determine the direction and angular amount of the drift produced in the stars by the motion of the solar system through space. The results were very puzzling, because different mathematical methods and different groups of stars gave widely different directions for the solar motion. The cause was discovered about ten years ago by Professor Kapteyn, who found in the proper motions of the stars another indication of regularity—or perhaps it might be called a systematic irregularity—smaller than the one discovered by Herschel, but unmistakable when once pointed out. He interpreted these systematic irregularities to mean that the stars are divisible into two groups, streaming through one another in opposite directions in space. Prof. Kapteyn's discovery has been submitted to mathematical analysis by Prof. Eddington and Prof. Schwarzschild. Their researches have illuminated the whole subject of stellar motions, and, though they are not in entire agreement, they leave no doubt of the existence of a preferential movement among the stars towards the north part of Orion, and the diametrically opposite direction in the constellation of the Serpent.

10. We must next consider the *motus peculiares*—the irregular movements of the stars themselves. From observations of the velocities of stars in the line of sight, especially from those made at the Lick Observatory under Prof. Campbell's direction, it is known that a few stars are moving with great velocity, such as 100 kilometres a second, while others are moving very slowly. The following analysis of Campbell's results for one class of stars (Table II.), those of spectral type A (taken from a paper by Prof. Eddington), shows the proportion of slow-moving, moderate, and quick-moving stars.

Comparison with the third column of the table shows that the velocities are distributed in accordance with the law of errors. This law is identical with that found by Maxwell for the velocities of the molecules of a gas. In the case of a gas, this distribution of velocities results from the frequent collisions. For the stars there is no evidence that it has resulted from their interaction. It must be regarded as an observational fact that the distribution of the velocities of the stars is stated concisely by this simple mathematical formula.

11. The three movements—the movement of the solar system in space, the streaming of the stars, and their irregular movements—are

TABLE II.—NUMBER OF STARS OF DIFFERENT VELOCITIES.

Velocities	Number observed	Number given by Error Law
0–5 kil/sec.	55	53·4
5–10 ,,	47	46·2
10–16 ,,	30	38·3
16–25 ,,	30	27·4
25–40 ,,	10	6·7
> 40 ,,	0	0

all shown in their proper motions. The diagram (taken from a paper by Mr. Jones—Monthly Notices of the R.A.S., vol. lxxiv, p. 196), exhibits the proper motions of some of the brighter stars situated near the North Pole. If the stars had all been placed at O, they would in a century have spread out as shown in the diagram.

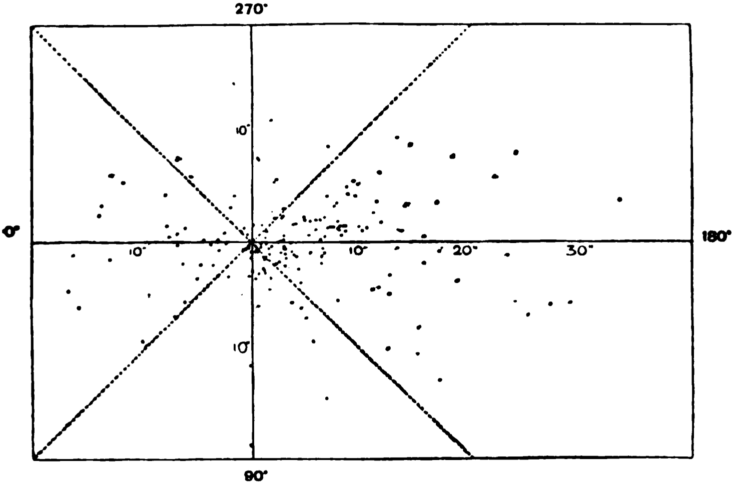


FIG. 2.—PROPER MOTIONS OF GROUP A5–F9.

This spreading out has been caused by :—

(*a*) The solar motion, which has shifted the centre of gravity of the swarm towards 180°.

(*b*) The peculiar motions of the stars themselves, which have spread them out in the directions towards 90° and 270°.

(*c*) The streaming in the direction 0° to 180° combined with the peculiar motions, which have made the spreading out much greater in this than in the perpendicular direction. In this part of the sky the streaming happens to be in the direction of and opposite to the solar motion.

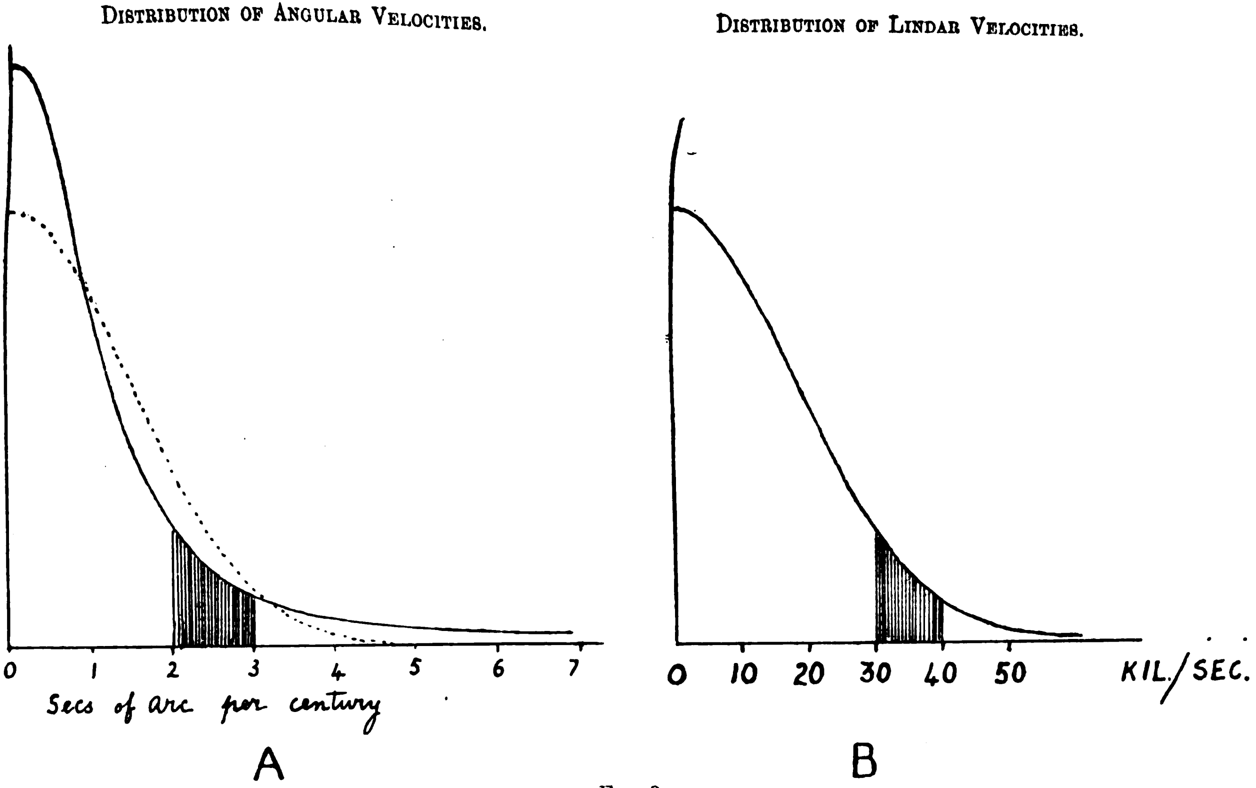
12. Let us now consider the proper motions of the 3700 stars observed by Carrington in the light of these discoveries. The shift of the centre of gravity caused by the solar motion is 1″·44 a century. As we know how far the sun has moved in a century, this gives the average distance of these stars as forty-eight million times the distance of the sun from the earth. Turning now to the proper motions in a direction perpendicular to that of the sun's motion which arise from the *motus peculiares* of the stars themselves. Counting these cross proper motions, it is found that :—

16	are greater than	15″·0			a century
25	lie between	10″·0	and	15″·0	,,
33	,,	8″·0	,,	10″·0	,,
66	,,	6″·0	,,	8″·0	,,
191	,,	4″·0	,,	6″·0	,,
873	,,	2″·0	,,	4″·0	,,
2504	,,	0″·0	,,	2″·0	,,

When suitable allowance is made for the accidental error in these observations, it is found that the number less than any given amount τ can be represented by an algebraical formula

$$3700 \frac{\tau}{\sqrt{(\tau^2 + 1''\cdot 44^2)}}$$

13. Now suppose that all these stars were actually moving with the same velocity, say 10 kilometres a second, then their distance could be calculated : those with proper motion 1″ a century being 40 million times as distant as the sun, those with proper motion 2″ a century 20 million times, those of 4″ a century 10 million times, and so on, the larger the proper motion the nearer the star to us. This is only an illustration, for the velocities of the stars are not all the same, but are distributed according to the law of errors. Looking at the question rather differently, let us start with the assumption that the actual accidental velocities of the stars in one co-ordinate are distributed according to the law of errors. If the stars were all at the same distance, their proper motions, found by dividing their velocities by this distance, would also be distributed according to the law of errors. But, actually, they are distributed according to a different law. The diagram A (Fig. 3) gives the proportion of stars with proper motions between given limits (the area of the shaded portion gives the proportion between 2″ and 3″ a century) ; the diagram B gives the proportion of stars with velocities between given limits (those between 30 kil/sec. and 40 kil/sec. are given by



DISTRIBUTION OF ANGULAR VELOCITIES.

DISTRIBUTION OF LINDAR VELOCITIES.

FIG. 3.

the shaded portion).* How must the stars be distributed in distance to make these laws harmonize? This is a mathematical problem which is solved fairly easily, and the answer is that the stars must be distributed in distance as shown graphically in Fig. 4.

14. In the diagram distances are measured horizontally, the unit of distance being that at which a star's parallax equals 1″. This distance is 206,265 times the distance of the sun, and is a convenient unit for purposes of calculation. It is convenient to have a name for the unit, and I shall adopt *parsec*, a composite word suggested to me by Prof. Turner. With this unit, a distance of 100 in the diagram denotes 20 million times the distance of the sun from the earth. It may be verified by the diagram (Fig. 4) that:—

6	per cent of the stars are between	0 and 100	parsecs
15	,, ,, ,,	100 ,, 200	,,
40	,, ,, ,,	200 ,, 400	,,
33	,, ,, ,,	400 ,, 700	,,
6	,, ,, greater than	700	,,

It follows that 88 per cent of the stars in Carrington's catalogue, that is 88 per cent of all stars brighter than about $10^{m} \cdot 5$, lie between 20 and 150 million times the distance of the sun from the earth. This law of the distribution of the stars is at first sight rather surprising. It must be remembered that the only stars at a great distance which are included are those which are intrinsically very bright, and these form only a small proportion of all the stars. Prof. Eddington has found that a similar law holds for stars brighter than $6^{m} \cdot 0$.

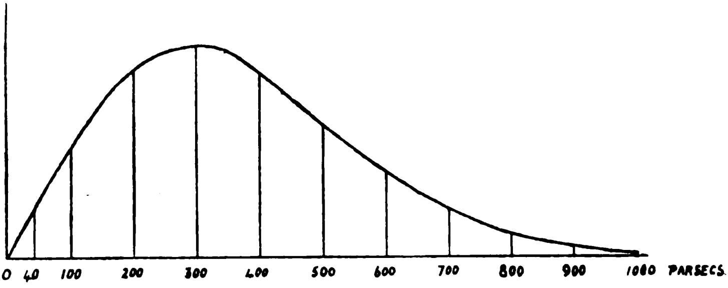


FIG. 4.

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* The number of stars out of a total N with velocities between v and $v + dv$ is $N \frac{h}{\sqrt{\pi}} e^{-h^2} dv$; the number of proper motions between τ and $\tau + d\tau$ is $N \frac{d\tau}{a} \left(1 + \frac{\tau^2}{a^2}\right)^{-\frac{3}{2}}$. The solution of an integral equation, gives for the number between r and $r + dr$, $N\, 2 a^2 h^2 r\, e^{-a^2 h^2 r^2} dr$.

15. Having found the law of distribution of the distances of these stars, it is not difficult to determine something about their absolute luminosities—i.e. how they would compare with the sun in brightness if placed at an equal distance from us.

If the sun were at a distance of one parsec, it would appear as a bright star brighter than the first magnitude, actually of magnitude 0·5. If at a distance of 100 parsecs, its magnitude would be 10·5. Now all the stars in Carrington's catalogue may be taken as brighter than 10^{m}·5. Thus at least 95 per cent of the 3700 stars of this catalogue are intrinsically brighter than the sun, and at least

80	per cent are	4	times as bright
40	,,	16	,,
8	,,	50	,,

We may conclude that the great majority of the stars brighter than 10^{m}·5 are intrinsically brighter than the sun, and a considerable proportion very much brighter.

16. The distribution of bright and faint stars in a given volume of space is quite different, and contains a much larger proportion of faint stars. If we make the assumptions that the density of the stars and the proportions of bright and faint ones are the same at the different distances from the sun within which these Carrington stars are situated, it is possible to find the actual number of stars of different luminosities in a given volume of space. In a sphere with radius 100 parsecs, or 20 million times the distance of the earth from the sun, there are at least—

24	which are	100	times	as luminous	as the sun
340	,,	50	,,	,,	,,
1530	,,	25	,,	,,	,,
4840	,,	10	,,	,,	,,
23,200	,,	1	,,	,,	,,
93,300	,,	$\frac{1}{10}$	,,	,,	,,

The data only admit of a rough determination of the number of very faint stars, and the number of very bright ones. The figures give a general indication of the density of the stars in space and of their intrinsic brightness, and serve to draw attention to the fact that there are many stars much less luminous than the sun, and a certain proportion very much more luminous.

17. Our conclusions, up to this point, have been based entirely on a consideration of the proper motions of the stars, irrespective of whether they are bright or faint, provided only that they are sufficiently bright to have been observed by Carrington. But as the apparent magnitude of a star depends on its distance as well as on its intrinsic brightness, we naturally expect some assistance, in assigning the distances of these stars, from their magnitudes. The brightest star in this small area round the pole is Polaris—the pole star—whose

magnitude is 2. It may be remarked incidentally that the distance of the pole star has been actually measured; it is 20 parsecs or 4 million times the distance of the sun from the earth, and if it were at the same distance as the sun it would appear to be 100 times as bright. Then there are about 20 stars which are visible to the naked eye. The following table gives the actual number of stars of different magnitudes (photographic).

TABLE III.—NUMBER OF STARS OF DIFFERENT MAGNITUDES (PHOT.)

	m m	
Brighter than . .	7·0	61 stars
Between	7·0 and 8·0	124 „
„	8·0 „ 9·0	397 „
„	9·0 „ 10·0	998 „
Fainter than . .	10·0	2140 „

18. Then again, we may separate the stars into groups according to the physical characteristics revealed by the spectroscope. The researches of Kapteyn, Campbell, Boss and others have shown, at any rate for the brighter stars, remarkable relationships between the distances and velocities of the stars, and the type of spectrum which they manifest. It is therefore desirable to examine the proper motions of stars of different spectral types separately. The spectra of many thousands of stars have been determined at Harvard College, under Prof. Pickering's direction, by Miss Cannon. The different classes are indicated in the Harvard Classification by the letters B, A, F, G, K, M, with further subdivisions. The B stars are characterized by the presence of helium; the A stars by series of broad hydrogen lines. In the F stars the hydrogen lines are thinner and fine metallic lines are shown. The G stars are very like the sun, full of metallic lines, and with broad lines due to calcium. In the K stars the two calcium lines are still broader and there are many fine metallic lines. The M stars are characterized by broad absorption bands. A further characteristic is that, taking the stars in the order M to B, their spectra extend more and more towards the violet. This classification is a regular progression and places the stars in the order of their temperatures—the B stars are the bluest and hottest, and the M stars the reddest and coolest.

19. The character of the spectra of about 800 of the stars in Carrington's catalogue is given by the Harvard observations. For the fainter stars the spectra have not been determined, but they can be inferred in another way. As the blue stars are more active photographically than the red stars, if a red and a blue star have the same visual magnitude, the magnitudes estimated from the images on a photograph will differ considerably, and this difference is an index of the colour, and thus of the type of spectrum. This is

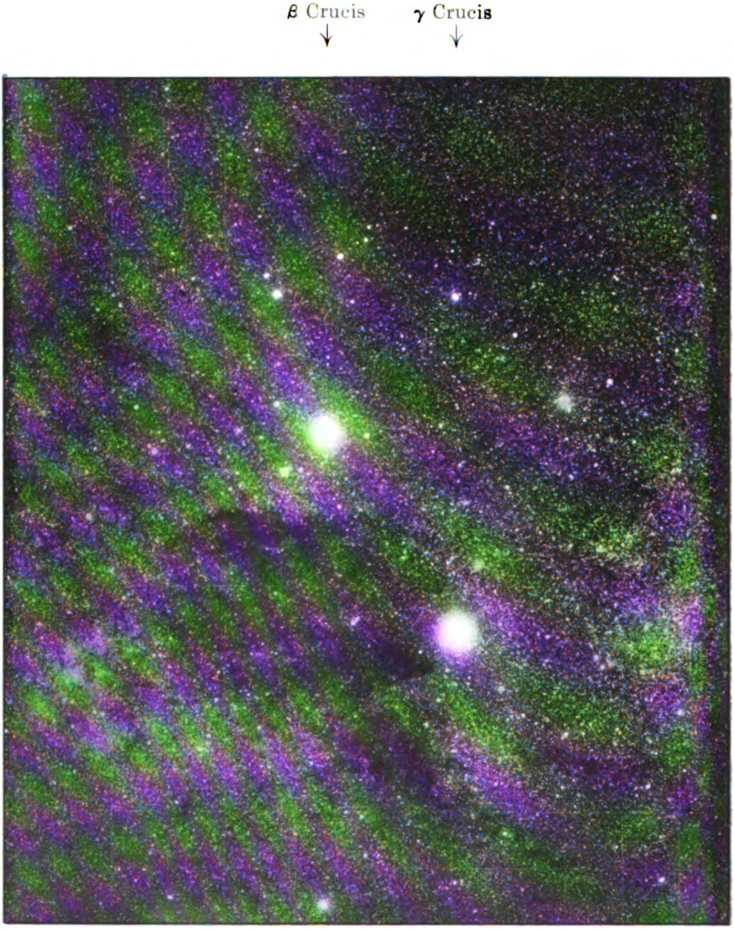
β Crucis
γ Crucis
γ Crucis
β Crucis

Fig. 5.—The Southern Cross and the Coal Sack.

well shown in the photograph of the Southern Cross (Fig. 5), where β and γ have the same visual magnitudes, but γ is a red star. Now the visual magnitudes of most of these faint stars have been very accurately determined at Potsdam by Messrs. Müller and Kron (and have been kindly communicated to me in manuscript), and the photographic magnitudes have been determined at Greenwich. The differences have been taken between the photographic and visual magnitudes, and serve to classify the stars according to their temperature.

20. Separating the stars into two groups, those which are brighter than $9^m\cdot5$ on the Potsdam scale of magnitudes, and those which are fainter than $9^m\cdot5$, and dividing each group into four classes according to the colour index, we may determine for each class the parallactic motion, i.e. the mean angular movement per century, arising from the motion of the sun through space. The results are exhibited in the following table :—

Spectral Class	Colour Index	Stars brighter than $9^m\cdot5$		Stars fainter than $9^m\cdot5$	
		No.	Parallactic Motion	No.	Parallactic Motion
K–M	> 8	175	0″·65	269	0″·36
G–K	4 to 8	168	1″·31	428	0″·95
F–G	-1 to 4	264	2″·58	959	1″·53
A–F	< -1	240	1″·97	460	1″·28

In this table the red stars are on the top line; the third line consists of stars which are in the same stage of development as the sun, those in the second line are somewhat cooler and redder; those in the last line hotter and bluer. The last line includes a few, but only a few, B stars, as there are not many in this part of the sky. The quantities in the fourth and sixth columns of the table are a gauge of the distance of the stars to which they refer. It is only necessary to divide 337″, which is the angle through which a star, in this part of the sky, distant 1 parsec, would have been displaced by the solar motion in 100 years, to obtain the distances in parsecs. Thus the 240 stars belonging to type A and F and brighter than $9^m\cdot5$ are at an average distance of 337/1·97 or 170 parsecs.

The first point to notice is that the quantities in the sixth column are always considerably less than the corresponding quantities in the fourth column. This is, of course, because the faint stars are on the whole farther away. The average distance of stars of magnitude $10^m\cdot0$ is approximately 1¾ times as great as for a star of $8^m\cdot0$. The next point to notice is the very great distance of the red stars. The 269 faint red stars are very nearly 1000 parsecs away or 200 million times as distant as the sun. At this great distance the sun would appear as of magnitude 15·5, but these stars vary in magnitude from 9·5 to 11·0 on the Potsdam scale, and are therefore intrinsically from 250 to 63 times as bright as the sun. Now it happens that among the stars nearest to the sun, whose distances have been actually measured, there are several red stars, and these are all very much fainter than the sun. It has been suggested by Prof. Russell and Prof. Hertzsprung independently that the red stars are of two distinct classes, which they call the giants and the dwarfs, and that in accordance with Sir Norman Lockyer's views the giant red stars are in an early stage of evolution, and are increasing in temperature, while the dwarf stars are at the other end of the series and are growing colder and darker.

Leaving the red stars, it is seen that the stars whose colour indexes lie between -1 and $+4$ are nearer to us than the groups on either side of them. These stars are those whose spectra are of the types F and G in the Harvard notation, and are the stars most like the sun. The mean distances of these stars is only 130 parsecs for the stars brighter than $9^{m}\cdot5$, and 220 parsecs for the stars fainter than $9^{m}\cdot5$. At this distance the sun would be of magnitude 12·1. It follows that these stars are on the average from eight times to twice as bright as the sun. The A–F stars are a little, but not much farther away, the stars fainter than $9^{m}\cdot5$ being at an average distance of 263 parsecs. At this distance the sun would have a magnitude 12·5, and these stars are from sixteen to four times as luminous as the sun.

21. We have seen how the knowledge that the solar system is moving in a known direction with a velocity of 19·5 kil/sec. leads to a determination of the distances of groups of stars whose angular movements are known. The hypothesis we make is that in a number like 100 or 200 stars, the irregular angular movements due to the motions of the stars themselves, neutralize one another on the average. But this is only the mean distance of the group, and some are much nearer and some much farther. The distribution of the stars about this mean distance may be derived from the proper motions, if we know how the linear velocities are distributed. I shall apply this method to the group of stars which are like the sun in type of spectrum, and, therefore, presumably like it in temperature and physical constitution.

Dividing them into three classes according to their magnitude, it is found that their parallactic motion due to the sun's movement, and their average motion in the perpendicular direction, due to their own peculiar movements, are as given on next page (Table IV.).

In the last column is given the ratio of the average cross motion to the parallactic motion. The agreement of the numbers shows that the bright stars and the faint stars have the same average velocity. Taking the velocity of the sun as 19·5 kil/sec., it follows that the average velocity of these stars in the direction perpendicular to the sun's motion is 13·7 kil/sec. This is readily seen in the diagram

TABLE IV.

—	Number	Parallactic Motion	Average Cross Motion	Ratio
All stars down to $11^m \cdot 0$	1247	1″·92	± 1″·67	0·87
Stars brighter than $10^m \cdot 0$	470	2″·50	± 2″·10	·84
,, ,, $9^m \cdot 0$	178	3″·34	± 2″·90	·87

(Fig. 6)—the sun moves from S to S^1 while the star moves from σ to σ^1—the parallactic motion is the angle $S \sigma S^1$ and the cross motion $\sigma S \sigma^1$, and the angle $\sigma S S^1$ is 125° for this part of the sky. $S \sigma$ is very large compared with $S S^1$ or $\sigma \sigma^1$.

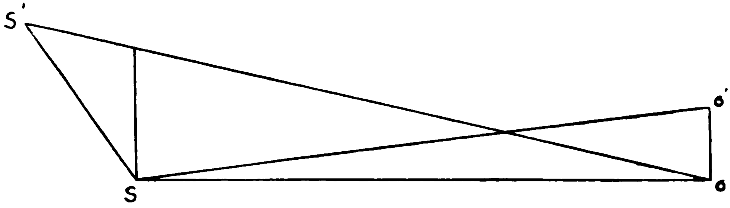

FIG. 6.

22. We shall now make the assumption that some of these stars are moving faster than this velocity, and some slower, just as errors of observation are distributed about a mean error. With a mean velocity of 13·7 kil/sec. there will be in 1000 stars—

233	with velocities	0– 5 kil/sec.
210	,, ,, ...	5–10 ,,
175	,, ,, ...	10–15 ,,
141	,, ,, ...	15–20 ,,
163	,, ,, ...	20–30 ,,
59	,, ,, ...	30–40 ,,
18	,, ,, ...	40–50 ,,
1	,, ,, ...	> 50 ,,

If now the observed proper motions are arranged it is found that the number less than any value τ can be represented satisfactorily by an algebraic formula $N \frac{\tau}{(\tau^2 + a^2)^{\frac{1}{2}}}$, where N is the total number of stars and a is the mean value of τ. The following table shows the actual number of stars with proper motions between certain limits compared with the number given by the formula :—

Limits of Proper Motion	Number of Stars Observed	Number given by Formula	Difference
0″ to 1″ a century	427	429	− 2
1″ ,, 2″ ,,	346	337	+ 9
2″ ,, 4″ ,,	324	332	− 8
4″ ,, 7″ ,,	105	103	+ 2
7″ ,, 10″ ,,	25	22	+ 3
> 10″ ,,	20	19	+ 1

We may take it that the formula substantially represents the observed facts. With the proper motions distributed according to this formula and the actual velocities distributed according to the law of errors, the distribution of the stars in distance may be determined. When this is done we find that these 1247 stars are distributed in space in the following manner :—

Limits of Distance	Number of Stars	Limits of Distance	Number of Stars
0- 50 parsecs	32	300-350 parsecs	138
50-100 ,,	89	350-400 ,,	116
100-150 ,,	136	400-450 ,,	85
150-200 ,,	162	450-500 ,,	61
200-250 ,,	172	500-600 ,,	65
250-300 ,,	161	> 600 ,,	31

The most remarkable feature of this table is that 70 per cent of the stars lie between the narrow limits of 100 and 400 parsecs.

I have treated the 470 stars brighter than $10^m\cdot0$ in a similar manner, and also the 148 stars brighter than $9^m\cdot0$. Combining the separate results it is found that the stars are distributed in space as shown in the following table :—

TABLE V.—NUMBER OF SOLAR STARS (TYPES F AND G) AT DIFFERENT DISTANCES.

Distance	Out of total 1247 stars	Out of 470 stars brighter than $10^m\cdot0$	Out of 148 stars brighter than $9^m\cdot0$
< 100	121	76	40
100-200	298	161	65
200-300	332	136	34
300-400	254	68	8
400-500	146	23	1
500-600	65	5	
600-700	23		
> 700	8		

23. In considering the luminosities of the stars it will be convenient to choose the distances 50, 63, 79, 100, 126, 158, 200, 251, 316, 398, 501, 631, 794 parsecs. These distances are in geometrical progression, and are chosen so that if the same star were moved from any one to the next greater it would appear to us half a magnitude fainter. Thus the sun at these distances would be of magnitudes 9·0, 9·5, 10·0, 10·5 . . . . 15·0.

Next let us suppose a cone drawn with its apex at the sun, embracing the area round the North pole, and divided into boxes by spheres of these radii. The volume of each box will be twice as great as the one next to it and nearer the sun. The number of stars in each box is indicated in Fig. 7. Let us consider the four boxes A, B, C, D.

In volume D = 2 C, C = 2 B, and B = 2 A.

If D be taken as a standard volume, the number of stars in a box this size placed at A, B, C, D, respectively would be 200, 172, 132, 98. Thus the density as we go farther from the sun diminishes. But we must remember now that all these stars seen at S appear to be of $10^m\cdot5$. Thus the stars in D must be brighter than those in C, those in C than those in B, and those in B than those in A. A contains stars of the same absolute magnitude as the sun up to those ½ magnitude brighter. B are from ½ to 1 brighter, C are from 1 to 1½ magnitude brighter, and D are from 1½ to 2 magnitudes brighter than the sun.

24. If we may assume that the actual density of the stars is the same in all parts of the space with which we are dealing, we obtain by reasoning of this kind the number of stars between different limits of absolute brightness, and it is found that in a sphere of 100 parsecs radius the numbers are as given in the following table :—

Luminosity (sun taken as unit)	Number of stars	
	Mag. 10 to 11	Mag. 9 to 10
0·4 to 1·0	16,000	18,600
1·0 „ 2·5	9,500	11,200
2·5 „ 6·3	5,750	7,300
6·3 „ 16	2,570	3,600
16 „ 40	502	1,040
Brighter than 40	14	68

The results in the second column have been arrived at by considering the faintest stars, those from $10^m\cdot0$ to $11^m\cdot0$; those in the last column by taking the stars which appear to be of magnitudes 9·0 to 10·0.

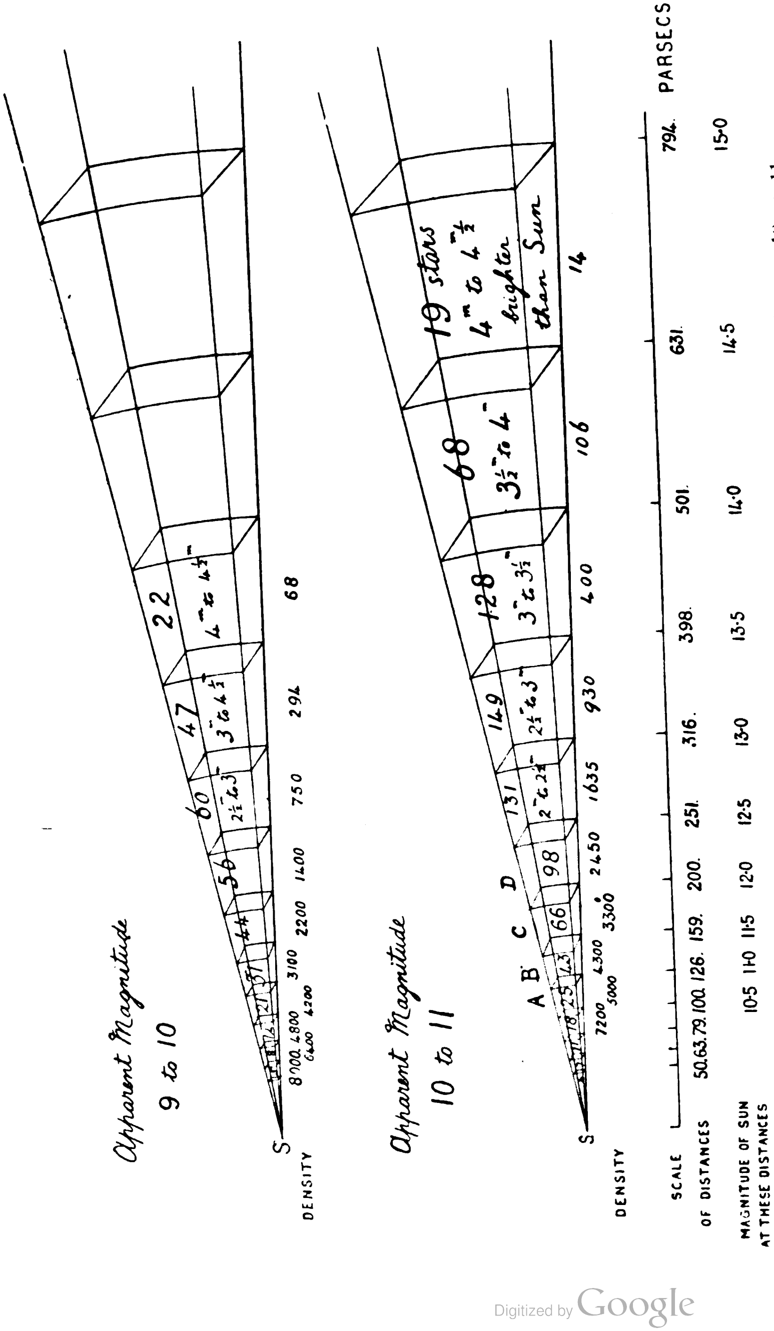

FIG. 7.—Distribution in Distance of Stars of Apparent Magnitude 9 to 10 and 10 to 11.

Although there is fair agreement for the stars of smallest luminosity, there is an increasing divergence between the results for those of higher luminosity. Now, it is to be remembered that these figures have been derived from regions at different distances from the sun. Thus the stars which are between 16 and 40 times the brightness of the sun, and which are apparently of magnitude 10 to 11, lie between 398 and 631 parsecs, while those which are apparently of magnitude 9 to 10 lie between 251 and 398 parsecs. We may conclude therefore that the density of this class of stars is somewhat less at this greater distance from the sun. Following out this line of reasoning, the diminution of density of the stars is found to be as follows:—

At 50 parsecs . . . . Density = 1·30
„ 100 „ . . . . „ = 1·00
„ 200 „ . . . . „ = 0·70
„ 300 „ . . . . „ = 0·48
„ 400 „ . . . . „ = 0·32
„ 500 „ . . . . „ = 0·21

Although much weight cannot be attached to the exact figures, there must be, I believe, a very considerable falling off in the density of the stars between the distances of 100 and 500 parsecs. A falling off in the total density of the stars would affect the tables, giving the proportion of stars of different brightness, and would increase considerably the proportion of bright stars.

25. Although the conclusions presented in this discourse have been derived from a study of the proper motions of the stars in a small area of the sky, and may be somewhat modified by the investigation of other regions, they may be considered as fairly applicable to the stars in general. We have considered the stars down to a magnitude approaching 11·0. There are in the whole sky half a million stars brighter than this limit of magnitude.

We may say of them—

(1) On the whole, the yellow stars, the stars like the sun in physical condition, are the nearest.

(2) They lie within fairly narrow limits of distance—80 per cent are between 100 and 500 parsecs; 10 per cent nearer than 100 parsecs; and 10 per cent farther away than 500 parsecs.

(3) As we go from the yellow to the blue or the orange stars, the average distances increase.

(4) The red stars are at great distances—an average of about 1000 parsecs.

(5) The stars vary greatly in *intrinsic brightness*. The red stars are specially luminous, being on the average 100 times as bright as the sun.

(6) If we consider all the stars down to this limit of magnitude, we find that approximately 95 per cent are intrinsically more luminous than the sun.

(7) When, however, the luminosity of the stars in a given volume of space is considered, there are found to be far more faint than bright stars. There is no contradiction between this conclusion and the last one, because the more distant bright stars are visible, while we only see the faint ones which are comparatively near.

(8) Evidence has been found that the stars thin out very materially at great distances from the sun.

26. These conclusions are in harmony with the conception of a finite stellar universe. Most of the stars we see and a great many fainter ones are within the distance of 1000 parsecs. Doubtless the stars extend to much greater distances, perhaps 10 times as far or farther, but we can hardly doubt that we are near the middle of a finite group of stars, and that the extent of this group is of the order of 1,000 to 10,000 parsecs.

It has been suggested, though the opinion is a speculative one, that the Andromeda Nebula, and Spiral Nebulæ in general, may be similar groups of stars separated by vast intervals of empty space from the stellar system to which we belong. If this is the case, we may suppose that our system of stars, as seen from outside (say a distance of the order of 100,000 parsecs), would present an appearance similar to that of the Andromeda Nebula as seen from the earth.

[F. W. D.]

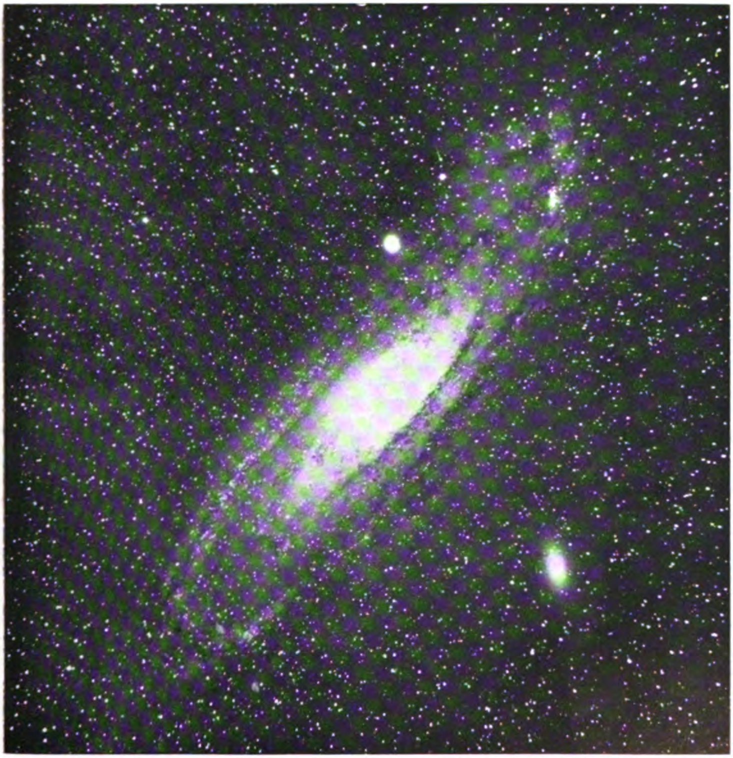

FIG. 8.—GREAT NEBULA IN ANDROMEDA.

ANNUAL MEETING,

Friday, May 1, 1914.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S.,
President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1913, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, with one dissentient.

Thirty-three new Members were elected in 1913.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1913.

The Books and Pamphlets presented in 1913 amounted to about 289 volumes, making, with 710 volumes (including Periodicals bound) purchased by the Managers, a total of 999 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
SECRETARY—Alexander Siemens, M.Inst.C.E. M.I.E.E.

MANAGERS.

The Right Hon. Lord Blyth, J.P.—*V.P.*
Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. LL.D.—*V.P.*
Charles Hawksley, M.Inst.C.E.—*V.P.*
Major Edmond H. Hills, C.M.G. D.Sc. F.R.S.
Donald William Charles Hood, C.V.O. M.D.—*V.P.*
Sir Edwin Ray Lankester, K.C.B. LL.D. F.R.S.
Sir Alexander C. Mackenzie, Mus.Doc. D.C.L. LL.D.
Robert Mond, J.P. M.A. F.R.S.E.
The Right Hon. Lord Moulton, P.C. LL.D. F.R.S.—*V.P.*
Edward Pollock, F.R.C.S.—*V.P.*
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
The Right Hon. the Marquess of Salisbury, G.C.V.O.
Alan A. Campbell Swinton, M.Inst.C.E. M.I.E.E.
Harold Swithinbank, J.P. F.R.S.E.

VISITORS.

William R. Bousfield, K.C. M.A.
Charles John P. Cave, M.A. J.P.
Rev. Edward S. Dewick, M.A. F.S.A.
William J. Gow, M.D. M.R.C.S.
William A. T. Hallowes, M.A.
John W. Jarvis, F.G.S.
James Y. Johnson, M.R.I.
H. R. Kempe, M.Inst.C.E.
Adolph Liebmann, M.A. Ph.D.
Carl E. Melchers, M.R.I.
H. Foster Morley, M.A. D.Sc.
F. W. Passmore, Ph.D.
Charles E. S. Phillips, F.R.S.E.
Henry G. Plimmer, F.R.S. M.R.C.S.
Arthur J. Walter, K.C. LL.B.

WEEKLY EVENING MEETING,

Friday, May 1, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

EDWARD F. BENSON, B.A.

A Criticism on Critics.

THERE are many characteristics of the human race, which we may say for certain existed in the earliest ages of the world, and will continue to be characteristic of the race until we have developed into supermen, or have been destroyed by a comet, or have come to any such good or bad end as Mr. Bernard Shaw or Mr. Wells so confidently predict for us. Primitive man certainly looked about for something to eat when he was hungry, and took any rude or reasonable steps to obtain it. He laughed, as we do, when he was amused at some grotesque calamity that had happened to a friend, and foamed at the mouth, as we do, when the same diverting incident occurred to himself, and, if we may judge from his habits at the present day, it seems quite certain that if one primitive man sat down and began to construct something of whatever sort, other primitive men formed a ring round him, and, after watching him for a little told him, even though they did not clearly know what he was doing, that he was doing it wrong. Indeed there is a Rabbinical legend of high antiquity that when Adam began to exercise the earliest of all known arts, that of apron-making, Eve stood by and watched him. She soon informed him that he had not the slightest notion of how to sew, upon which Adam, with excellent forbearance, merely replied, "No, my dear, but I am learning" . . . . At least, if there is not such a legend there ought to be, for it presents in archaic form those most ancient instincts of constructive artists and critics.

The charm of standing by and telling people they do not know how to do what they are doing, is of universal application. It is sufficient that somebody should faint at a football match for him to be instantly surrounded by a ring of critics, one of whom holds his head up, a second lays it down, a third unbuttons his collar, a fourth removes his hat, while a fifth covers his knees with a coat. They are not, as you might superficially suppose, trying to give him first aid; they are critics showing him how to faint. It is probable that the artist in question knows how to faint perfectly well, and is doing it quite nicely; it is probable, too, that these unfortunate critics are profoundly ignorant of the elements of his art. But neither his knowledge nor their ignorance make the smallest difference to the volubility of their hints. There has always been and will always be a ring of critics to every public performer. That there was not a ring of critics round Adam in the Rabbinical legend mentioned above was a mere accident due to the newness of the world: at any rate, all the other inhabitants of the world were there, and I have no doubt the serpent joined in the hissing.

Onlookers proverbially see most of the game; but it would appear from the tone they adopt that they alone see and understand any of it, while those who happen to be engaged in it are completely ignorant of the subject. In fact, where the carcase is, there are the eagles gathered together, and with claw and blood-stained beak they discuss it as the vultures discussed Prometheus. And like Prometheus, the carcase for the most part continues to live and circulate under all this storm of squawling and pecking, which may be painful, but is not fatal. Indeed, most of those scarified carcases seem in course of time to become callous to their vultures. Even the man who faints at the football match recovers in spite of the ministrations of his critics; Adam continues his work, and Eve very likely abandons the critical attitude and turns her hand to apron-making—becomes in fact a constructive artist too. That instinct, as we shall see, finds a parallel in more advanced and civilized eras, and is productive of curious results.

As I have said, the moment one man begins to do a thing, others give their opinion on it. It is quite clear that this must be so. All performances must be able to stand a test, must show themselves capable of fulfilling the design which the constructor had in his mind when he devised them. If a man invents a steam-engine, the test is that it should be able to move and to pull, and not blow up oftener than is reasonable. Here the engine is its own critic, and provided it does its work, it justifies its creator. In consequence the circle of loafers who are unable to make steam-engines have nothing to chatter about, and therefore they stroll away in search of subjects which give them more scope. What they want is an invention, the efficiency of which is not a matter that can be verified by material tests such as pounds avoirdupois, or miles per hour, but one of which the efficiency is a matter of taste, a thing about which there are certainly not less than two opinions, and probably about two million. And those affairs, the efficiency of which is a matter of taste, are beautiful things, things of which the aim is to rouse the emotions without regard to their practical utility. They are in fact what we know as works of art. They may be good works of art, or bad works of art, but they are not directly useful, nor is that their excuse for existence. They set out to be beautiful, or perhaps ugly, which now-a-days seems to be considered the same thing. It is round these that the critics congregate; it is the consideration of them that has given rise to the class we know of as professional critics. Their profession is to go on talking for ever and ever about anything in the world of which the efficiency is a matter of taste.

It has been supposed that the art of printing and the dissemination of cheaply-multiplied literature was responsible for the creation of these professional critics, and that, whereas Art existed, as far as we can judge, from the earliest days of civilization, the critic-class was of comparatively modern date. But we have only got to look at the pages of Greek literature to see that, in those ancient days of supreme art, critics must have been fully as prolific as they are now. Innumerable are the references in the Greek anthology to the statues, the paintings, the plays of the period; indeed, it is not too much to say that the bulk of Greek epigrams is concerned entirely with two subjects, love and art. We read that the technique of bronze set forth Myron's genius better than that of marble: that the academic correctness of Polycleitus was likely to please artists more than the general public: or the critic in front of the Zeus at Olympia tells us that Pheidias must have seen a vision of God himself descending from heaven, for so majestic was his conception that the frail mind of man could not have fashioned forth so supreme an embodiment of the Godhead. Or, standing in the Painted Porch at Athens, the epigrammatist says that in the eyelids of Helen lay the whole Trojan war; or, looking at the Aphrodite of Praxiteles, says that the goddess herself asked where the sculptor saw her unclad, and wonders who breathed into the marble the desire and the soul that is in it. Indeed, the whole nature and blood of the Athenian was that of the critic, ever longing to hear some new thing, to have some fresh work submitted to him in order to exercise his trained judgment upon it. Again, in Roman days, we find prevailing the same curiosity, the same appreciation. Pliny tells us that Zeuxis painted not the body but the soul of his sitters, and intersperses his history with stores of the most trenchant art-criticism. So, too, does Strabo; and Ausonius, in somewhat exaggerated admiration of realism, beseeches the neat-herd to pasture his bulls farther away from the bronze cow of Myron, lest she be agitated. As for Pausanias' tour in Greece, one is tempted to believe that he got a Baedeker of the period, interleaved it, and covered the pages with art-criticism.

There is no need to multiply instances to show the antiquity of the art of criticism. It is an old and noble art, coeval with that of creation, and it is only the ignorant who bewail the halcyon days which never had any existence in fact, when the artist was left to dream and do, unhampered by carping cries and vulture beaks. For the truth is that creative art is so bound up with critical art that it is impossible to conceive of the one without the other. Art itself exists only by the critical spirit; the very fact that its efficiency is a matter of taste, that its object is beauty, that it aims not at useful but emotional targets, presupposes that selection which is the basis of criticism. The critic, in so far as he is worthy of the name, must be steeped to the bottom of his soul in the art he loves: it is his to discern the subtleties of its beauty, the poignancy of its pathos. A perfect understanding of the art, saner and more cool than the hot imagination of the artist, must be his; he must be able to see the beauty of intention through the mist of its imperfect rendering; he must divine the fire that maybe smoulders beneath ash, and by his exposition make it visible to others. He must be endlessly patient, even as is the seeker for gold in alluvial deposits, washing the minute grains from the mud, treasuring them, testing them with the corrosive acid of his trained judgment, rejecting the worthless, nor caring to speak of it, but firstly and lastly and supremely storing the gold.

Nor does his task end here: he is the interpreter of beauty as well as its gatherer, and thus he may become the creator of a new beauty. For it is the pianist who plays in a Beethoven sonata, the conductor who takes us into the mind of Wagner, the actor who shows us the blood-stained infirmity of Macbeth, who are the critics of the masterpieces they interpret for us. They give us their impressions of beautiful things; and, rightly, we call them artists as well.

Mr. Matthew Arnold, in his essay on "The Function of Criticism at the Present Time," demands all this and something more debatable from the critic when he says that his aim is to make the best ideas prevail. But, since art deals entirely with questions of taste, it is an assumption to suppose there is a definite test, though we must be allowed to suppose that some, critically considered, are good, and many bad. Even then large allowances, and how large nobody can say, must be made for acute differences of opinion in all such matters. At the present time, for instance, while the old-fashioned among us happen to prefer portraits by Sir Joshua Reynolds to those by the post-impressionists, and the landscapes of Turner to those of the futurists, others think differently, and we have no right to assert that their ideas belong to the class of bad ideas, though we may think them lunatics on all matters connected with painting. Similarly, we must allow the same latitude to the appreciators of cubism, and permit them to believe that they find beauty, or some equivalent for it, in what appear to us to be insane or at least insanitary canvases, regarding them as representing the best ideas, though to us those ideas seem comically bad.

Even more debatable is another dictum of the same author that critics must see the object as in itself it really is. This, applied, as he does apply it to the arts, passes comprehension, for if we take it literally, there is nothing more to be said about a statue than that it is a piece of Carrara marble, or about a painting than that it is a superficies of pigments. Or, if we take it to mean that the critic must judge of a work of art by its truth and fidelity to the object it represents, we are again fearfully at sea. For to demand this is to demand a slavish realism which cuts away the very ground on which art is founded. For all art is built on convention: it is always symbolic in that it is selective. To ask for the other is to turn romance into a chronicle, and to demand of a picture that it should by its verisimilitude deceive the beholder into imagining that it is real. That a work of art should to some extent be like what it represents, and thus conform to some broad creed of realism, may perhaps be expected of the artist, though the latest exponents of pictorial art have apparently rejected this as an apostasy. But most of us still want to be able to see, without having to consult a catalogue, whether a picture represents Christopher Columbus or Waterloo Bridge; to ask of a statue that means to be a man, that it should have a head of some kind and the usual number of arms and legs; of fiction and poetry that they should tell us about persons who might conceivably belong to the human race, unless it is definitely stated that they do not, and that they live in a world which we may be excused for supposing is our own, unless the scene is laid in Heaven, or Mars, or Hell. But to demand a complete realism is to do away with that on which art is based. Besides, if art is merely to reproduce life, its object is more than sufficiently realized by life itself, and it can have no excuse for its existence at all. And to ask of the critic that he should judge by such standards as these is to take from him his appreciation of imaginative vision. His office is not to see the thing as it really is, but to see the vision which inspired the artist, its beauty, or, if you will, the ugliness which roused his misguided enthusiasm, to judge of the skill with which he has realized it, and to convey his sense of the same to his readers by his exposition of it. We do not ask him to be infallible; we require only the clear statement of his interpretation, his view of what he criticizes conveyed as forcibly as may be necessary, but put forward only as an opinion, though based on collateral knowledge. We do not expect him to be the critic *in excelsis*, who may make a fresh work of art out of what he criticizes. But it should not be too much for him to express a view and to avoid the tiara. Instead of which he is infallible with almost indecent frequency, and it is often impossible to understand what he means.

Here, while still clearing the arena, we may consider the Whistler heresy. Others before Whistler were guilty of it, but he put it forward with such splendid Aryan pomp that it must always be associated with him. He demands that any critic who is worth the attention of the artists should be a life-long practitioner in the art he criticizes; and passionately resents Mr. Ruskin being ranked as a critic at all, because he was not a painter (which, as a matter of fact, he happened to be). "Two and two," Mr. Whistler exclaims, "make four according to the mathematician, and it does not matter what the amateur and the critic say." He tells us that it would be as reasonable to have the Observatory at Greenwich in the hands of an apothecary, or the College of Physicians under the presidency of Tennyson, as to put art-criticisms in the hands of a man who is not himself a painter. But none of these analogies will hold. Certainly it does not matter what the amateur and the critic say regarding the addition of two and two, since the fact that they do make four can be demonstrated to and by everybody, for it is in no sense a matter of taste, but of fact readily verifiable, and the entire science of mathematics is built up on this class of fact, which is in no way a matter of opinion. But on the other hand no apothecary, if he were only an apothecary, could possibly demonstrate the orbit of Saturn, nor would writing millions of idylls of a thousand kings prevent Tennyson from giving prescriptions of prussic acid to cure colds in the head But art, even if everybody in the world agreed about it, must be a matter of taste in a way in which addition sums or the effects of the administration of large doses of poison are not, except to Christian scientists; and it is by taste, that both critics and artists prove their quality. Very likely Whistler's critics were wrong when they decided that his picture of Battersea Bridge was not worth £200, but they had a right to be wrong, and were not wrong in the same sense that Tennyson would have been wrong if he had dispensed drugs without knowing anything whatever about the effects of them. Ruskin did not admire Whistler's picture, though Whistler did: to the artist its efficiency was proved, but to this particular critic it was not. But if Tennyson had given a pound or two of aconite to a man with a sore throat, the result would have been proved to the satisfaction or otherwise of everybody. We may add also that when artists such as Sir Edward Burne-Jones were called in to pronounce upon Whistler's work, he did not like it any the better.

Indeed, there are admirable reasons why the practitioners in any art should not be installed as critics of that art; and Mr. Tom Taylor, art-critic to the *Times*, pointed out one of them in answer to this pamphlet of Whistler's when he said, "God help the artists if once the criticism of pictures falls into the hands of painters." But mere censorious jealousy, which is what is here hinted at, does not go to the root of the matter, nor near it. The fact is that, inasmuch as a man does practise a certain art, he is for that very reason incompetent to take a sane and critical attitude about either his own performance, or that of his colleagues in the same art. For in that he is an artist, he cannot really imagine anybody seeing or rendering a thing in a manner altogether different from his own. A certain key of writing, a certain colour of words, or melody of pigments, as the critics say, is his, and since he uses that, he must think that it is the best possible mode or melody. Who, for instance, except the pundits of the English law-courts, could seriously value the opinion of Mr. Frith with regard to Whistler's own picture, or who, if he wanted a sound opinion on the work of Miss Marie Corelli would go to Mr. Hall Caine for it? The very fact that these illustrious persons are the practitioners of an art, invalidates their criticism on the work of their colleagues, for as was long ago pointed out, "great artists do not admire each other's work." They are inevitably biased; their concentration on their own modes must necessarily have narrowed, or at least focused, their taste; it would be as reasonable to expect a lens to serve the purpose of a plate glass window. They cannot have the broad outlook of the competent critic, who, though he may not wholly divest himself of personal preferences, must be able to survey with a breadth impossible to the artist. For this reason Mr. William Morris refused the chair of poetry at Oxford, definitely stating that the practice of an art debars a man from criticizing it. So, too, thought Molière when he went for the criticism of his plays to his housemaid. She—any housemaid in fact—could take a view which was barred to Molière simply because he wrote plays. Indeed, Whistler himself is an admirable refutation of his own heresy, and his views on painting conclusively show how little right has a consummate artist to have any. But where shall we find so acute a critic of other people's manners? And where shall we look for the cause of this but in the fact that he had not got any himself? It must needs be so: those who practise any form of art must be content to be criticized by those who are hopelessly incapable, as they are sometimes unwise enough to show, of doing it themselves.

We may take it, then, that part of the equipment of a critic is incompetence in the practice of the art which he criticizes, and will not demand a symphony from Mr. Robin Legge, a portrait from Sir Claude Phillips, or a novel from Mr. Courtney. Just, then, as all but painters may be listened to with patience about painting, so—for criticism is an art in itself—everyone but critics may be permitted to say something about criticism, which is precisely the reason why I am doing so now. And as the critics of the present day find that there is very little in the artistic world to which they are able to extend their cordial approval, so certain of those who are not critics find that there is little in the critical work of to-day that is worth saying. In every profession there are, of course, at all times a large quantity of wholly incompetent people, and the fact that the journals in which they write give their observations so wide a currency, perhaps makes the incompetent appear more conspicuous than they really are. But wide circulation does not constitute authority, any more than the advertisement boards which now decorate the fields near railway lines are a guarantee of the efficacy of the tonics they recommend; or at the most, these tonics and these critics have a kind of hypnotic authority arising from the effect which incessant repetition, like that of the importunate widow, brings with it. Never before, perhaps, has criticism been so scribal, and, in consequence, so lacking in authority in spite of the thousand mouths through which it speaks—in spite, too, of the crowning glory of the lately-constituted English Academy, which sheds its halo round the heads of so many of our prominent critics. They adopt, however, the attitude that prize sheep might be supposed to assume if turned out to jejune pastures. "How fat and well liking we should be," they seem to say, "if only there was anything to browse upon." They may be perfectly right, but we cannot help remembering how often before they have said that, when, as a matter of fact, the meadows where they were turned loose were, as subsequently proved to be the case, abounding in nutriment.

Did they browse on the wondrous pastures opened by Keats, those who in that day had some authority? Or did they find him a "writer of uncouth language, a cockney versifier, negligible as vermin?" It is true that since then they have lost their authority, so that even that hyper-sensitive soul would not to-day suffer those utmost pangs of mortification; but has their loss of authority led to any gain in acumen? Too often have they shown themselves eager to belittle instead of to appreciate new forms of beauty, though too often they have been extravagant in their applause of the mildly incompetent. But when have they been the first to recognize a great personality in the world of art, when to lead the public up some new and royal road?

Indeed, if we examine quite cursorily the record of the achievements of English criticism as won by our leading critics, it is a poor tale. Time was when a review in the "Quarterly" or the "Edinburgh" could, if not make or unmake a reputation, give an artist the chance of being heard, or withhold that chance. And how, in the main, was that authority used? What chance did they give to Keats, and who, but they, made it possible that the genius of Shelley was not recognized in his life-time? No doubt it is difficult to recognize fresh forms of beauty at first sight, when the vehicle—the idiom—is a new one; but the critics, whose business that happens to be, have always found it not difficult merely, but impossible. To read the criticisms with which the leading musical lights of the day hailed the advent of Wagner, when he gave the recitals from his operas in the Albert Hall, is to make one doubt the evidence of one's own eyes. Or, take the strange case of Mr. Robert Montgomery's poems. Literary critics in those days of poetry and their own authority had puffed his lines on the Omnipotence of the Deity into eleven editions. That was bad enough, but worse was to come when Macaulay killed him dead by his review in the "Edinburgh," which must remain for all time as one of the most monstrous crimes in criticism. In order to show the unbiased and judicial character of his attack, he prefaces his review by saying that he knows nothing about him, "except what he has learned from his books and from the portrait prefixed to one of them, in which he appears to be doing his best to look like a man of genius and sensibility, though with less success than his strenuous efforts deserve." And this is supposed to be criticism, this the work of the most authoritative critic of the day! It is mere personal abuse, as rude and as irrelevant as mud thrown at a politician by a street boy. That inept kind of personal attack has, unfortunately, not become obsolete; and an instance may be mentioned from a reputable journal the other day, when a musical critic called attention to the fact that, whereas most musicians had long hair, Mr. Schomberg, who was conducting his own works, was bald. What that has to do with Mr. Schomberg's music he did not explain.

Or, again, is it possible to read the account of Charlotte Brontë eagerly waiting for press-cuttings about "Villette" without a sense of incredulity? Or what of Fitzgerald's "Omar Khayyam," which did not attract the notice of a single critic till Mr. Swinburne fished out a copy from a two-penny book-box? Or what of the peerless Greek marbles which Lord Elgin submitted to the judgment of a leading critic in antique sculpture? "You have been taken in, my lord," said this eminent personage, after a careful examination of them; "they are late Roman." Nor must we forget that all Carlyle found to say about them was that neither God nor man can do without chins. What welcome did the critics give to a noble brotherhood of Pre-Raphaelites, to the work of Rossetti and Burne-Jones, until the appreciation of the public opened their eyes? Or what of the wreath of ridicule with which criticism crowned Mr. Whistler's exhibition of his own painting? This has been preserved to us by the artist himself, and it is impossible not to pluck a few blossoms woven in by the best-known and leading critics of the day. Here are some.

"We are not sure but that it would be something like insult to our readers to say more about these things. . . . They must surely be meant in jest. . . . A farce in moonshine with a dozen dots. . . . Art is not served by freaks of resentment. . . . (I do not know what this means.) . . . The blue and black smudges which purport to depict the Thames at night . . . a gruesomeness in grey . . . I have expressed, and still adhere to the opinion, that these only come one step nearer pictures than a delicately-tinted wall-paper!"

In spite of this, however, we may remark that it has been found necessary to have a piece of this delicately-tinted wall-paper in the Tate Gallery, where it now gloriously hangs. It was rather an expensive pattern, which worked out at the price of something like £5,000 a yard. As a final touch, the name of the master on the label below it was spelt wrong. It is a thousand pities that Whistler was not alive on that day: he would have written a charming letter to somebody about it.

Or what of the reception given by the critics to Tennyson's earliest and most exquisite work? Did they find beauty in

"Mariana," or food for ridicule in "My Room"? Or when did they become aware of Wordsworth? At what age was Browning, and how many masterpieces of drama and song had been achieved by him, when first the critics, instructed by the public, began to recognize his existence? And when did they awake to the fact of George Meredith?

This is not a very promising record of past services that we have here, when we seek to find out what we owe to critics for their discovery and recognition of beauty. There is not one star in the artistic firmament of the nineteenth century whose rising was seen by the telescopes of these magi. Invariably it either escaped them altogether, until when, blazing in mid-heaven, the public pointed it out to them, or, if they saw it, they called it no true star, but a gaseous comet, which would disintegrate again if they threw a few stones at it, or clapped their hands, as if the star and the singer were a cat on the tiles, which obstructed their view. On the other hand, they often thought that a cat on the tiles was a star, and hailed it as such, and magnificently proclaimed how high it would soar into the zenith; and this, I think, has been as much due to their fear of again not recognizing the rising of some star, as to genuine admiration of the cat. Having learned by a century of sad experience that their ancestral colleagues had often missed a thing because they did not understand it, they not continually prostrate themselves before some bizarre charlatan, in case he, like Keats and Charlotte Brontë and the pre-Raphaelite brotherhood, and a few little trifling names of that sort, may afterwards prove to be of some significance. And if it turns out right, they pat each other on the back, and say, "We told you so." A tragic instance of what we may call the cat-star occurred quite lately.

There was an exhibition in the Grafton Street galleries a few years ago which filled our mouths with laughter and our tongues with joy. It was called the post-impressionist school, and apparently now is endemic amongst us like, indeed, very like, measles. The critics went there, and remembering their sad dilatoriness in days gone by, determined that this time they would be up to date, and with tear-dimmed telescopes they swarmed up Pisgah and beheld the promised land. Some of the most authoritative among them thought it a rather hazardous expedition, but in general by their writings or support they joined the flight of eagles who could look at the sun without laughing. Dawn had come: no star but the sun itself uprose over the promised land. A new renaissance more lovely than Italy had ever seen, more glorious than the art which blossomed by the Ægean Sea, had streaked the heavens. "We have ceased to ask," exclaimed one of them, in a dithyrambic outburst—"we have ceased to ask, 'What does this picture represent?' and ask instead, 'What does it make us feel?'" And . . . it is almost too pathetic, once again the critics appear to have done the wrong thing. At least most of them are quietly strolling down from Pisgah again, with their telescopes in their pockets.

We must not, then, ask too much from the critics in the way of recognition of new forms of beauty, for it is pretty clear we shall not get it, and we will limit our expectations to finding sincerity, knowledge of the subject with which they deal, and expositions grammatically and intelligibly expressed. Even then we must be prepared to find ourselves disappointed by the most learned critics of the day, for even those great enough to sign their names at the top of their articles are placed in a position in which it is difficult for them to do the work which is demanded of them with anything like thoroughness, or, in the incessant hail of books and pictures that descend on them to read with care, or to look at with a seeing eye, one tithe of what they profess to pronounce on. It has for a long time been a standing sneer at literary critics, for instance, that they do not read all the books of which they so pontifically judge: and we find that this sneer, if it is a matter for sneering, is substantiated with charming naiveté by one of the most prominent of them whom I will call Scriptor. Every week, or oftener for all I know, he reviews with considerable pomp some outstanding publication, and not long ago he made a candid confession as to his methods. "I do not propose," he wrote, "to have read the whole of Mr. George Moore's final instalment of biography which he calls 'Vale.' It runs to 350 pages, and the book is closely written, so that the despairing reviewer finds it well-nigh impossible to adopt his customary device of skipping." Then follows a review as papal as usual. Now this is all very well for the despairing reviewer, but what of the despairing author or the despairing public who expect at the hands of so eminent a critic something more than this? Is it any wonder that critics have not much authority when the foremost of them all complains that the close writing of a book forbids him to skip, and that therefore he has not read it all? But his avowedly slight acquaintance with his subject does not impede the finality of his judgment. But pontificality is characteristic of Scriptor's work; and if a contemporary account of the creation of the world turned up, and Scriptor thought it worth reviewing, he would give the impression of having had a hand in it himself, and of having been chosen as conductor of the morning stars when they sang together. I do not complain of this solemnity; the judge should be bewigged and berobed even though he has not followed the whole of the case; he should not jest as some judges do when on the bench: or try to be witty, which indeed Scriptor never attempts. But the solemnity does not conceal a certain difficulty that Scriptor seems to experience in writing the English language quite correctly. The following sentence, for instance, is not quite happy. "Whitman's life, especially during the last part of it, is that which ought to rest in the memories of his countrymen, and which was in many respects better than anything he wrote." Nor should he talk of a thing as "translated in English." It is a pity to say things like that; it is more than a pity to speak as follows. Here the subject was Mrs. Gaskell, of whom Scriptor approves in this manner. "But then she wrote 'Cranford,' which secured her place in English literature—a book full of a gentle comprehension, of a simple life lived in clear autumnal weather, in its way one of the most successful studies of the little town which we now know as Knutsford, and which was subsequently described as Hollingford, which any sympathetic artist could have put on paper."

Now, nothing will persuade me that this is a proper sentence; indeed, it is difficult to know how anybody who knew what a proper sentence was could possibly have written it; and I dig up these scandals with great personal pleasure, because I have so frequently and smartly been told by Scriptor's colleagues that I am incapable of writing grammar myself. This is the sort of sentence which critics, if they are happy enough to find it, print in full in a short review of a long book as a sample of their author's style with the statement that there is nothing more to be said about him. Nor perhaps should there be: and we will pass on.

But if these are the official pronouncements of a general of the critical army, what must we expect from the conversation, so to speak, of the privates—of those who, lately enlisted, supply the shorter and anonymous notices of books? Let us grant that they have what is called a literary bent: let us credit them with a public school education and a smattering of Greek, and they now have obtained posts as reviewers on one of the daily papers. But they have yet to learn to write, they have yet to form their literary taste and judgment, to acquire the knowledge on which it is based. And they find no school, no tradition, by which they can educate themselves, no standard, such as for two hundred years has marched in front of the critics of France, to guide them, but the whole business is haphazard, slovenly, and party. Even the driver of a taxi-cab has to show his competence in steering through traffic: no such test is required of those who steer the public through the mazes of art. Instead, cartloads of inferior novels are weekly shot out on them; they have to judge not of the 350 pages which are too much for Scriptor, but five times that number of pages, and to write what purports to be a respectable opinion on them. No wonder they get practised in the art of skipping, for the task imposed on them is an impossible one. To review books which they cannot possibly read with care may teach them how to skip, and how to write swift twaddle, but it cannot lead to the formation of a sound literary judgment. Very likely three-quarters of what they are supposed to read is very poor stuff, but what they say about it is like to be poorer. And further, they have to convey the impression of having discriminated, and so they mingle their contempt for most of what they tell us about with the most extravagant eulogies of the stars they have discovered. If they only had the authority of the critics who showed Mr. Robert Montgomery through eleven editions, the lot of the novelist would be full of the most marvellous prizes. But these eulogies really serve no purpose except that of the publisher's advertisements, for these obsequious notices are written by young and absolutely unknown men, and are carefully extracted from the flood of criticism which is poured out on any new book, if its publication is what we may call carefully managed. But if they were signed, as the opponents of anonymity would have them be, they would not carry one jot more authority than they at present do. And a vague suspicion enters the mind that there has been a touch of log-rolling about it all. But the log rolls, rolls out of sight.

The average critic, in fact, is ill-equipped for his work, ill-paid and sweated. At least he would be sweated if he did the work which his reviews assume that he has done. And he has exactly as much authority as an advertisement of pills when he should be giving a sound professional opinion.

In this matter the unfortunate public are like sheep without a shepherd, a pathetic flock. But vastly more pathetic is the case of all those critics who may be likened to shepherds without any sheep, for the reading public do not hear their voice or throng into their folds. Indeed, the contemplation of these flocks of shepherds is positively heart-rending. Madly and distractedly they rush up and down the highways and hedges of the press, seeking sheep and finding none. With shrill and clamorous cries, they encourage each other to explore all sorts of quagmires and muddy places in the wild hope of finding some sheep there; they fall headlong into ditches full of printers' ink, they stumble in the gloaming across empty and desolate fields in the hopeless pursuit of finding a flock somewhere. They follow with sudden kindlings of excited enthusiasm any will-o'-the-wisp that hovers over mephitic places, any marsh-fire of poisonous gases, hoping that its fitful flame will attract the sheep that they are searching for, and that by hook or crook (they do not mind which) they will capture them, while the night of their Critic-dammerung darkens hideously around them. Others have given up the search and sit them wearily down, bewailing the entire absence of flocks and pastures, while their more sanguine colleagues continue to rush madly along in their unrewarded search—unhappy, distracted shepherds, without the semblance of a sheep between them.

I do not think that art critics are in much more fortunate plight than their brothers in literature. Their *doyen* and our scape-goat I take to be Pictor, whose industry no amount of picture exhibitions seem to tire: who, when there is no fresh exhibition to stimulate his acumen, tells us about Rembrandt and others. Indeed, in this he resembles Mr. Pickwick at Wardle's Christmas dance, who, "when there was no demand whatever on his exertions, kept perpetually dancing in his place." Pictor appears not to find it at all difficult to write English: I should say he found it too easy if anything, for he is apt to burst into French for no ascertainable reason, and the difficulty comes in when we try to understand what he says. Being a sedulous reader of his work, I permit myself from time to time to pick out the plums which so plentifully bestrew his puddings, and illustrate his style and his teaching. Here are some.

He appreciates Grunewald.

"The Nativity, if such it can be called, utterly indefensible if we analyse it in cold blood, so wonderfully, nevertheless, expressing an ethereal if short-lived joy, carried to the verge of frenzy, that we are swept away with our master into regions where reason is in abeyance."

That appears to be the case. There is no verb but many adjectives.

Or this. I cannot say with any certitude what it is about.

"Even the passion of the human voice, of the human gesture emphasized and broadened, even these supreme modes of expression, supported and reinforced though they should be, by the divine colour, the divine pulsation of music, fail to convey certain phases of superhuman joy, of desire for the infinite, certain modes of what might be called Praxitelean or Corregiesque suavity."

Now, I am willing to believe that Pictor meant something when he wrote this, and even that he knew what he meant: all I am quite certain about is that he does not tell us what it was, and we may indeed ask, "Shepherd, what art thou pursuing?" If we imagine Mr. Ruskin suddenly stricken with some brain-numbing disease in the middle of one of his splendid sentences, and his hand carried on by its own impetus, this is the sort of thing we may imagine he would have written. Pictor seems to lack the clarity of the fine critic: he is too suave: his suavity is more than Corregiesque; it is glutinous, and we stick in it helplessly like flies in jam. We do not doubt his knowledge, but his message is as inarticulate as the sonorous note of the honey-laden bee, and we want our Maeterlinck to interpret. Now the interpreter should not need an interpreter. He is there to make things plain to us, to help us to understand, not to daub us with syrup, as we daub tree-trunks to catch moths. His aim should be not to stupefy, but to stimulate, to kindle the eye of others to see what he has seen, and to do that he must tell us what it was.

Pictor begins one of his articles of the "Masterpieces of Painting" thus:

"A favourite game, and one of a superior order, is that of putting together the hundred masterpieces of painting, or the hundred best pictures of the world. For those who plunge into it with a light heart, and feel no misgivings, for those who play the game in a measure for themselves, but partly also for the gallery, it is one of exquisite fascination and delight."

I daresay that is a very good game, and hope to taste the exquisite fascination of it some day; in the interval I suggest another game for art-critics which I think should also prove exquisitely fascinating. I propose that when they next fashion their annual thunderbolts out of the Royal Academy, they should not be given catalogues, and that wads should be placed over their mouths so that they cannot confer. They would thus write their notices without knowing the names of the artists whose pictures they criticized. Probably they would recognize the work of Mr. Sargent and one or two others; but I think it would be an exquisitely fascinating moment, when, having delivered their criticisms to the printer, they were given the catalogues, and found out what they had said about whom. For as in the case of literary critics, it is impossible that they should really look with care at all the works which they lightheartedly condemn or approve, and by this plan they would sincerely, and without guidance of eminent names, be helped to tell us what to their trained tastes was really admirable, and why. I think this game should be tried.

Now, the case of dramatic critics is harder yet, and their authority even more forlorn. They have to attend a first performance one night and to make ready their oracles in time for the first edition of the papers next morning. Of course, they have their conferences in the foyer between the acts, which help them, and must do so much to encourage independent opinion. But can we attach much value to work done in such haste? I do not imagine that most of them attach much value to it themselves, and with a wise modesty they frankly accept the role of Molière's housemaid, and confine themselves to saying just the things that a housemaid would have said, such as "Lor," or "Well, I was astonished to find that Mr. Bourchier had come back from Queensland," or "Poor thing! but she should never have allowed the other gentleman to have come to her bedroom." They then proceed to tell us about Sir George Alexander's trousers, and the ladies in the stalls. The anonymous critic of the *Times*, however, whose anonymity Mr. Bernard Shaw has so ruthlessly revealed in the preface to "Man and Superman," which he prudently dedicates to him, refuses this role, and recklessly assumes all the responsibility which his colleagues, or, as he would say, his confrères, waive. But even if we include "Dramaticus" (for I refuse to follow Mr. Shaw's craving for the publicity of others), the whole collective power of dramatic criticism nowadays is what scientists would call absolute zero, and there is not a dramatist living who cares one jot for the disapproval of the morning press, if only his audience have signified their enjoyment of his play; nor is he, from the point of view of its success, the least consoled by the panegyrics of the press, if the audience go yawning away. For Mr. Smith and Mr. Jones and Molière's housemaid know quite well if they have passed a pleasant and an interesting evening, and do not wait for Dramaticus to inform them next morning whether they have or not, while if they have been bored, they do not care whether Dramaticus was amused. For a good play, after all, from the point of view of the audience, is one which the largest number of Molière's housemaids like, and in drama above all the arts does the individual rely on his own taste. An author may be boomed by the critics into some sort of esoteric popularity: and, if they are acute, they will be able to find subtleties and beauties in books and pictures which to the ignorant require to be pointed out, and are enjoyed when seen. For in the matter of reading, it is possible to sit at ease, to look for such beauties, to find them, to enjoy them; but in a play, with the appeal to the eye, the ear and the mind going on simultaneously, and with the inability of the audience to put it back a scene or two, to go over it again, as we can retrace our passage in a book, they cannot consider what they ought to like, but only what they do like. No dramatist was ever boomed into popularity by the critics, or ever boomed out of it. For all the laws of dramatic art are founded on the taste of the audience; the need of the dramatic unities, for instance, only implies that the average mind requires a certain order and consecution in the events that it watches. Morally, and on principle, the average mind does not care one straw about the question. Meantime, the flood of dramatic criticism that deluges the press after every new play provides excellent practice in rapid writing for the critics, and they have the pleasure of being at all the first nights.

It has been said—I think most unjustifiably—that critics as a class are unsuccessful and disappointed artists; but this is not a fair statement, since they have not generally been anything before they became critics, except school-boys. But it often happens that a critic, having perhaps learned something about literature and drama in the course of his judgments upon the works of others, tries his hand at producing himself, and it is then that the disappointment comes. For just as we saw that the practitioners of an art were unfitted by their very practice to be good critics of it, so criticism unfits a man for practising the art which he has spent his time in criticizing. The critic or essayist is apt to be discursive and explanatory if he turns his hand to romance, while in poetry, as in the case of Matthew Arnold, the lyrical mood stiffens into the moralizing or didactic. The critic's business has been to instruct and to interpret, and the habit clogs his pen which should now merely present. His roses are full of censorious thorns, and, when the reader tries to smell them, he merely pricks the end of his nose. Quite as unsuccessfully, though more rarely, does the creative artist try to turn himself into the critic—unsuccessfully because, as we have seen, his work has given him a biased vision, made him squint, if you will: more rarely, because it is undeniably more fun to make a thing, if you can or think you can, than talk about the things other people have made, particularly if you cannot. But there are, of course, instances where an eminent novelist, having exhausted the reservoir of his imagination, begins to take to criticism rather as a tiger takes to man-eating on the approach of old age. He usually begins by eating himself, that is to say, he writes immense autobiographical critiques. Or, if his zeal leads him to maul other people, he seems still to be uncertain what he is eating: sometimes he takes a mouthful of his victim, sometimes he merely bites himself. He produces on his reader—I have lately been such a one—the effect of being in a beleaguered city surrounded by the enemy and a dense fog, out of which sound a confused and bewildering clatter of arms, from this quarter and that, without a direct assault being ever delivered. Or he seems to resemble the boy at Mrs. Leo Hunter's garden party, who proceeded to entangle himself with the legs of a chair, crawl under it, wriggle round it, do anything but sit on it.

Another reason why the creative artist seldom develops into a critical artist, is that, quite unjustifiably, he often feels contempt for the sister art, on which his own is so largely founded, through resentment against its practitioners, who perhaps have told him many things he did not want to know. Wordsworth, for instance, confounds the art of criticism with its artists, of whom he takes the gloomiest view when he tells us that "the writers in these periodicals, while they prosecute their inglorious employment, cannot be supposed to be in a state of mind very favourable for being affected by the fine influence of a thing so pure as genuine poetry." Tennyson, on the other hand, is frankly personal when in one of his terse and naked moments, of which he was so gloriously unashamed, announced that "critics are the lice upon the locks of literature." Shelley expressed himself in the preface to "Queen Mab" with equal freedom; while Gautier's remarks in the preface to "Mademoiselle Marpin" blister the page. But all these seem to confound the critic who has annoyed them with the art he practises, and it would be as reasonable to gird at poetry, because Mr. Pye wrote some extremely bad verses.

A noble curiosity, the passion to learn some new thing, to find and to embrace the beauty that lurks there, to pursue it instinctively, like a well-trained hound on a hot scent, and by the music of his baying to lead the hunt after him—there is the true function of the critic. He will often have to condemn, but only as the hound rejects the false scent that may lead others astray, and through briar and thicket, and the muddy stream of error he pursues his way, devoting to the urgency of his work all that he has of perception and of leadership. And though he does not create in the sense that a musician, a sculptor, a painter creates, fashioning new forms out of the eternally old materials, yet often his interpretation, his musing on the beauty that others have made, becomes in itself, as one of the most subtle critics of our time has pointed out, a new and wonderful work, more marvellous, it may be, than that on which he builds. Thus from parts but meanly conceived by the writer, the actor may make a fresh work of art, finer than that of which the materials have been given him; the pianist may weave dreams about a sonata, that beautify and ennoble it, and even from the supremest work of art a new marvel may spring to birth, when the eye of the true seer looks on it, and he tells us what it holds for him. So, for instance, was born "the presence that thus so strangely rose beside the waters, the head on which all the ends of the world have come." With art as his material the critic-artist fashioned a new creation, even as out of the materials of life and landscape the poet and the artist have seen and have shown the romance and the magic eternally and everywhere spread, which waited for his revelation of it. Even as it was given to Turner to see and to show a sunset in such manner that those who look on his picture know new wonders in the evening lights or in the clear shining after rain—borrowing, so to speak, his eyes, so they who study the work of the prince of English critics, in whose prose burn the jewels of Job, in which is set the seal of Solomon, look with new eyes, his, on the masterpiece of Leonardo. On the fire that inspired such creation the critic feeds, and the wonders of art are but the ashes from which his Phœnix springs. Patient and keen as tempered steel will he be, eager to find gold and to find fire, which he fashions yet again into forms of new beauty, brooding like the holy men of Benares by the banks of the sacred Ganges, on the sunny river of art that flows ever by and is eternally renewed . . . I confidently expect his coming. Perhaps he has come.

[E. F. B.]

GENERAL MONTHLY MEETING.

Monday, May 4, 1914.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Sir John H. Biles, LL.D. D.Sc. M.Inst.C.E.
Reginald Claude Bussell, Esq., F.Z.S.
James Carmichael, Esq., J.P.
Roger Cookson, Esq.
Montague Stanley Napier, Esq.
Miss Kathleen O'Sullivan,

were elected Members of the Royal Institution.

The Chairman announced that a Legacy of £100 had been received from the Executors of the late Mrs. Edward Singleton, a Member of the Royal Institution for twenty-two years.

The Special Thanks of the Members were returned to Richard Pearce, Esq., *M R.I.*, for his Donation of £100 to the Fund for the Promotion of Experimental Research.

The Right Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. Sc.D. F.R.S. *M.R.I.*, was re-elected Honorary Professor of Natural Philosophy, and Sir Joseph John Thomson, O.M. M.A. LL.D. D.Sc. F.R.S. *M.R.I.*, was re-elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations, 1912. 4to. 1913.
Greenwich Photo-Heliographic Results, 1912. 4to. 1913.
Position of the Sun's Axis, 1874–1912. 4to. 1913.
Photographic Magnitudes of Stars between 75° and the Pole. 4to. 1913.
Cape Observatory: Annals, Vol. X. Part 4. 4to. 1913.
Cape Astrographic Zones, Vol. I. 4to. 1913.

The Secretary of State for India—Census of India, 1911, Vol. I. Parts 1–2. 4to. 1913.
Memoirs of Department of Agriculture: Botanical Series, Vol. VI. No. 4. 8vo. 1914.

Abbadia, Observatoire (the Director)—Procés Verbaux des Séances de l'Académie des Sciences depuis la fondation, Tome III–IV. 4to. 1913.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXIII. 1^{o} Semestre, Fasc. 6. 8vo. 1914.
Accountants, Association of—Journal, No. 29, Vol. VII. 8vo. 1914.
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Alchemical Society—Vol. II. Part 11, March 1914. 8vo.
American Geographical Society—Bulletin, Vol. XLVI. No. 4. 8vo. 1914.
Asiatic Society, Royal—Journal for April 1914. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIV. No. 5. 8vo. 1914.
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  Journal for April 1914. 8vo.
Dax, Société de Borda—Bulletin, 1913, Nos. 3-4. 8vo.
East India Association—Journal, Vol. V. No. 2. 8vo. 1914.
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WEEKLY EVENING MEETING,

Friday, May 15, 1914.

DR. DONALD HOOD, C.V.O. M.D. F.R.C.S., Vice-President,
in the Chair.

PROFESSOR FREDERICK KEEBLE, M.A. D.Sc. F.R.S.

Plant Animals: a Study in Symbiosis.

THE discourse treated of the bionomics of *Convoluta roscoffensis* and *C. paradoxa*, two marine Turbellarian worms, the bodies of which contain algal cells.

The relation between worm and alga is intimate—indeed, infection by the alga is a condition for the development of the animals.

The mode of infection of the body of larval *C. roscoffensis* has been followed in detail, and is described in memoirs contributed to the 'Quarterly Journal of Microscopic Science.'*

The infecting organ of *C. roscoffensis*—a species of *Chlamydomonas*—is a unicellular motile green alga. It is swallowed by the just hatched larva and transferred to vacuoles in the body, but instead of undergoing digestion like the other organisms ingested by voracious larval *C. roscoffensis*, the alga multiplies by division and forms a green assimilatory tissue beneath the skin of the animal. With the development of this tissue the animal ceases to ingest solid food, and relies entirely on the algal cells for supplies of carbohydrate and of protein. The green cells of the body of *C. roscoffensis* lose the power of independent existence, and each generation of *C. roscoffensis* is infected anew by free-living green cells.

The waste nitrogenous substances produced during the metabolism of the animal serve as the raw materials from which the green algæ of the body of *C. roscoffensis* manufacture amino-compounds and proteins, which substances serve for the nutrition of both animal and alga.

The discourse dealt also with the various phenomena of periodicity exhibited by the plant-animals, and showed how these phenomena enable the convolutas to maintain their several stations on the sea shore.†

[F. K.]

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* See also the volume entitled Plant Animals (Cambridge Manuals of Science).

† Op. cit.

WEEKLY EVENING MEETING,

Friday, May 22, 1914.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Robert Mond, Esq., J.P. M.A. F.R.S.E. M.R.I.

The Mortuary Chapels of the Theban Nobles.

When Achilles, in the ninth book of "Iliad," told of

> "Hundred-gated Thebes where twice ten score in martial state
> Of valiant men with steeds and cars march through each massy gate,"

this famed capital of one of the mightiest Empires of the East could look back on a thousand years of checkered history.

The mighty sovereigns of the 12th Dynasty who had, by the conquest of Nubia, extended the boundary of Egypt from the First to the Second Cataract, required a more central residence for the administration of the country than "white-walled Memphis," situated at the head of the Delta, and they therefore selected a village in the fertile plains which stretched from Erment to Keneh (the modern Luxor), to become the site of their capital. Here they erected the first great Temple to Amen, the local Harvest God, who, under the double appellation of Amen Ra, was later to become the principal divinity of Egypt. Their great irrigation schemes, which added the Province of the Fayum, their expeditions on the Red Sea to the sacred land of Punt, their re-opening of the turquoise mines of Sinai, and their contact with the coast of Asia Minor and the Greek Islands, marked the commencement of the rise of that Egyptian power which, in the next two thousand years, left an indelible mark on the evolution of civilization and the culture of the world.

The internecine struggles of the succeeding dynasties, which ended in the conquest of Egypt by the Canaanitic tribes, known as the Hyksos, kept Egypt in a state of suspended animation, until, at the end of the forty-five years' war of liberation, "Ahmes," the founder of the 18th Dynasty, after the final siege and capture of the great Hyksos fortress of Avaris and his re-conquest of Nubia, finally re-established Thebes as his capital. His successors, especially

Amenhotep II. and Thotmes III., the Napoleon of Egypt, rapidly extended its boundaries to the banks of the Euphrates on the south, and including Phœnicia and Palestine as far as the Tauros Mountains and the frontier of Armenia in the north, and brought Egypt in close connection with the Mediterranean Islands.

For a few years during this dynasty Thebes suffered an eclipse when Amenhotep IV., renouncing the worship of Amen, established a new capital at Tell el Armarna, which scarcely survived his reign, and under his successors, Thutaneh Amen and Hor-em Heb, both Thebes and its god Amen regained full sway.

This period was contemporary with the great or second Minoan period of Crete, the period of Mykena, and of the great development of the Hittites and Babylonians and Mitanni. Assyria was beginning to rise to power.

During the 19th and 20th Dynasties the power of Egypt suffered gradual eclipse, in spite of the efforts of their rulers, the Setis, Ramses the Great, and Ramses the Third. Their reigns saw the Biblical Exodus, and the destruction of Crete and Mykena by the Dorians, and the subsequent irruption of the Cretans into Philistina and Palestine.

Henceforward the King-Priests of Amen ruled independently at Thebes, whilst Lower Egypt was under the sway of dynasties which came from the Delta and Lybia, with the exception of the 25th Dynasty, which came from Nubia, and fought with varying success against the great Assyrian invaders until Assur Bani Pal conquered Egypt and sacked Thebes, a blow from which that city never recovered.

After Psamthek had defeated the Assyrians, Egypt again fell a prey to the Persians under Cyrus, whose rule was ended by the defeat of Xerxes by Alexander the Great.

Ptolemy I. then obtained Egypt as his share of the division of Alexander's Empire, until at last Egypt became a Roman province under Ptolemy XV and Cleopatra.

This very condensed excursion into the history of Egypt has been thought necessary to indicate the importance of the rôle which Thebes has played in the world's history, to explain some of the gaps we find in the historical continuity of its monuments, and especially to emphasize the vital interest which appertains to all contemporary monuments which are yet extant or likely to be discovered.

It was the first occupation of every Egyptian, grown to man's estate, to provide for his soul or spirit an imperishable dwelling-place, consonant with his wealth and dignity, and well furnished, by means of pictorial representations, with those amenities which had constituted his ambitions and delights during life. Hence the due study of these monuments, these sacred mortuary chapels, equips us with invaluable evidence of the history, art, manners, and customs of those far-off days.

Before proceeding to a more detailed examination of these mortuary chapels, disclosed to us by the devoted work of the great Egyptologists of the last century, a few words must be given to the origin and the selection of the site of the great Theban Necropolis.

The great line of fracture through the Cretaceous Limestone Plateau, which stretched from the Archean Mountain Chains of the Arabian Desert far into the Saharan Desert, became the path by which the Nile found its way to the Mediterranean. Irregularly subsided blocks along the edge of the great line of faults constitute a series of foot-hills, while deep canyon-like valleys have been eroded in the plateau itself. The Cretaceous rocks consist of layers of varying thickness of cherty limestones separated by bands of shale. These foot-hills have determined the site of the cemetery, and the formation of the rocks the individual position of the mortuary chapels.

From very early days it had been the custom in Egypt for the nobles and great officials to cause their funeral chapels and burial places to be constructed in the vicinity of those of their kings or rulers. The sovereigns, who were deified after death, had great temples erected in close proximity to their burial place, which, up to the time of the 12th Dynasty, was protected by a structural tumulus, known to us as a pyramid. The sovereigns of the 18th, 19th, and 20th Dynasties, however, selected sites for their burial chambers in the deep-cut valleys in the great limestone plateau, but placed the funeral temples in close proximity to the water and cultivated land on the edge of the Nile valley. The Theban Nobles placed their mortuary chapels and burial places on the hills, close to and overlooking the mortuary temples of their rulers. Hence, visitors to Thebes will find that from the great mortuary temple of Ramses III at Medinet Habu, on the south, to the temple of Sete I at Drah Abu Nagah, three miles to the north, these hills are honeycombed with the openings of these chapels, while the intervening slopes are pitted with minor tombs.

The problem which faced the Egyptian architect who was instructed to provide a dignified mortuary chapel for a high-placed and wealthy Egyptian noble or official in one of these hills, the right of burial in which was only granted by royal favour, was of a three-fold nature. In the first place, the mortuary chapel had to face the rising sun (hence, only the eastern face of the hill was available). Secondly, a suitable layer of rock had to be found which was of the necessary thickness for the proposed excavation. Thirdly, as the inviolability of the mummy was essential for its resurrection, a safe and abiding resting place had to be provided in close connection with the chapel, or in the rocky surface of the hill in its vicinity. A site having been allotted on the eastern slope on one of the hills, the first step would be to excavate a section so as to obtain a good face of rock, thus constituting a courtyard.

In certain cases the excavation had to be deepened and a sunk courtyard made to obtain a proper section. This rock-face and the other walls of the courtyard were in some instances decorated with statues of the deceased, memorial tablets or stela and colonnades either rock-cut or built in imitation of the court of a temple. The jambs and architrave of the main door situated in the centre of the rock-face was also frequently decorated with the name and title of the deceased. A short passage gave access to a rectangular chamber, the longer side parallel to the rock face and approximately of the same length. When the width of a chamber exceeded two metres, square columns were left to support the roof. From this chamber, in the more ambitious chapels, a passage frequently decorated with hunting scenes opened facing the main door, which either ended in a niche or led into a large square chamber, at the end of which would be either a niche or small chapel containing the statue of the deceased. In some of the larger chapels these halls opened into smaller chambers destined to receive the offerings made at the funeral celebrations. The coffin or sarcophagus containing the embalmed remains was placed in an underground chamber, to which access was gained either by sloping passages or vertical shafts. The entrances to these passages or shafts were carefully dissimulated, but they were usually made in the floor of the main passage or one of the side chambers, or on the right-hand side of the courtyard, so that the chamber with which the shaft communicated would be situated below the chapel. False or unused pits were also employed to mislead unscrupulous grave robbers, who have abounded in Egypt ever since it became customary to bury valuables with the deceased. These rough excavations, made with chert, flint, and bronze tools, having been completed, the architect's next task was to prepare the surface of the walls for the decoration by the sculptors and painters. As previously mentioned, the character of the rock varies considerably, and, where the quality of the rock permitted, sculpture and painting were applied directly on it, and flaws were mended with lime-plaster and the inserting of pieces of limestone; but when the surface was not sufficiently smooth or homogeneous to lend itself for the purpose of bas-reliefs or the painting being directly applied to the stone, either walls of sun-dried brick were built, or a mud plastering mixed with chaff was applied to the rock wall.

These sun-dried bricks owe their peculiar strength and toughness to the fact that the fine silt which is deposited from the Nile floods, on being mixed with water in which straw chaff has been fermented, acquires the property due to some colloidal bodies extracted from the straw of a highly plastic clay which sets extremely hard on being dried in the sun. This affords an explanation of the complaints of the Hebrews—as recorded in Chapter 5 of Exodus—who were engaged in the erection of the granaries of the cities of Ramses and Pithom, that they could not make bricks without straw.

With very few exceptions the mummy-pits and their chambers were left entirely undecorated, in marked contrast to the elaborate and beautiful scenes of a mystical and ritual character which are found in the tombs of the kings. The sculptor provided seated and standing figures either cut from the living rock, or carved in lime or sandstone or granite for placing in the niches. The flat surfaces of the walls, which lent themselves to this treatment, were decorated in low relief of the champlevé type. In a few cases we find the reliefs modelled in stucco and then painted. Where the walls are painted, the colours are applied to a lime-wash laid on the surface. The usual Egyptian colours, the different iron ochres, ground copper glazes, rose-madder and lamp-black, were put on in flat washes, with ordinary gum-arabic as a binding material. As this work had to be done in dark chambers by means of oil-lamps, we only occasionally find an attempt at modelling.

The text and scenes depicted on the walls, varying with the exigencies of space and the individual taste of the owner and of the artist, in most cases represent :—

1. The bestowal of honours on the deceased by the king.
2. The representation of the deceased occupied in his daily avocations, surrounded by his family, and supervising his servants.
3. A stele, or tablet, containing, besides invocations to the gods, generally some important biographical notice.
4. A hunting or fishing scene.
5. The deceased, generally accompanied by his wife, adoring Osiris.
6. The funeral banquet.
7. A number of religious texts, including the gifts and funeral furniture provided for the interment, the weighing of the heart, the revivification of the mummy, and lists of the offerings to be made to the deceased at the funeral festivals.

The ceilings were decorated either with a five-rayed star in yellow on a dark blue ground, or with beautiful geometrical patterns in various colours, containing frequently one or more lines of inscriptions.

When the architect and his assistants completed their task, we may feel sure that the owner would show the same to his friends with pride and gratification, as we frequently find small inscriptions made by subsequent visitors expressing their satisfaction at their visit. When seventy days after his death, the mummy of the owner was brought at night on sledges after a real or fictitious journey to Abydos, where he had shared some little time the honour of temporary burial near the tomb of the god Osiris, before finally being deposited in its underground chamber, a mystery play was performed. Even as the great god Osiris, after his cruel murder and dismemberment by his brother Set, had been, by the assiduous attentions of his son, the Sun-god Horus, Anubis, the god of the Dead, and of his wife and her sister, the godesses Isis and Nephtis, brought back to life to rule as King of the Dead over the beautiful land of Amenti, the same ceremonies were performed over the mummy by the son, his best friend, the priest and the two professional weeping women, who, adopting the traditional costume, attributed to the gods and goddesses they represented, and repeating the same magic formularies, hoped and believed that the deceased would be enabled by their aid to come to life and enjoy the glories of the kingdom of Osiris. This ceremony would be followed by a funeral feast, partaken of in the presence of the statue of the deceased, at which a harper would extol the deeds of the deceased, and make philosophical reflections on the shortness of life and of fame, extremely similar to those we are conversant with in the Rubayat of Omar Khayam. The feasts were repeated at regular intervals.

Besides individuals, speculative undertakers also acquired considerable tracts of the cemetery for the burial of the less well-to-do. The cemetery was guarded by a special police force, and inspectors from time to time visited the chapels and recorded their visits on the walls. As their exposed position and accessibility constantly rendered them liable to damage, they suffered severely from both human and natural causes. The iconoclasts under Amenhotep IV, when he tried to suppress the worship of the god Amen, had the name of Amen and the face of all such whose name included the word Amen carefully destroyed. Others were destroyed through private spite. This risk continues down to the present from either a dismissed employee or someone having a grievance against the authorities or an individual.

These chapels were frequently re-used by subsequent owners in later epochs, who either enlarged or reduced the size of the chambers, and obliterated the old decorations partly or completely with lime-wash, and had them repainted.

Again, the Monks of the Thebaid obliterated the graceful figures of the ladies and goddesses with mud-wash, and frequently disfigured the scenes with grotesque figures of animals or similar subjects, and when in later periods the belief in the Ka or spirit had degenerated into a dread of Ghosts, the fellah attempted to guard himself against the supposed evil influences by obliterating the nose, eyes, and mouth of the figure and statues he found, even as he does to-day. In a number of cases the courtyards and the entrances to the chapels had been so completely obliterated owing to subsequent excavations of tombs on a higher level, or the collapse of retaining walls, which had been built for the shoring up of the debris, that their existence was unrecognized, thus preserving them from further damage. The scenes have also suffered through the collapse of the roofs during earthquakes, through the grinding of the walls by bats, through wasps and hornets making their nests on the mud plaster and the detachment of this latter from the walls, through holes being cut in the walls by plunderers, through smoke and dirt caused by their continuous inhabitation even up to the present time, and through the attempts to detach scenes or fragments of scenes for sale to Museums and tourists.

The work of preservation and restoration divides itself into three parts :—

1. The expropriation of natives who are living in the tombs.
2. The preservation of such tombs as are already exposed.
3. The excavation of those which are still buried.

With the able assistance of the Chief Inspector of Upper Egypt, Mr. Howard Carter, and, more recently, Mr. Weigall, and also that of the native inspector, Rushdi Effendi, who has conducted the actual negotiations, the work of expropriation has proceeded smoothly, and the natives have transferred their abode from rock-cut chapels to new houses which they have built for reasonable compensation—an arrangement which has satisfied both expropriee and expropriator. Where ceilings, walls or columns have collapsed or been removed, these have been replaced, as far as necessary for the structural stability, by stone walls, brick arches or girders. The replacing of such fragments or destroyed sculptured walls or detached portions of painted mud brick, and the securing of such portions as may have become loosened, is steadily being proceeded with. Copper clips or studs, plaster-of-Paris cement, either alone or mixed with glue, and similar substances, are used for carefully refixing the detached fragments. Wherever possible, casts of fragments preserved in European museums are being obtained, to be replaced in the position from which they originally came. Partly and completely buried tombs are being systematically and carefully excavated, the courtyards are being surrounded with walls to prevent further débris from falling into them, and all chapels which contain sufficient detail to render their accessibility advisable are being fitted with iron gates, with wire netting to prevent bats and wasps having access to the interior. A monthly system of inspection has been instituted, and by these means their safety during the immediate future is effectually provided for. To prevent the abrasion of the colours on the walls, various substances, such as Kessler's magnesium fluo-silicate, a solution of cellulose in acetone, white of eggs, and similar bodies, have been and are being exhaustively tested, as the excessive dryness of the climate and equability of temperature permit the use of a wider range of substances than are permissible in Europe. For instance, cellulose solution applied some ten years ago when last examined had still retained the freshness of the colour, and its surface could not be marked with a finger-nail. All varnishes, even those employed by the ancient Egyptians, have a tendency to become brown. Some of the frescoes to which they have been applied have suffered in consequence. Care is also being taken when repairing a tomb to guard against the access of too much light so as to try and preserve the pristine freshness of its colours. As in spite of all these precautions, no absolute guarantee can be assured for the indefinite preservation of these records for posterity, a complete photographic survey of the scenes represented in these tombs is now being made. In order to solve the difficulty of obtaining a satisfactory set of negatives accurately to scale in these dark narrow passages, Mr. Sanger Shepherd has constructed to my design a camera supported on a large and extensible optical bench, provided with electric lamps and lighted by a small portable electric station Two native boys, whose training was kindly undertaken by Professor Reisner of Harvard, have, under the supervision of my coadjutor and assistant, Mr. Ernest Mackay, who I am pleased to say has been able to return from Egypt in time to assist me this evening, produced the composite pictures you see before you. The principle of this apparatus is very simple. The camera is mounted on an adjustable table, which slides up and down a vertical tubular mast, adjustable as to length, and fixed to a heavy casting provided with four grooved gun-metal rollers, running on tubular rails; these are also adjustable for length. The camera, which is designed to take quarter plates, is provided with a lens of sufficient depth of focus to require no adjusting for the irregularities in the wall surface. An aluminium rod or pointer permits the camera being focused automatically. The table also supports two semitubular reflectors in which incandescent electric lamps are symmetrically placed. The generating station consists of a small petroleum motor of 2½ H.P., connected with a belt to a small dynamo, which is placed on a light bed-plate and provided with a switchboard and resistances; the whole arrangement, weighing about two cwt., being easily transportable. After some preliminary adjustments, the apparatus is now working satisfactorily, and I am looking forward at some subsequent date to be able to deposit the properly catalogued collection of negatives in some public institution, so that a permanent and authentic record may be preserved for the benefit of future students.

Although we have a few tombs of the sixth and the twelfth dynasties, the great majority date from the eighteenth, and although the nineteenth and twentieth are well represented, there are only a few of a subsequent date; but, as was pointed out in the short historical introduction, it is just this period which marked the highest tide of Egyptian influence and culture both at home and abroad, and hence information gathered from these tombs, partly of an historical and biographical, partly of a statistical nature—such as the representation of the foreigners bringing tribute, or the Grand Vizier presiding in the law courts, or the foreign secretary transacting business in the foreign office, or a high official superintending the transport of an obelisk, the overseer of the armoury superintending the manufacture of arms, or the overseer of the treasury checking the weighing out of gold, electrum, and silver, and superintending the metal workers, or the many high officials connected with the temples of Egypt in their corresponding functions—provide us with a valuable and much more authoritative material than the grandiloquent texts and scenes with which the kings represented their exploits on the temple walls.

The close intercourse with Asia Minor and the Mediterranean Islands and the large influx of prisoners of war from the successful campaigns in these countries, did much to colour and affect Egyptian life and art. The acquisition of the Assyrian language, then the language of diplomacy, and the inter-marriage of Egyptian reigning families with those of the countries of Asia Minor, still further strengthened this connection. All these factors have got to be taken into account if a true appreciation of Egyptian culture and Egyptian art is to be arrived at; and when we consider how in a later age the great philosophers for whom Greece was famous spent many years of study in Egypt, and frequently acknowledged their indebtedness for the knowledge gained there, we may assume that it was during these dynasties and under the influence of the men whose mortuary chapels we are studying that this knowledge first took definite shape. A close study of the art and sculpture of this period should enable us, by the examination of the large amount of material now at our disposal, to find a personal equation of this great art period which hitherto has been strangely lacking. To those of us who have been brought up in the study of the individuality of the world's great artists, it must appear both strange and disquieting that a great art epoch of the world should have existed without disclosing any distinctive personal touch, and I trust that subsequent study will disclose the necessary clues.

The colour photographs, of which some have been taken direct by Mr. Mellor and myself, and others are reproductions of faithful copies made by Mrs. Davies and Mr. Lindon Smith of the scenes in these chapels, will give an idea of the perfection at which this art has arrived.

We are yet far from fully understanding and appreciating the wonderful heritage which has been preserved for us in these chapels. Hence, whilst we should be filled with gratitude that notwithstanding the vicissitudes of time, so many of these relics of the past should have survived to our day for our reverent care and study, the more should we be imbued with the feeling that it is a duty we owe to posterity to preserve, to the best of our ability, these relics which none of our endeavours will ever enable us to replace.

It must be a source of great satisfaction to all those who have a real interest in the advancement of knowledge that this duty is becoming rapidly and more fully recognized. Those of us who have had an opportunity of observing the meticulous care with which the present excavations are being conducted in Rome and Pompeii, and the magnificent work done by the Antiquities Department of the Egyptian Government by Mr. Legrain, Barsanti, and others under the guidance of Sir Gaston Maspero, and by the Egypt Exploration Fund by Professor Naville and his band of able and enthusiastic workers, in excavating and restoring the great temples to which they have devoted their attention, will be encouraged and inspired by the results which they have obtained.

Our knowledge of the Theban Necropolis dates from Napoleon's great mission to Egypt, and their contents have been described by Champollion, Mariette, Prisse d'Avenne, Sir Gardiner Wilkinson, Rhind, Lepsius, De Morgan, and the members of the French Archæological Mission, besides a whole host of other indefatigable workers. We owe it to Professor Newberry that the first steps were taken for the preservation of these tombs. It is now some thirteen years since I began collaborating with him, and when, in consequence of other duties, he had to relinquish this work, he generously transferred his concessions to me. My own excavations covered a period of three winters, and subsequently, when other duties prevented my continuing the work personally, Messrs. Howard Carter and Weigall, the successive Inspectors of Antiquities of Upper Egypt, continued the task with the assistance of myself and of others, especially Prince Djemil Pasha Toussoum, the Metropolitan Museum of New York, and a number of the native inhabitants of the town of Luxor, with the greatest self-devotion and enthusiasm.

During the winter of 1909, Mr. Jelf, whom I sent out to assist Mr. Weigall, rendered valuable services, and Dr. Alan Gardiner having become equally imbued with the importance of our task, enthusiastically supported it with his personal work and money during the last few seasons.

The monumental topographical catalogue, which it has been my privilege to assist Dr. Gardiner, working in collaboration with Mr. Weigall, to publish, is a record of what has been accomplished.

They have increased the large number of chapels thus cleared in the last twelve years from some 30 to 250, and their exposure to the action of light and attrition due to frequent visits, rendered a well-considered and continuous scheme of restoration and preservation a matter of immediate urgency.

This object, which had been one of my day-dreams for many years, took shape when the opportunity arose which enabled me to secure, after many consultations with the most competent Egyptologists, and especially with Dr. Alan Gardiner, the services of Mr. Ernest Mackay, for many years chief assistant of Professor Flinders Petrie, who will now devote his whole time to the systematic inspection, excavation, restoration, and preservation of these chapels.

During the past year he has already succeeded in accomplishing most notable results, as will be apparent to you from the photographs which he has taken and which I have submitted to you to-night.

I have also the most sincere appreciation of the kindness of Sir Gaston Maspero and the Egyptian Government Service for the liberality with which they have granted me facilities and unhampered liberty to prosecute this work.

[R. M.]

WEEKLY EVENING MEETING,

Friday, May 8, 1914.

Charles Hawksley, M.Inst.C.E., Vice-President,
in the Chair.

Professor Karl Pearson, F.R.S.

Albinism in Men and Dogs.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, May 29, 1914.

Donald W. C. Hood, C.V.O. M.D. F.R.C.P.,
Vice-President, in the Chair.

Professor Jagadis Chunder Bose, M.A. D.Sc. C.S.I. C.I.E.,
Professor, Presidency College, Calcutta.

Plant-Autographs and their Revelations.

There are professors of sciences bordering on the mystical who declare that they can discriminate the character and disposition of any one simply by a careful observation of his handwriting. As to the authenticity of such claims scepticism is permissible; but there is no doubt that one's handwriting may be modified profoundly by conditions, physical and mental. There still exist at Hatfield House documents which contain the signatures of no less a person than the historical Guy Fawkes of Gunpowder Plot celebrity. And those who have seen them declare that there is a sinister variation in these signatures. The crabbed and distorted characters of the last words Guy Fawkes wrote on earth—as in the dark hours of the morning on which he was executed he set his hand to the written confession of his crime—tell their own tale of what had transpired in the solitary imprisonment of that fateful night.

Such, then, is the history that may be unfolded to the critical eye by the lines and curves of a human autograph. Under a placid exterior, there is also a hidden history in the life of the plant. Storm and sunshine, warmth of summer and frost of winter, drought and rain, all these and many more come and go about the plant. What coercion do they exercise upon it? What subtle impress do they leave behind? Is it possible to make the plants write down their own autographs, and thus reveal their hidden history? Were this possible, the fact would be fraught with far-reaching consequences.

For about the life-reactions of plants, there are contending and irreconcilable hypotheses. Does the plant, like the animal, give an answering twitch to an external shock? Is there any possible relation between plant-life and our own? On these points very little is definitely known. For numerous are the experimental difficulties which confront and baffle the investigator.

One school of thinkers, by far the most numerous, would have us believe that some of the most characteristic reactions in the animal are not to be found in the plant; for example, it is urged that, unlike the animal, the majority of plants are insensitive to a blow, exhibiting no shuddering twitch, either mechanical or electrical; and that even in the sensitive *Mimosa*, an irritation does not cause an excitatory impulse, but a mere hydraulic disturbance. The pendulum then swings from these hasty assumptions to the diametrically opposite extreme. Under these circumstances the clear path is that which leads us away from theory and disputations to find the thread of fact. We must, therefore, abandon all our preconceptions, and put our questions direct, insisting that the only evidence which can be accepted is that which bears the plant's own signature.

How are we to know what unseen changes take place within the plant? If it be excited or depressed under some special circumstance, how are we, on the outside, to be made aware of it? The only conceivable way would be, if that were possible, to detect and measure the actual response of the organism to a definite testing blow. When an animal receives an external shock, it may answer in various ways; if it has voice, by a cry; if it is dumb, by the movement of its limbs. The external shock is the stimulus; the answer of the organism is the response. If we can find out in the plant the relation between the stimulus and response, we shall be able to determine its state of vitality at the moment. In an excitable condition, the feeblest stimulus will evoke an extraordinarily large response; in a depressed state even a strong stimulus evokes only a feeble response; and, lastly, when death has overcome life, there is an abrupt end of the power to answer at all.

We might, therefore, have detected the internal condition of the plant, if we could have made it write down its responses. In order to succeed in this, we have, first, to discover some compulsive force which will make the plant give an answering signal; secondly, we have to supply the wherewithal for an automatic conversion of these signals into an intelligent script; and, last of all, we have ourselves to learn the nature of the hieroglyphic.

RESPONSE OF PLANT AND ANIMAL.

In answering the question whether there is a fundamental unity in the response of plant and animal, we have first to find out whether sensitiveness is characteristic of only a few plants or whether all plants and every organ of every plant is sensitive. Then we have to devise apparatus by which visible or invisible reactions are detected and recorded. Having succeeded in this, we have next to survey the characteristic reactions in the animal, and find out whether phenomena corresponding to these may also be discovered in the plant.

Thus, when an animal is struck by a blow, it does not respond at once. A certain short interval elapses between the incidence of the blow and the beginning of the reply. This lost time is known as the latent period. In the plant is there any definite period which elapses between the incident blow and the responsive twitch ? Does this latent period undergo any variation as in the animal, with external conditions ? Is it possible to make the plant itself write down this excessively minute time-interval ?

Next, is the plant excited by various irritants which also excite the animal ? If so, at what rate does the excitatory impulse travel in the plant ? Under what favourable circumstances is this rate of transmission enhanced, and under what other circumstances is it retarded or arrested ? Is it possible to make the plant itself record this rate and its variation ? Is there any resemblance between the nervous impulse in the animal and the excitatory impulse in the plant ?

The characteristic effects of various drugs are well known in the case of the animal. Is the plant similarly susceptible to their action ? Will the effect of poison change with the dose ? Is it possible to counteract the effect of one poison by means of another ?

In the animal there are certain automatically pulsating tissues like the heart. Are there any such spontaneously beating tissues in the plant ? If so, are the pulsations in the animal and the plant affected by external conditions in a similar manner ? What is the real meaning of spontaneity ?

Growth furnishes us with another example of automatism. The rate of growth in a plant is far below anything we can directly perceive. How, then, is this growth to be magnified so as to be rendered instantly measurable ? What are the variations in this infinitesimal growth under external stimulus of light and shock of electric current ? What changes are induced by giving or withholding food ? What are the conditions which stimulate or retard growth ?

And, lastly, when by the blow of death life itself is finally extinguished, will it be possible to detect the critical moment ? And does the plant then exert itself to make one overwhelming reply, after which response ceases altogether ?

PLANT-SCRIPT.

We shall first take up the question of recording response of a plant like *Mimosa*. Here, at the joint of the leaf, there is a cushion-like mass of tissue known as the pulvinus. This serves as the motile apparatus. The swollen mass on the lower side is very conspicuous. Under excitation, the parenchyma in this more effective lower half undergoes contraction, in consequence of which there is a fall of the leaf. This sudden movement constitutes the mechanical response of

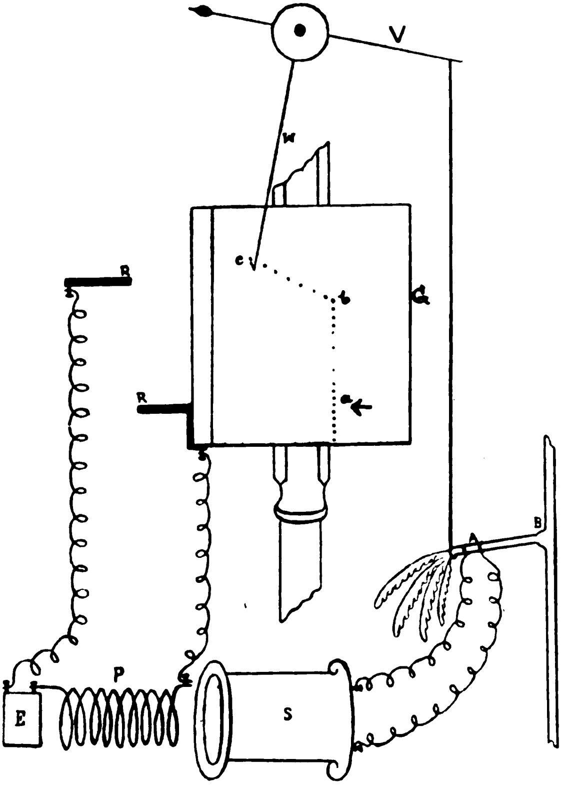

Fig. 1.—Diagrammatic Representation of Plant-Recorder. Responding leaf attached to one arm of lever V, at the fulcrum of which is attached W, the writer. G, sliding smoked glass plate for record. Recording plate is lifted and allowed to drop. At a definite position during fall, R makes momentary electric contact with R′, giving rise to instantaneous electric shock at A. Moment of application of stimulus marked on recording plate by arrow *a*; arrival of excitation at B causes fall of leaf, which pulls the writer to left describing *b c*. For determination of latent period, stimulus is applied on the pulvinus at B.

the leaf to the impinging stimulus, just as the contractile movement of a muscle in similar circumstances forms its characteristic mechanical response. For obtaining a record, the leaf of ***Mimosa*** is attached to one arm of a lever, V; the other is loaded with a small weight, which acts as a counterpoise. A long wire, W, bent at the tip, is placed at right angles to the lever, and serves as a writer. The tip of this writer touches a smoked-glass plate, which is allowed by means of a clock-work to fall at a definite rate (Fig. 1). An instantaneous electric shock is applied on the leaf-stalk at A. The excitation will, after a time, be propagated from A to the responding pulvinus at B, inducing the responsive fall of the leaf. After a definite period the leaf recovers from excitations and is re-erected. A complete curve of response is thus obtained in which the ordinate *a b* represents the intensity of excitation, and the abscissa *a c* the period of complete recovery (Fig. 2). Any condition which increases excitability will also enhance the amplitude of response. Depression, on the other hand, is attended by a diminution of response. At death the response is altogether abolished. Thus, by means of testing blows, we are able to make the plant itself reveal those invisible internal changes which would otherwise have entirely escaped us.

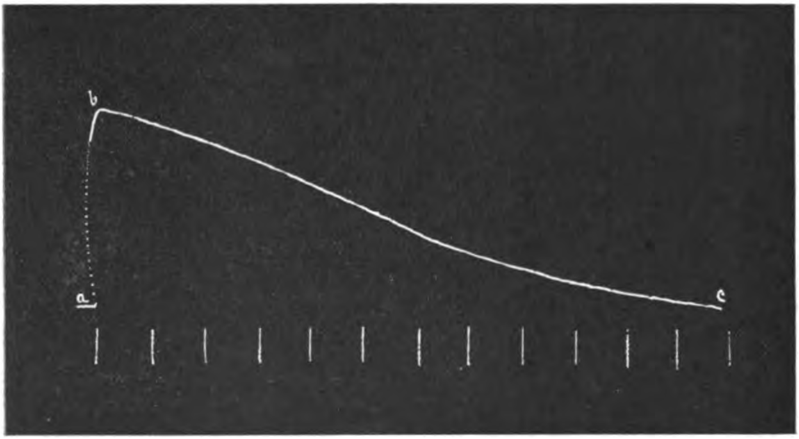

FIG. 2.—RESPONSE-CURVE OF PRIMARY LEAF OF *Mimosa*.
The vertical lines below the record indicate intervals of one minute each.

The above is a description of the theoretical method of obtaining response of the plant. In practice numerous difficulties have to be overcome. In the case of muscle-contraction, the pull exerted is considerable and the friction offered by the recording surface constitutes no essential difficulty. In the case of plants, however, the pull exerted by the motile organ is relatively feeble, and in the movement of the very small leaflets of *Desmodium gyrans*, or the Telegraph plant, for instance, a weight so small as four-hundredths of a gram is enough to arrest the pulsation of the leaflets. Even in the leaf of *Mimosa* the friction offered is enough to introduce serious errors into the amplitude and time-relations of the curve. This error could not be removed as long as the writer remained in continuous contact with the writing surface. I was, however, able to overcome this difficulty by making an intermittent, instead of a continuous, contact. The possibility of this lay in rendering the writer tremulous. Fresh difficulties arose which were finally eliminated by an invention depending on the phenomenon of resonance.

The Resonant Recorder.

The principle of my Resonant Recorder depends on a certain phenomenon, known as resonance or sympathetic vibration. In illustration of this we may construct an artificial ear tuned to a definite note. The drum of the artificial ear is made of thin soap-film ; a beam of light reflected from its surface forms characteristic pattern of colour on the screen. To various cries this ear remains deaf, but the apathy disappears as soon as the note to which the ear is tuned is sounded at a distance. On account of sympathetic vibration the artificial ear-film is thrown into wildest commotion, and the hitherto quiescent colour pattern on the screen is now converted into a whirlpool of indescribably gorgeous colour of peacock green and molten gold.

In the same manner, if the strings of two violins are exactly tuned, then a note sounded on one will cause the other to vibrate in sympathy. We may likewise tune the vibrating writer V, with a reed C (Fig. 3). Suppose the reed and the writer are both tuned to vibrate a hundred times per second. When the reed is sounded the writer will also begin to vibrate in sympathy. In consequence of this the writer will no longer remain in continuous contact with the recording plate, but will deliver a succession of taps a hundred times in a second. The record will therefore consist of series of dots, the distance between one dot and the next representing one-hundredth part of a second. With other recorders it is possible to measure still shorter intervals. It will now be understood how, by the device of the Resonant Recorder, we not only get rid of the error due to friction, but make the record itself measure time as short as may be desired. The extraordinary delicacy of this instrument will be understood when by its means it is possible to record a time-interval as short as the thousandth part of the duration of a single beat of the heart. The complete apparatus for obtaining plant-record is shown in Fig. 4.

COMPARISON BETWEEN SENSITIVENESS OF MAN AND PLANT.

We have next to find some method of stimulation which will not cause any mechanical disturbance to the plant. In connection with this I made an important discovery which demonstrates the identical characteristics of excitation in plant and animal. In the animal tissue a constant electric current causes very characteristic excitations at the moment of "make" or "break" of the current. In most cases there is no excitation during the continuation of the current. At the "make" excitation takes place only at the kathode; at the

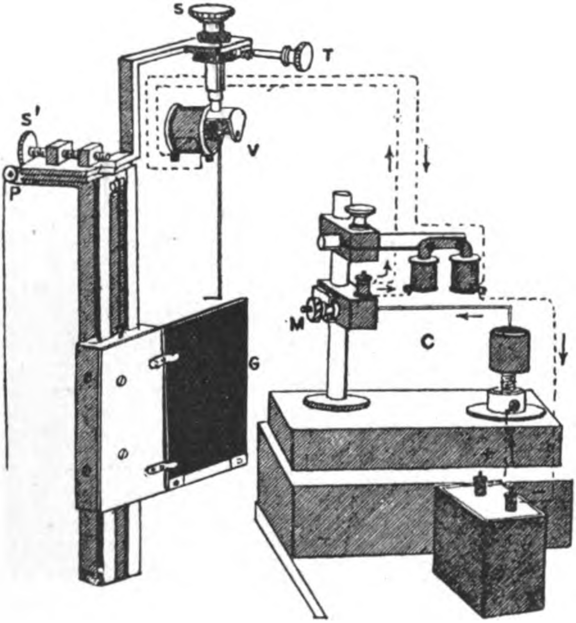

FIG. 3.—UPPER PART OF RESONANT RECORDER (from a photograph). Thread from clock (not shown) passes over pulley P, letting down recording plate. S', screw for adjustment of distance of writing-point from recording plate. S, screw for vertical adjustment. T, tangent screw for exact adjustment of plane of movement of recorder, parallel to writing-surface. V, axis of writer supported perpendicularly at centre of circular end of magnet. C, reed. M, micrometer screw for adjustment of length of reed.

"break" of the current, however, excitation is induced once more, but this time at the anode. These characteristic effects I find repeated also in the plant. At this point it is interesting to institute a comparison between the sensitiveness of a plant and a human being. The most sensitive organ by which an electric current can be detected is our tongue. An average European, according to Laserstein, can perceive by his tongue a current as feeble as 6·4 microamperes—a microampere being one-millionth part of the unit of current. This value might be subject to certain variation, de-

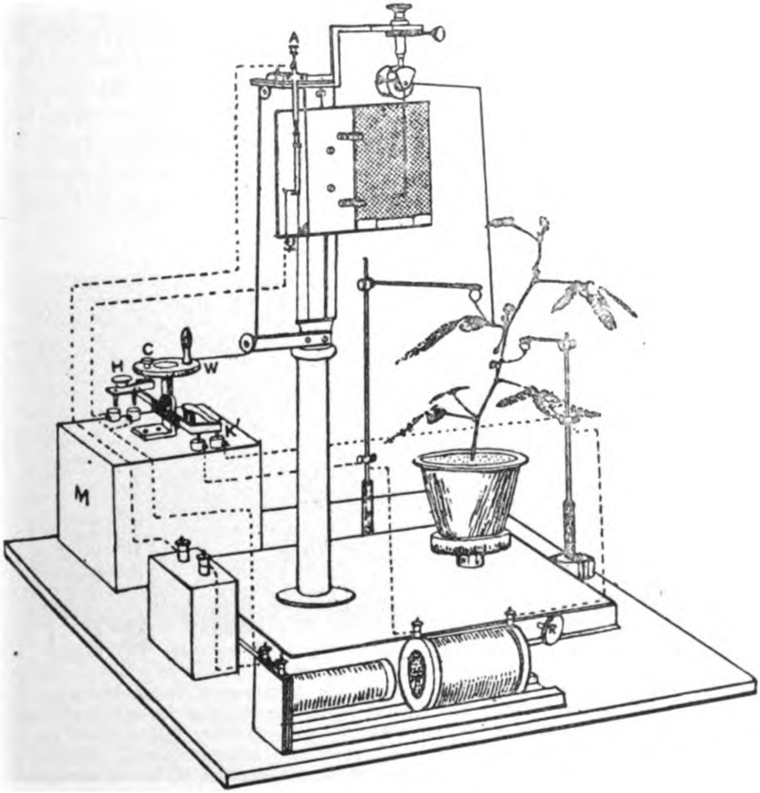

FIG. 4.—APPARATUS FOR DETERMINATION OF LATENT PERIOD OF *Mimosa*. M, spring motor. W, winding disc. C, projecting catch. H, release handle, pressure on which also completes primary circuit of induction coil. K', short circuit key. The automatic break consists of contact rod adjusted by micrometer screw A.

pending on racial characteristics. One might expect that the tongue of the Celt would be far more excitable than that of the stolid Anglo-Saxon. In any case the superiority of man has to be established on foundations more secure than sensibility ; for the plant *Biophytum*, I find, is eight times more sensitive to an electrical current than a human being. With regard to the stimulus of induction shock, *Mimosa* is ten times as sensitive. As with the animal so also with the plant, the effect of stimulus is additive, that is to say, effective stimulation is determined not only by the intensity, but also by the duration of application. In fact I have been able to establish in plants a strictly quantitative relation as regards the additive effect of sub-minimal stimulus, which is, that *the effective excitation is equal to individual intensity of stimulus multiplied by the number of repetitions*. In order that successive stimulations may be uniform, we have to assure ourselves that the duration of the tetanizing shock is maintained absolutely constant. This I am able to secure by means of the special device of automatic stimulator. The results of experiments to be presently described appeared so astonishing, that for many reasons it became highly desirable to remove completely all elements

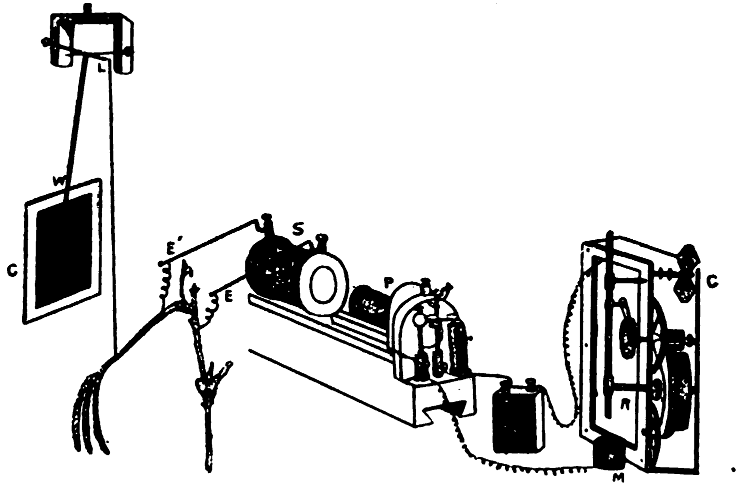

FIG. 5.—DIAGRAMMATIC REPRESENTATION OF AUTOMATIC PLANT-RECORDER. Petiole of *Mimosa*, attached by thread to one arm of lever L; writing index W traces on smoked glass plate G the responsive fall and recovery of leaf. P, primary, and S, secondary, of induction coil. Exciting induction shock passes through the plant by electrodes E, E'. A, accumulator. C, clockwork for regulating duration of tetanizing shock. Primary circuit of coil completed by plunging rod R dipping into cup of mercury M.

of personal equation. In fulfilment of this, I spent several years in perfecting various instruments by which the plant attached to the recording apparatus is automatically excited by successive stimuli which are absolutely constant. In answer to this it makes its own responsive records, goes through its period of recovery, and embarks on the same cycle over again, without assistance at any point from the observer (Fig. 5). In this way the effect of changed external condition is seen recorded in the script made by the plant itself.

THE SLEEP OF PLANTS.

In studying the effect of a given change in the external condition, an assumption has to be made that during the time of experiment there has been no spontaneous variation of excitability. Is the plant equally excitable throughout day and night? If not, is there any particular period at which the excitability remains uniform? Is there again a different time during which the plant loses its sensibility—going, as it were, to sleep? On these points no definite information has been available. The fanciful name of sleep is often given to the closure of leaflets of certain plants during darkness. These movements are brought about by variation of turgor, and have nothing whatever to do with true sleep; for similar closure of leaflets takes place under the precisely opposite condition of strong light.

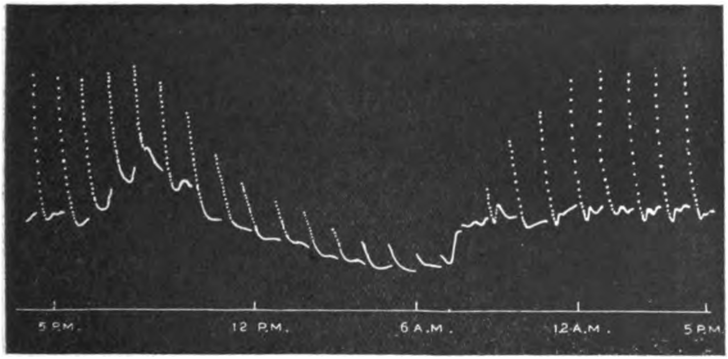

FIG. 6.—RECORD FOR TWENTY-FOUR HOURS, EXHIBITING DIURNAL VARIATION OF EXCITABILITY (spring specimen). The displacements of base-line are due to nyctitropic movements.

In order to find out whether *Mimosa* exhibits diurnal variations of sensibility, I made it record its answer to uniform questioning shocks, repeated every hour of the day and night. The amplitude of the answering twitch gave a measure of the "wakefulness" of the plant during twenty-four hours. The results obtained were quite unexpected. The plant is found to keep up very late, and fall asleep only at the early hours of the morning. It makes up for its late hours by gradually waking up by noon (Fig. 6). It then remains in a condition of uniform sensibility all the afternoon. This period of uniformity is chosen for investigations on the effect of changed external conditions on excitability.

EFFECT OF LIGHT AND TEMPERATURE.

Does the plant feel the depressing effect of darkness? The following record shows the effect of a passing cloud (Fig. 7). It is the sudden change which exerts a marked depressing effect. The plant partially regains its sensibility when accustomed to darkness.

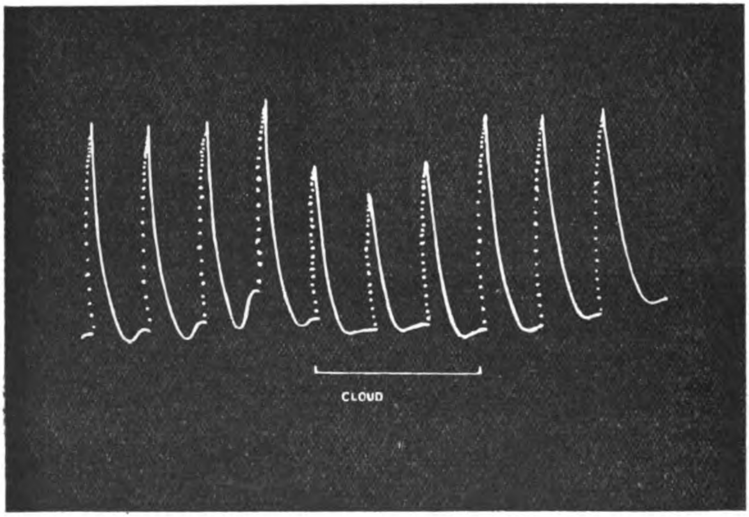

FIG. 7.—EFFECT OF CLOUD. Dotted up-curve indicates responsive fall, and continuous down-line exhibits slow recovery. First four responses normal; next three show depression due to diminution of light brought on by cloud, the duration of which is indicated by horizontal line below. Last three records show restoration of excitability brought on by clearing of sky. All records read from left to right.

When brought suddenly from darkness to light, there is also a transient depression followed by enhanced excitability.

Temperature has also a marked effect on excitability. Up to a critical point warmth increases excitability, the recovery being also hastened. Cooling conversely depresses excitability. The motile excitability is abolished at about 20° C.

EFFECT OF AIR, FOOD, AND DRUGS.

The plant is intensely susceptible to the impurities present in the air. The vitiated air of the town has a very depressing effect. According to popular science, what is death to the animal is supposed to be life for the plant; for does it not flourish in the deadly atmosphere of carbonic acid gas? The record (Fig. 8) shows that, instead of flourishing, the plant gets suffocated just like a human being. Note the gasp of relief when fresh air is introduced. Only in the presence of sunlight is the effect modified by photosynthesis. In contrast to the effect of carbonic acid, ozone renders the plant highly excitable. Sulphuretted hydrogen, even in small quantities, is fatal to the plant. Chloroform acts as a strong narcotic, inducing a rapid abolition of excitability. The ludicrously unsteady gait of

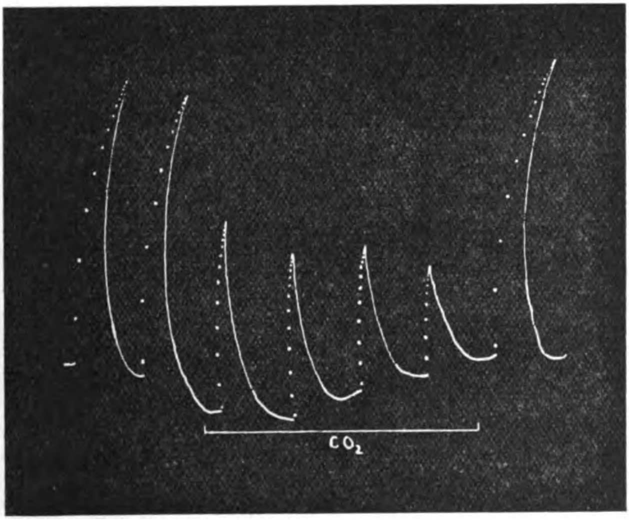

FIG. 8.—EFFECT OF CARBONIC ACID GAS.

the response of plant under alcohol (Fig. 9) could be effectively exploited in a temperance lecture! The next record is in the nature of an anticlimax, where the plant has drunk (pure water!) not wisely

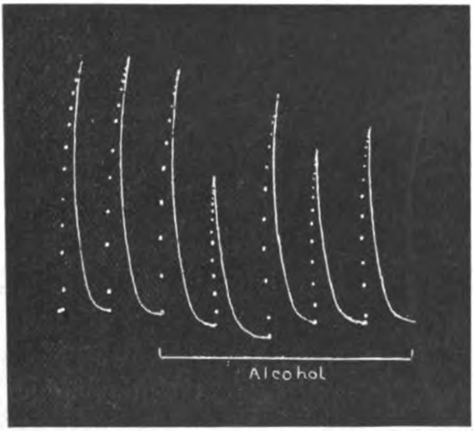

FIG. 9.—EFFECT OF VAPOUR OF ALCOHOL.

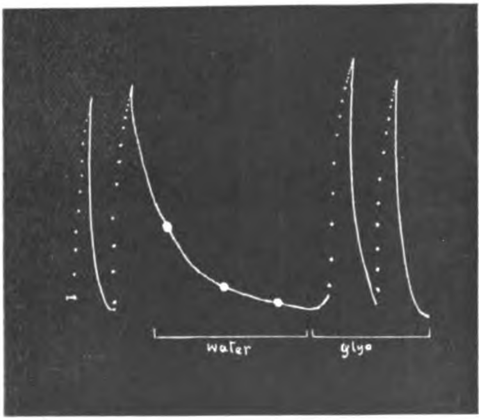

FIG. 10.—ABOLITION OF MOTILE EXCITABILITY BY EXCESSIVE ABSORPTION OF WATER, AND SUBSEQUENT RESTORATION BY WITHDRAWAL OF EXCESS.

but too well. The gorged plant is seen to have lost all power of movement. I was, however, able to restore the plant to normal condition by extracting the excess of liquid by application of glycerin (Fig. 10).

UNIVERSAL SENSITIVENESS OF PLANTS.

It may be urged that the various reactions of irritability may hold good only in the case of the particular plant *Mimosa*, and that the majority of plants are quite insensitive. I shall presently show that this view is quite erroneous. In *Mimosa* diffuse stimulation causes relatively greater contraction of the more excitable lower half of the pulvinus, and this differential action is magnified by the long petiolar index. Had the upper half of the pulvinus been equally excitable as the lower, then the antagonistic reactions would have balanced each other. In radial organs we do not observe any lateral movement as in *Mimosa*. This is not owing to insensitiveness, but to equal contractions on all sides balancing each other. The shortening of length of various radial organs like soft stem, tendril, pistil, and stamen, can easily be shown by means of magnifying levers. Again, if we take a hollow tubular organ of some ordinary plant, say the peduncle of daffodil, it is clear that the protected inner side of the tube must be the more excitable. When this is cut in the form of a spiral strip and excited by means of an electric shock, we observe a responsive movement by curling, brought about by greater contraction of the inside of the strip. If again we take a tendril which has curled round a support, the outside is fresh and free from irritation and therefore more excitable. In this case the response is by uncurling, due to greater contraction of the more excitable outer side of the spiral.

In the case of woody plants, responsive movement is prevented by the rigid support. Even in such a case I have been able to demonstrate its excitation by means of electric response, first exhibited at this very hall thirteen years ago.* No plant could appear more stolid and irresponsive than the common radish ; appearances are, however, deceptive, and we find it giving a series of vigorous responses in answer to successive stimuli. The electric response comes to an end with the death of the plant.

LATENT PERIOD OF PLANT.

I next take up the very difficult problem of finding out how long it takes for the plant to perceive and respond to a blow. In attempting to make such measurements the results are vitiated by our personal limitations. The conditions of the experiment demand accurate measurements of time-intervals shorter than a hundredth part of a second; but sluggishness of our perception makes such an attempt an impossibility. It is therefore absolutely necessary to invent a special device by which the plant itself should be compelled to write down its own latent period. In the case of the leg-muscle of a frog, the latent period, according to Helmholtz, is about a hundredth part of a second. This result is not without some error, on account of the inertia of the recording lever, and the inferring of time-relations from a neighbouring chronographic record. In my Resonant Recorder these errors have been reduced to a minimum. In the first place, the curve of response or phytogram is at the same time a chronogram. Secondly, the weight of my plant recorder is only a hundredth part of the usual muscle-recorder. The latent period of the animal tissue undergoes appropriate variation with changing external conditions. With feeble stimulus it has a definite value; this becomes shortened under a stronger blow. Again, when we are tired our perception-time becomes prolonged. Every one of these results is equally applicable in the case of the plant. The delicacy of the Resonant Recorder will be understood from the response curve exhibiting the latent period of *Mimosa* (Fig. 11). Here determination is carried to a thousandth part of a second, the value being 0·076 sec., or eight times its value in an energetic frog! The reliability of this method can be gauged from successive records under uniform conditions, when the results are found to be identical. Another curious thing is that a stoutish plant will give its response in a slow and lordly fashion, whereas a thin one attains the acme of its excitement in an incredibly short time! Perhaps some of us can tell from our own experience whether similar differences obtain amongst human kind. The perception-time of the plant becomes

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* Bose—Friday Evening Discourse. May 10, 1901.

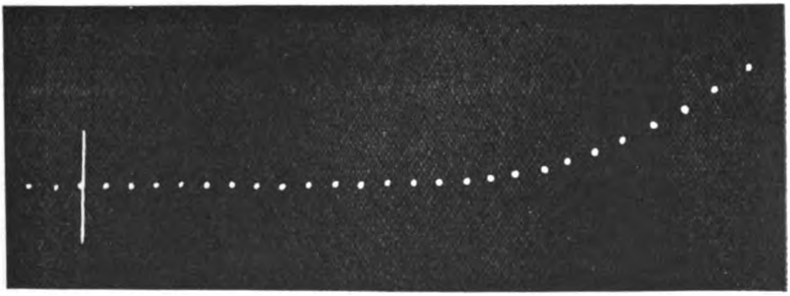

Fig. 11.—Record showing the Latent Period of *Mimosa*. This recorder vibrates 200 times per second. The time-interval between successive dots is here 0·005 sec.

very sluggish under fatigue; when excessively tired it temporarily loses its power of perception. In this condition the plant requires at least half-an-hour's absolute rest to regain its equanimity.

EXCITATORY IMPULSE IN *MIMOSA*.

We next take up the question of the function of transmission of excitation. It has hitherto been supposed that in *Mimosa* the impulse caused by irritation is merely hydro-mechanical, and quite different from the nervous impulse in the animal. According to this hydro-mechanical theory, the turgid plant tissue is imagined to be like india-rubber tube filled with water. The application of mechanical stimulus is supposed to squeeze the tissue, in consequence of which the water forced out delivers a mechanical blow to the contractile organ of the plant. The propagation of mechanical disturbance is thus occasioned by the bodily transfer of fluid material in a pipe. In strong contrast to this is the transmission of nervous impulse, which is a phenomenon of passage of protoplasmic disturbance from point to point. The molecular disturbance, constituting excitation, passes along the conducting nerve, and this point-to-point propagation of molecular upset is known as the transmission of excitatory or nervous impulse. If by any means the physiological activity of a portion of the nerve be enhanced, then excitation will pass through the particular portion with quickened speed. Such favourable condition is brought about by the application of moderate warmth. If a portion of nerve, on the other hand, be rendered physiologically sluggish, then the speed of nervous impulse through that portion will be slowed down. There are certain agents which paralyse the nerve for the time being, causing a temporary arrest of the nervous impulse. Such agents are known as anæsthetics. There may, again, be poisonous drugs which destroy the conducting power. Under the action of such poisonous agents the nervous conduction is permanently abolished.

We are now in a position to distinguish between mechanical and nervous transmission. The mechanical conduction of water through a pipe will in no way be affected by warmth or cold; the pipe will not lose consciousness and stop the flow of water, if it be made to inhale chloroform; nor will its conducting power be abolished by applying round it a bandage soaked in poison. These agents will, on the other hand, profoundly affect the transmission of excitation. The nature of an impulse may thus be discriminated by several crucial tests.

If physiological changes affect the rate of conduction, then the impulse must be of a nervous character; absence of such effect, on the other hand, proves the mechanical character of the impulse.

Of the various physiological tests, Pfeffer employed that of the narcotic drug. Chloroform applied on the *surface* of the stem of

Mimosa failed to arrest the impulse. This result, at first sight, appears most convincing, and has been universally accepted as a disproof of the existence of nervous impulse in *Mimosa*. A little reflection will, however, show that under the particular conditions of the experiment the conducting tissue in the interior could not have been affected by the external application of the narcotic, the task being, in fact, as difficult as narcotizing a nerve-trunk lying between muscles by the application of chloroform on the skin outside.

The question of nervous impulse in plants has thus to be attacked anew, and I have employed for this purpose twelve different methods.

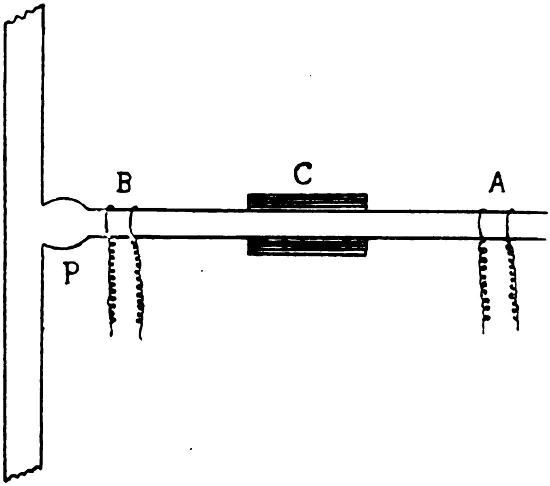

Fig. 12.—Experimental Arrangement for Determination of Velocity of Transmission and its Variation. Record is first taken when stimulus is applied near the pulvinus at B (latent period) and then at a distant point on the leaf-stalk at A. Difference of two gives time for transmission from A to B. The band of cloth C is for local application of warmth, cold, anæsthetics, and poison.

They all prove conclusively that the impulse in the plant is identical in character with that in the animal. Of these I shall give a short account of three different modes of investigation. It is obvious that the transmitted impulse in *Mimosa* must be of an excitatory, or nervous, character:

(1) If excitation can be initiated and propagated without any physical disturbance. The central fact in the mechanical theory is the squeezing out of water for starting the hydraulic impulse: the

hydro-mechanical theory must necessarily fall to the ground, if excitation can be effected without any mechanical disturbance whatsoever. I have shown that excitatory impulse is initiated under the polar action of current in the complete absence of any mechanical disturbance, the intensity of the current being so feeble as not to be perceived even by the very sensitive human tongue.

(2) If it can be shown that physiological changes induce appropriate variation in the velocity of transmission of the impulse.

(3) If the impulse in the plant can be arrested by different physiological blocks by which nervous impulse in the animal is arrested.

For the last two investigations the research resolves itself into the accurate measurement of the speed with which an impulse in the plant is transmitted, and the variation of that speed under changed condition. A portion of the tissue at C may, for example, be subjected to the action of cold, or of a poison (Fig. 12). In order to find the speed of normal transmission, we apply an instantaneous stimulus, say, of an electric shock, at B, near the pulvinus A short interval, the latent period, will elapse between the application of stimulus and the beginning of responsive movement. After the determination of the latent period, we apply stimulus once more at A, and observe the time which elapses between the application of stimulus and the response. The difference between the two periods gives us the time required for the excitation to travel from the point of application of stimulus at A, to the responding organ at B; hence we obtain the speed of impulse in the plant. The experiment is repeated once more, after the application of a given agent at C. If the speed undergoes any variation, it must be due to the action of the given agent.

DETERMINATION OF SPEED OF EXCITATORY IMPULSE IN PLANTS.*

As relatively long intervals have to be measured in the determination of velocity, the recorder has its frequency adjusted to ten vibrations per second; hence the space between successive dots represents an interval of one-tenth of a second. In Fig. 13 is given a record for determining the velocity of transmission. The two lower figures give practically identical results of successive experiments when stimulus was applied at a distance of 30 mm. The uppermost is the record for direct stimulation. From these it is seen that the interval between stimulus and response is 1·6 second, and that the latent period is 0·1 second. Hence the true time for the excitation to travel through a distance of 30 mm. is 1·5 second, the velocity being 20 mm. per second.

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* For a more detailed account confer:

BOSE—An automatic method for the investigation of velocity of transmission of excitation in *Mimosa*. Phil. Trans. of Royal Society, Series B, Vol. CCIV.

BOSE—Researches on Irritability of Plants (Longmans, Green, 1913).

The velocity of excitatory impulse in the plant is slower than those of higher, but quicker than those of lower animals. The speed of the impulse is, however, subject to variation under different conditions. One significant result that came out was that while a plant carefully protected under glass from outside blows looked sleek and flourishing, yet as a complete and perfect organism it proved to be a failure. Its conducting power was found atrophied or paralysed. But when a succession of blows rained on this effete and bloated specimen, the stimulus canalized its own path of conduction and it became more alert and responsive, and its nervous impulses became very much quickened.

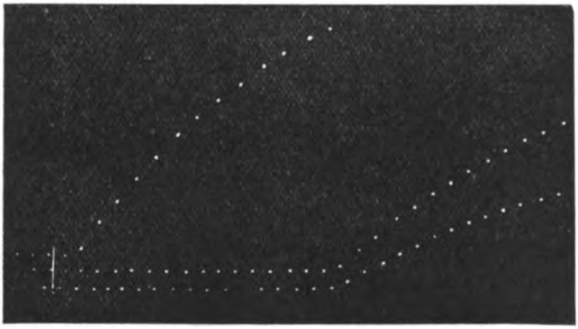

FIG. 13.—DETERMINATION OF VELOCITY OF TRANSMISSION IN *Mimosa*. The two lower records are in response to stimulus applied at a distance of 30 mm.; the upper record exhibits latent period in response to direct stimulus applied on the pulvinus. Successive dots in this and following records are of intervals of one-tenth part of a second.

INFLUENCE OF TEMPERATURE ON VELOCITY.

A decisive experiment to discriminate between the theories of mechanical and nervous transmissions consists in the determination of the effect of temperature on the speed of transmission. Temperature has no effect on mechanical propagation, whereas a moderate variation of it profoundly affects nervous transmission. The result given in Fig. 14 is quite conclusive as regards the excitatory character of the impulse in plants. It is seen that with rising temperature, the time required for transmission through the same distance is continuously reduced. In the present case the velocity is seen to be more than doubled by a rise of temperature through 9 degrees.

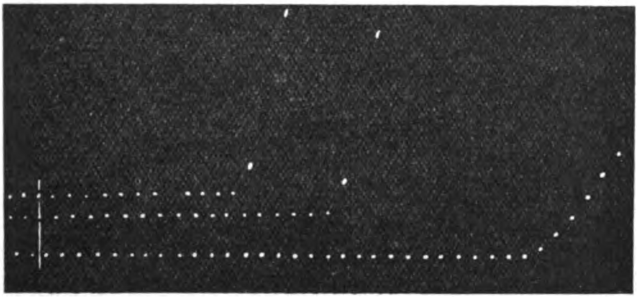

FIG. 14.—EFFECT OF RISING TEMPERATURE IN ENHANCING VELOCITY OF TRANSMISSION. The three records from below upwards are for temperatures 22° C., 28° C. and 31° C. respectively.

The converse experiment is to subject a portion of conducting petiole to the action of cold. This retards the speed of conduction. Excessive cold temporarily abolishes the conducting power.

INDUCED PARALYSIS AND ITS CURE BY ELECTRIC TREATMENT.

As an after-effect of the application of intense cold, the conducting power remains paralysed for a considerable length of time. It is a very interesting and suggestive fact that I have been able to restore the conducting power quickly by subjecting the paralysed portion of the plant to a measured and moderate dose of electric shock. The application of too strong an intensity is, however, very detrimental.

BLOCK OF CONDUCTION BY THE ACTION OF POISON.

I have also succeeded in arresting conduction of excitation in plants by local application of poisonous drugs. The defect of Pfeffer's experiment lay in his attempt to arrest the impulse by the application of a volatile anæsthetic like chloroform, on a surface of a thick stem. The chloroform escapes in the form of vapour; the access of the solution under these conditions to the interior of the tissue by absorption can only be slight, and therefore ineffective in arresting the excitatory impulse. It occurred to me that the physiological block induced by a drug could be rendered more effective in two different ways. First by the selection of a thin leaf-stalk instead of a thick stem for the purpose of the experiment, so that the access of the solution to the interior became less difficult. In the second place by the employment of strong non-volatile toxic agents like solutions of copper sulphate or of potassium cyanide. The choice of a strong poison was deemed advisable, because the absorption of even a small quantity might in such a case prove effective in abolishing the conducting power. My anticipations were fully justified. By the application of copper sulphate, the conducting power was found arrested in the course of twenty minutes. But the more deadly cyanide solution abolished the conducting power in a period as short as five minutes (Fig. 15).

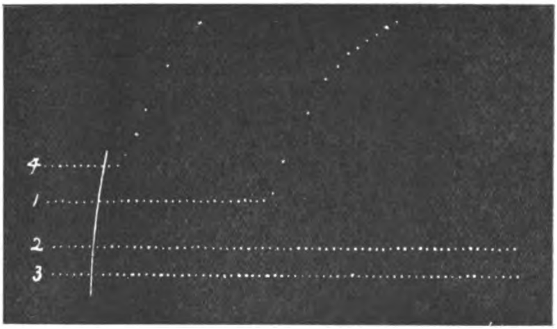

Fig. 15.—Abolition of Conduction by Local Application of Potassium Cyanide. (1) Normal record; (2) Arrest of conduction after application for five minutes; (3) Persistent abolition of conduction, even when stimulus was increased fifteen-fold; (4) Record of direct stimulation.

Accounts have thus been given of some typical experiments by which the nervous impulse is discriminated from the mechanical impulse. It has been shown that excitation may be initiated and transmitted in the plant in the complete absence of any mechanical disturbance. It has been shown that the various conditions which accelerate, retard, or arrest the nervous impulse in the animal also enhance, retard, or block the impulse in the plant in a manner which is identical. I have, moreover, from my investigations on the plant-nerve led to the discovery of certain hitherto unknown characteristics of the animal nerve. The investigation on the simplest type of plant-nerve is expected to cast a flood of light on the obscure phenomenon of nervous impulse in general, and the causes operative in bringing about the degeneration of the normal function of the nerve.

SPONTANEOUS PULSATION.

In certain animal tissues a very curious phenomenon is observed. In man and other animals there are tissues which beat, as we say, spontaneously, As long as life lasts, so long does the heart continue to pulsate. There is no effect without a cause. How, then, was it that these pulsations became spontaneous? To this query, no fully satisfactory answer has been forthcoming. We find, however, that similar spontaneous movements are also observable in plant tissues, and by their investigation the secret of automatism in the animal may perhaps be unravelled.

Physiologists, in order to know the heart of man, play with those of the frog and tortoise. "To know the heart," be it understood, is here meant in a purely physical, and not in a poetic sense. For this it is not always convenient to employ the whole of the frog. The heart is therefore isolated, and made the subject of experiments as to what conditions accelerate, and what retard, the rate and amplitude of its beat. When thus isolated, the heart tends of itself to come to a standstill, but if, by means of a fine tubing, it be subjected to internal hydrostatic pressure, its beating will be resumed, and will continue uninterrupted for a long time. By the influence of warmth, the frequency of the pulsation may be increased, but its amplitude diminished. Exactly the reverse is the effect of cold. The natural rhythm and the amplitude of the pulse undergo, again, appropriate changes under the action of different drugs. Under ether the heart may come to a standstill, but on blowing this off the beat is renewed. The action of chloroform is more dangerous, any excess in the dose inducing permanent arrest. Besides these there are poisons also which arrest the heart-beat, and a very noticeable fact in this connexion is, that some stop it in a contracted, and others in a relaxed condition. Knowing these opposed effects, it is sometimes possible to counteract the effect of one poison by administering another.

RHYTHMIC PULSATIONS IN *Desmodium*.

The existence of such spontaneous movements is seen in the well-known Indian plant, *Desmodium gyrans*, or the telegraph-plant, whose leaflets dance up down more or less continuously. The characteristics of the automatic pulsations in the plant could not be determined on account of the apparent impossibility of obtaining a record. The leaflets are too minute and the pull exerted too feeble to overcome friction of the recording surface. This difficulty I have been able to remove by the device of my Oscillating Recorder. From the records thus obtained, I am enabled to say that the automatic movements of both plants and animals are guided by laws which are identical.

Firstly, when, for convenience of experiment, we cut off the leaflet, its spontaneous movements, like those of the heart, come to a stop. But if we now subject the isolated leaflet, by means of a fine tube, to an added internal hydrostatic pressure, its pulsations are renewed, and continue uninterrupted for a very long time (Fig. 16). It is found again that the pulsation-frequency is increased under the action of warmth, and lessened under cold, increased frequency being attended by diminution of amplitude, and *vice versa*. Under ether, there is a temporary arrest, revival being possible when the vapour is blown off (Fig. 17). More fatal is the effect of chloroform. The most extraordinary parallelism, however, lies in the fact that those poisons which arrest the beat of the heart in a particular way, arrest the plant-pulsation also in a corresponding manner, the arrest produced being either at systole or diastole, depending on the characteristic reaction of the poison. Taking advantage of the antagonistic reactions of specific poisons, I have been able to revive a poisoned leaflet by the application of another counteracting poison.

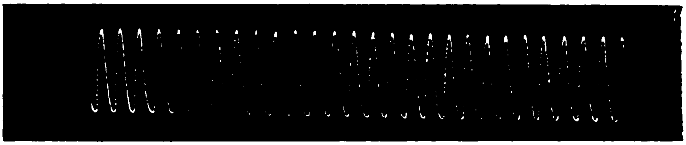

Fig. 16.—Record of Automatic Pulsations in *Desmodium gyrans.*

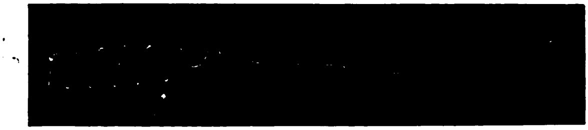

Fig. 17.—Arrest of Pulsation of *Desmodium* under Ether; Restoration of Pulsation on Blowing off Ether. The arrow indicates the time of application.

Let us now enquire into the causes of these automatic movements so-called. In experimenting with certain types of plant tissues, I find that an external stimulus gives rise to the same amplitude of response, whether the stimulus be feeble or strong. What happens, then, to the excess of the incident energy? It is not really lost.

for these particular plant-tissues have the power of storage. In this way, energy derived in various ways from without—such as light, warmth, food, and so on—is constantly being accumulated. When a certain point is reached, there is a bubbling overflow; and we call this overflow spontaneous movement. Thus what we call automatic is really an overflow of what has previously been stored up. When this accumulated energy is exhausted, then there is also an end of spontaneous movements (Fig. 18). But a fresh accession of stimulus from outside renews these pulsations.

In the matter of these so-called spontaneous activities of the plant I find that there are two distinct types. In one the overflow is initiated with very little storage, but here the unusual display of activity soon comes to a stop. To maintain such specimens in the rhythmic condition, constant stimulation from outside is necessary. Plants of this type are extremely dependent on outside influences, and when such sources of stimulus are removed they speedily come to an inglorious stop. *Averrhoa* is an example of this kind. In the second type of automatic plant-activity I find that long-continued storage is required before an overflow can begin. But in this case the spontaneous outburst is persistent and of long duration, even when the plant is deprived of any immediately exciting cause. These, therefore, are not so obviously dependent as the others on the sunshine of the world. Our telegraph-plant, *Desmodium*, is an example of this.

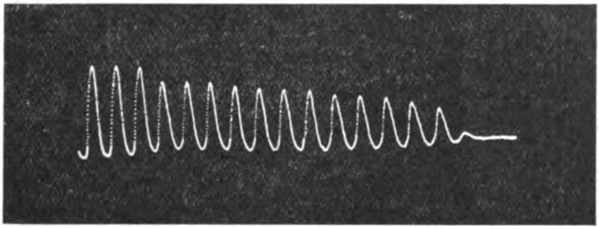

FIG. 18.—GRADUAL STOPPAGE OF PULSATION IN AN ISOLATED *Desmodium* LEAFLET DUE TO RUN-DOWN OF STORED ENERGY.

INSTANTANEOUS RECORD OF GROWTH.

As a further example of automatic activity we may take the phenomenon of growth. The rate of growth is so extremely slow that even the proverbial pace of the snail is two thousand times quicker! It would take an average plant two hundred years to cover the short distance of a mile. This extreme slowness is a serious drawback in the investigation on growth. For even with the existing magnifying growth-recorders it would take many hours for the variation of growth to be recorded under the given changed conditions in the environment. The results thus obtained are subject to errors brought about by the variation of growth which takes place spontaneously in the course of a few hours. Growth can be assumed to remain constant only for a short time; on this account it is necessary to conclude an experiment in the course of a few minutes.

By means of microscopic projection it is possible to magnify growth; but such an arrangement will not be self-recording; there is again a serious error introduced by the action of strong light, which profoundly modifies the rate of normal growth.

These difficulties have been overcome in my high magnification Crescograph, which records the absolute rate of growth in a time so short as the single beat of the pendulum. The various magnifications available are a thousand or ten thousand times. For demonstration purposes I have been able to secure a magnification of a million times. The infinitesimal growth thus becomes magnified so as to appear rushing forward as if in a race. The actual rate of growth and its variations under the action of drugs, of food-materials, of various electrical and other forms of stimuli, are thus recorded in the course of a few minutes. The great importance of this method of investigation in agriculture is sufficiently obvious.

The plant has thus been made to exhibit many of the activities which we have been accustomed to associate only with animal life. In the one case, as in the other, stimulus of any kind will induce a responsive thrill. There are rhythmic tissues in the plant which, like those in the animal, go on throbbing ceaselessly. These spontaneous pulsations in the one case, as in the other, are affected by various drugs in an identical manner. And in the one case, as in the other, the tremor of excitation is transmitted with a definite and measured speed from point to point along conducting channels. The establishment of this similarity of responsive actions in the plant and animal will be found of the highest significance; for we now realize that it is by the study of the simpler phenomena of irritability in the vegetal organisms that we may expect to elucidate the more complex physiological reactions of the animal.

THE PLANT'S RESPONSE TO THE SHOCK OF DEATH.

A time comes when, after an answer to a supreme shock, there is a sudden end of the plant's power to give any further response. This supreme shock is the shock of death. Even in this crisis there is no immediate change in the placid appearance of the plant. Drooping and withering are events that occur long after death itself. How does the plant, then, give this last answer? In man, at the critical moment, a spasm passes through the whole body, and similarly in the plant I find that a great contractile spasm takes place. This is accompanied by an electrical spasm also. In the script of the Death-recorder the line, that up to this point was being drawn, becomes suddenly reversed and then ends. This is the last answer of the plant.

These, our mute companions, silently growing beside our door, have now told us the tale of their life-tremulousness and their death-spasm in script that is as inarticulate as they. May it not be said that this their story has a pathos of its own beyond any that we have conceived?

We have now before our mind's eye the whole organism of the perceiving, throbbing and responding plant, a complex unity and not a congeries of unrelated parts. The barriers which separated kindred phenomena in the plant and animal are now thrown down. Thus community throughout the great ocean of life is seen to outweigh apparent dissimilarity. Diversity is swallowed up in unity.

In realizing this, is our sense of final mystery of things deepened or lessened? Is our sense of wonder diminished when we realize in the infinite expanse of life that is silent and voiceless the foreshadowings of more wonderful complexities? Is it not rather that science evokes in us a deeper sense of awe? Does not each of her new advances gain for us a step in that stairway of rock which all must climb who desire to look from the mountain-tops of the spirit upon the promised land of truth?

[J. C. B.]

WEEKLY EVENING MEETING,

Friday, June 5, 1914.

Charles Hawksley, Esq., M.Inst.C.E., Vice-President, in the Chair.

Professor William H. Bragg, M.A. F.R.S.

X-Rays and Crystalline Structure.

Two years have gone by since Dr. Laue made his surprising discovery of the interference effects accompanying the passage of X-rays through crystals. The pioneer experiment has opened the way for many others, and a very large amount of work, theoretical and practical, has now been done. As the preliminary exploration of the new country has proceeded, our first estimate of its resources has grown continuously; we have learned many things which help us to a better understanding of phenomena already familiar, and we have seen avenues of enquiry open out before us which as yet there has been little time to follow. The work is full of opportunities for exact quantitative measurement, where precision is sure to bring its due reward. There is enough work in sight to absorb the energies of many experimenters, and there is sure to be far more than we can see. When we consider the wideness of the new field, the quality and quantity of the work to be done in it, and the importance of the issues, we are scarcely guilty of over-statement if we say that Laue's experiment has led to the development of a new science.

The experiment itself—to put it very briefly—constitutes a proof that X-rays consist of extremely short ether waves. In order to appreciate the value of this demonstration, we must bear in mind the present conditions of our knowledge of the laws of radiation in general. Let us consider very shortly how the whole matter stood when the new work was begun.

When X-rays were first discovered eighteen years ago it was soon pointed out that they might consist of electro-magnetic disturbance of the ether analogous to those supposed to constitute light. It was true that the new rays seemed to be incapable of reflection, refraction, diffraction, and interference, which were familiar optical phenomena. But it was pointed out by Schuster* that these defects could be explained as natural consequences of an extremely small wave-length. The positive evidence consisted mainly in the knowledge that the impact of the electrons on the anti-cathode of the X-ray bulb ought to be the occasion of electro-magnetic waves of some sort, and in the discovery by Barkla that the X-rays could be polarized, which last is a property also of light.

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* Nature, Jan. 23, 1896.

As experimental evidence accumulated, a number of results were found which the electro-magnetic theory was unable to explain, at least in a direct and simple manner. They were mainly concerned with the transference of energy from place to place. In some way or other the swiftly moving electron of the X-ray bulb transfers its energy to the X-ray, and the X-ray in its turn communicates approximately the same quantity of energy to the electron which originates from matter lying in the track of the X-ray, and which is apparently the direct cause of all X-ray effects. Experiment seemed to indicate that X-ray energy travelled as a stream of separate entities or quanta, the energy of the quantum differing according to the quality of the X-ray. It looked at one time as if it might be the simplest plan to deny the identity in nature of X-rays and light, to describe the former as a corpuscular radiation and the latter as a wave motion. Otherwise, it seemed that the electro-magnetic hypothesis would be torn to pieces in the effort to hold all the facts together.

But it appeared on a close examination of light phenomena also, though in much less obvious fashion, that the very same effects occurred which in the case of X-rays were so difficult to explain from an orthodox point of view. In the end it became less difficult to deny the completeness of the orthodox theory than the identity in nature of light and X-rays. Modern work on the distribution of energy in the spectrum, and the dependence of specific heat upon temperature, has also led independently to the same point of view. It has been urged with great force by Planck, Einstein, and others that radiated energy is actually transferred in definite units or quanta, and not continuously; as if we had to conceive of atoms of energy as well as of atoms of matter. Let it be admitted at once that the quantum theory and the orthodox theory appear to stand in irreconcilable opposition. Each by itself correlates great series of facts; but they do not correlate the same series. In some way or other the greater theory must be found, of which each is a partial expression.

The new discovery does not solve our difficulty at once, but it does two very important things. In the first place, it shows that the X-rays and light are identical in nature; in fact, it removes every difference except in respect to wave-length. The question as to the exact place where the difficulty lies is decided for us; we are set the task of discovering how a continuous wave motion, in a continuous medium, can be reconciled with discontinuous transferences of radiation energy. Some solution there must be to this problem. The second important thing is that the new methods will surely help us on the way to find that solution. We can now examine the X-rays as critically as we have been able to study light, by means of the spectrometer. The wave-length of the X-ray has emerged as a measurable quantity. The complete range of electromagnetic radiations now lies before us. At one end are the long waves of wireless telegraphy, in the middle are first the waves of the infra-red detected by their heating effects, then the light waves, and then the short waves of the ultra-violet. At the other end are the extremely short waves that belong to X-radiation. In the comparative study of the properties of radiation over this very wide range we must surely find the answer to the greatest question of modern physics.

So much for the general question. Let us now consider the procedure of the new investigations, and afterwards one or two applications to special lines of enquiry.

The experiment due to Laue and his collaborators Friedrich and Knipping has already been described in this lecture room and is now well-known. A fine pencil of X-rays passes through a thin crystal slip and impresses itself on a photographic plate. Round the central spot are found a large number of other spots, arranged in a symmetrical fashion, their arrangement clearly depending on the crystal structure. Laue had anticipated some such effect as the result of diffraction by the atoms of the crystal. His mathematical analysis is too complicated to be described now, and indeed it is not in any circumstances easy to handle. It will be better to pass on at once to a very simple method of apprehending the effect which was put forward soon after the publication of Laue's first results. I must run the risk of seeming to be partial if I point out the importance of this advance, which was made by my son W. Lawrence Bragg. All the recent investigations of X-ray spectra and the examination of crystal structure and of molecular motions which have been carried out since then have been rendered possible by the easy grasp of the subject which resulted from the simpler conception.

Let us imagine that a succession of waves constituting X-radiation falls upon a plane containing atoms, and that each atom is the cause of a secondary wavelet. In a well-known manner, the secondary wavelets link themselves together and form a reflected wave. Just so a sound wave may be reflected by a row of palings, and very short sound waves by the fibres of a sheet of muslin.

Suppose a second plane of atoms to lie behind the first and to be parallel to it. The primary wave, weakened somewhat by passing through the first plane, is again partially reflected by the second. When the two reflected pencils join it will be of great importance whether they fit crest to crest and hollow to hollow, or whether they tend to destroy each other's effect. If more reflecting planes are supposed, the importance of a good fit becomes greater and greater. If the number is very large, then, as happens in many parallel cases in optics, the reflected waves practically annul each other unless the fit is perfect.

It is easily seen that the question of fit depends on how much distance a wave reflected at one plane loses in comparison with the wave which was reflected at the preceding plane: the fit will be perfect if the loss amounts to one, two, three, or more wave-lengths exactly. In its turn the distance lost depends on the spacing of the planes, that is to say, the distance from plane to plane, on the wave-length and on the angle at which the rays meet the set of planes.

The question is formally not a new one. Many years ago Lord Rayleigh discussed it in this room, illustrating his point by aid of a set of muslin sheets stretched on parallel frames. The short sound waves of a high-pitched bird-call were reflected from the set of frames and affected a sensitive flame; and he showed how the spacing of the planes must be carefully adjusted to the proper value in relation to the length of wave and the angle of incidence. Rayleigh used the illustration to explain the beautiful colours of chlorate of potash crystals. He ascribed them to the reflection of light by a series of parallel and regularly spaced twinning planes within the crystal, the distance between successive planes bearing roughly the same proportion to the length of the reflected wave of light as the distance between the muslin sheets to the length of the wave of sound.

Our present phenomenon is exactly the same thing on a minute scale: thousands of times smaller than in the case of light, and many millions of times smaller than in the case of sound.

By the kindness of Prof. R. W. Wood I am able to show you some fine examples of the chlorate of potash crystals. If white light is allowed to fall upon one of them, the whole of it is not reflected. Only that part is reflected which has a definite wave-length or something very near to it, and the reflected ray is therefore highly coloured. The wave-length is defined by the relation already referred to. If the angle of incidence is altered, the wave-length which can be reflected is altered, and so the colour changes.

It is not difficult to see the analogy between these cases and the reflection of X-rays by a crystal. Suppose, for example, that a pencil of homogeneous X-rays meets the cube face of such a crystal as rock-salt. The atoms of the crystal can be taken to be arranged in planes parallel to that face, and regularly spaced. If the rays meet the face at the proper angle, and only at the proper angle, there is a reflected pencil. It is to be remembered that the reflection is caused by the joint action of a series of planes, which, in this case, are parallel to the face; it is not a reflection by the face itself. The face need not even be cut truly: it may be unpolished or deliberately roughened. The reflection takes place in the body of the crystal, and the condition of the surface is of little account.

The allotment of the atoms to a series of planes parallel to the surface is not of course the only one possible. For example, in the case of a cubic crystal, parallel planes containing all the atoms of the crystal may also be drawn perpendicular to a face diagonal of the cube, or to a cube diagonal, or in many other ways. We may cut the crystal so as to show a face parallel to any series, and then place the crystal so that reflection occurs, but the angle of incidence will be different in each case since the spacings are different. It is not necessary to cut the crystal except for convenience. If wave-length, spacing, and angle between ray and plane are rightly adjusted to each other, reflection will take place in the crystal independently of any surface arrangement.

This is the "reflection" method of explaining the Laue photograph. W. L. Bragg showed in the first place that it was legitimate, and the second that it was able to explain in the position of all the spots which Laue found upon his photographs. The different spots are reflections in different series of planes which may be drawn to contain the atoms of the crystal. The simpler conception led at once to a simpler procedure. It led to the construction of the X-ray spectrometer, which resembles an ordinary spectrometer in general form, except that the grating or prism is replaced by a crystal and the telescope by an ionization chamber and an electroscope. In use a fine pencil of X-rays is directed upon the crystal, which is steadily turned until a reflection leaps out; and the angle of reflection is then measured. If we use different crystals or different faces of the same crystal, but keep the rays the same, we can compare the geometrical spacings of the various sets of planes. If we use the same crystal always, but vary the source of X-rays, we can analyse the latter, measuring the relative wave-lengths of the various constituents of the radiation.

We have thus acquired a double power:—1. We can compare the intervals of spacing of the atoms of a crystal or of different crystals, along various directions within the crystal; in this way we can arrive at the structure of the crystal. 2. We can analyse the radiation of an X-ray bulb. In fact, we are in the same position as we should have been in respect to light if our only means of analysing light had been by the use of coloured glasses, and we had then been presented with a spectrometer, or some other means of measuring wave-length exactly.

We now come to a critical point. If we knew the exact spacings of the planes of some one crystal, we could now by comparison find the spacings of all other crystals, and measure the wave-length of all X-radiations; or if we knew the exact value of some one wave-length, we could find by comparison the values of all other wave-lengths, and determine the spacings of all crystals. But as yet we have no absolute value either of wave-length or of spacings.

The difficulty appears to have been overcome by W. L. Bragg's comparison of the reflecting effect in the case of rocksalt or sodium chloride, and sylvine or potassium chloride. These two crystals are known to be "isomorphous": they must possess similar arrangements of atoms. Yet they display a striking difference both in the Laue photograph and on the spectrometer. The reflections from the various series of planes of the latter crystal show spacings consonant with an arrangement in the simplest cubical array, of which the smallest element is a cube at each corner of which is placed the same group, a single atom or molecule, or group of atoms or molecules. In the case of rocksalt, the indications are that the crystal possesses a structure intermediate between the very simple arrangement just described and one in which the smallest element is a cube having a similar group of atoms or molecules at every corner and at the middle point of each face. The arrangement is called by crystallographers the face-centred cube. The substitution of the sodium for the potassium atom must transform one arrangement into the other. This can be done in the following way, if we accept various indications that atoms of equal weight are to be treated as equivalent. Imagine an elementary cube of the crystal pattern to have an atom of chlorine at every corner and in the middle of each face, and an atom of sodium or potassium as the case may be, at the middle point of each edge and at the centre of the cube. We have now an arrangement which fits the facts exactly. The weights of the potassium and chlorine atoms are so nearly the same as to be practically equivalent, and when they are considered to be so, the arrangement becomes the simple cube of sylvine. But when the lighter sodium replaces the potassium as in rocksalt the arrangement is on its way to be that of the face-centred cube, and would actually become so were the weight of the sodium atoms negligible in comparison with those of chlorine. Of course the same result would follow were two or three, or any number of atoms of each sort to take the place of the single atom, provided the same increase were made in the number of the atoms of both sorts. We might even imagine two sorts of groups of chlorine and metal atoms, one containing a preponderance of the former, the other of the latter, but so that two groups, one of each kind, contain between them the same proportion of chlorine and metal as the crystal does. We must merely have two groups which differ in weight in the case of rocksalt, and are approximately equal in weight in the case of sylvine. But it was best to take the simplest supposition at the outset; and now the evidence that the right arrangement has been chosen is growing as fresh crystals are measured. For it turns out that in all crystals so far investigated, the number of atoms at each point must always be the same. Why, then, should it be more than one? Or in other words, if atoms are always found in groups of a certain number, ought not that group to be called the atom?

As soon as the structure of a crystal has been found we can at once find by simple arithmetic the scale on which it is built. For we know from other sources the weight of individual atoms, and we know the total weight of the atoms in a cubic centimetre of the crystal. In this way we find that the nearest distance between two atoms in rocksalt is $2 \cdot 81 \times 10^{-8}$ cm., which distance is also the spacing of the planes parallel to a cube face. From a knowledge of this quantity the length of any X-ray wave can be calculated at once as soon as the angle of its reflection by the cube face has been measured. In other words, the spectrometer has now become a means of measuring the length of waves of any X-radiation, and the actual spacings of the atoms of any crystal.

From this point the work branches out in several directions. It will not be possible to give more than one or two illustrations of the progress along each branch.

Let us first take up the most interesting and important question of the "characteristic" X-rays. It is known that every substance when bombarded by electrons of sufficiently high velocity emits X-rays of a quality characteristic of the substance. The interest of this comparison lies in the fact that it displays the most fundamental properties of the atom. The rays which each atom emits are characteristic of its very innermost structure. The physical conditions of the atoms of a substance and their chemical associations are largely matters of the exterior: but the X-rays come from the interior of the atoms and give us information of an intimate kind. What we find is marked by all the simplicity we should expect to be associated with something so fundamental.

All the substances of atomic weight between about 30 and 120 give two strongly defined "lines"; that is to say, there are found among the general heterogeneous radiation two intense almost homogeneous sets of waves. For instance, rhodium gives two pencils of wave-lengths, approximately equal to $0 \cdot 61 \times 10^{-8}$ cm. and $0 \cdot 54 \times 10^{-8}$ cm. respectively. More exactly the former of these is a close doublet having wave-lengths $0 \cdot 619 \times 10^{-8}$ and $0 \cdot 614 \times 10^{-8}$. The wave-lengths of palladium are nearly $0 \cdot 58 \times 10^{-8}$ and $0 \cdot 51 \times 10^{-8}$; nickel $1 \cdot 66 \times 10^{-8}$ and $1 \cdot 50 \times 10^{-8}$. Lately Moseley has made a comparative study of the spectra of the great majority of the known elements, and has shown that the two-line spectrum is characteristic of all the substances whose atomic weights range from that of aluminium, 27, to that of silver, 108. These X-rays constitute, there is no doubt whatever, the characteristic rays which Barkla long ago showed to be emitted by this series of substances.

Now comes a very interesting point. When Moseley sets the increasing atomic weights against the corresponding decreasing wave-lengths, the changes do not run exactly parallel with each other. But if the wave-lengths are compared with a series of natural numbers everything runs smoothly. In fact it is obvious that the steady decrease in the wave-length as we pass from atom to atom of the series in the periodic table implies that some fundamental element of atomic structure is altering by equal steps. There is excellent reason to believe that the change consists in successive additions of the unit electric charge to the nucleus of the atom. We are led to think of the magnitude of the nucleus of any element as being simply proportional to the number indicating the place of the element in the periodic table, hydrogen having a nuclear change of one unit, helium two, and so on. The atomic weights of the successive elements do not increase in an orderly way; they mount by steps of about two, but not very regularly, and sometimes they seem absolutely to get into the wrong order. For example, nickel has an atomic weight of 58·7, whereas certain chemical properties and still more its behaviour in experiments on radio-activity indicate that it should lie between cobalt (59) and copper (63·6). But the wave-lengths, which are now our means of comparison, diminish with absolute steadiness in the order cobalt, nickel, copper. Plainly, the atomic number is a more fundamental index of quality than the atomic weight.

It is very interesting to find, in the series arranged in this way, four, and only four, gaps which remain to be filled by elements yet undiscovered.

Let us now glance at another and most important side of the recent work, the determination of crystalline structure. We have already referred to the case of the rocksalt series, but we may look at it a little more closely in order to show the procedure of crystal analysis.

The reflection of a pencil of homogeneous rays by a set of crystalline planes occurs, as already said, at a series of angles regularly increasing; giving, as we say, spectra of the first, second, third orders, and so on. When the planes are all exactly alike and equally spaced the intensities of the spectra decrease rapidly as we proceed to higher orders, according to a law not yet fully explained. This is, for example, the case with the three most important sets of planes of sylvine, those perpendicular to the cube edge, the face diagonal, and the cube diagonal respectively. An examination of the arrangement of the atoms in the simple cubical array of sylvine shows that for all these sets the planes are evenly spaced and similar to each other. It is to be remembered that the potassium atom and the chlorine atom are so nearly equal in weight that they may be considered effectively equal. In the case of rocksalt the same may be said of the first two sets of planes, but not of the third. The planes perpendicular to the cube diagonal are all equally spaced, but they are not all of equal effect. They contain alternately, chlorine atoms (atomic weight 35·5) only and sodium atoms (atomic weight 23) only. The effect of this irregularity on the intensities of the spectra of different orders is to enhance the second, fourth, and so on in comparison with the first, third, and fifth. The analogous effect in the case of light is given by a grating in which the lines are alternately light and heavy. A grating specially ruled for us at the National Physical Laboratory shows this effect very well. This difference between rocksalt and sylvine and its explanation in this way constituted an important link in W. Lawrence Bragg's argument as to their structure.

When, therefore, we are observing the reflections in the different faces of a crystal in order to obtain data for the determination of its structure, we have more than the values of the angles of reflection to help us: we have also variations of the relative intensities of the spectra. In the case just described we have an example of the effect produced by want of similarity between the planes, which are, however, uniformly spaced.

In the diamond, on the other hand, we have an example of an effect due to a peculiar arrangement of planes which are otherwise similar. The diamond crystallizes in the form of a tetrahedron. When any of the four faces of such a figure is used to reflect X-rays, it is found that the second order spectrum is missing. The analogous optical effect can be obtained by ruling a grating so that, as compared with a regular grating of the usual kind, the first and second, fifth and sixth, ninth and tenth alone are drawn. To put it another way, two are drawn, two left out, two drawn, two left out, and so on. The National Physical Laboratory has ruled a special grating of this kind also for us, and the effect is obvious. The corresponding inference in the case of the diamond is that the planes parallel to any tetrahedral face are spaced in the same way as the lines of the grating. Every plane is three times as far from its neighbour on one side as from its neighbour on the other. There is only one way to arrange the carbon atoms of the crystal so that this may be true. Every atom is at the centre of a regular tetrahedron composed of its four nearest neighbours, an arrangement best realized by the aid of a model. It is a beautifully simple and uniform arrangement, and it is no matter of surprise that the symmetry of the diamond is of so high an order. Perhaps we may see also, in the perfect symmetry and consequent effectiveness of the forces which bind each atom to its place, an explanation of the hardness of the crystal.

Here, then, we have an example of the way in which peculiarities of spacing can be detected. There are other crystals in which want of uniformity both in the spacings and in the effective value of the planes combine to give cases still more complicated. Of these are iron pyrites, calcite, quartz, and many others. It would take too long to explain in detail the method by which the structures of a large number of crystals have already been determined. Yet the work done already is only a fragment of the whole, and it will take no doubt many years, even though our methods improve as we go on, before the structures of the most complicated crystals are satisfactorily determined.

On this side, then, we see the beginning of a new crystallography which, though it draws freely on the knowledge of the old, yet builds on a firmer foundation since it concerns itself with the actual arrangement of the atoms rather than the outward form of the crystal itself. We can compare with the internal arrangements we have now discovered the external forms which crystals assume in growth, and the modes in which they tend to come apart under the action of solvents and other agents. By showing how atoms arrange and disarrange themselves under innumerable variations of circumstances we must gain knowledge of the nature and play of the forces that bind the atoms together.

There is yet a third direction in which enquiry may be made, though as yet we are only at the beginning of it. In the section just considered we have thought of the atoms as at rest. But they are actually in motion, and the position of an atom to which we have referred so frequently must be an average position about which it is in constant movement. Since the atoms are never exactly in their places, the precision of the joint action on which the reflection effect depends suffers materially. The effect is greater the higher the order of the spectrum. When the crystal under examination is contained within a suitable electric furnace and the atoms vibrate more violently through the rise of temperature, the intensities of all orders diminish, but those of higher order much more than those of lower. The effect was foreseen by the Dutch physicist Debije, and the amount of it was actually calculated by him on certain assumptions. I have found experimental results in general accord with his formula. In passing it may be mentioned that as the crystal expands with rise of temperature the spacing between the planes increases and the angles of reflection diminish, an effect readily observed in practice.

This part of the work gives information respecting the movements of the atoms from their places, the preceding respecting their average positions. It is sure, like the other, to be of much assistance in the enquiry as to atomic and molecular forces, and as to the degree to which thermal energy is locked up in the atomic motions.

This brief sketch of the progress of the new science in certain directions is all that is possible in the short time of a single lecture: but it may serve to give some idea of its fascination and possibilities.

[W. H. B.]

GENERAL MONTHLY MEETING,

Monday, June 8, 1914.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Henry Best Hans Hamilton, Esq., J.P.
Sir William Hesketh Lever, Bart.
Hay Whitworth Pierre Matthey, Esq.
Percy St. Clair Matthey, Esq., J.P.
Miss Matthey,
John Francis Claverton Snell, Esq., M.Inst.C.E.
Thomas John Taylor, Esq., M.I.E.E.

were elected Members of the Royal Institution.

His Grace the President reported that he had nominated the following as Vice-Presidents during the ensuing year :—

The Right Hon. Lord Blyth, J.P.
J. H. Balfour Browne, Esq., K.C. LL.D.
Charles Hawksley, Esq., M.Inst.C.E.
Donald William Charles Hood, Esq., C.V.O. M.D. F.R.C.P.
The Right Hon. Lord Moulton, P.C. LL.D. F.R.S.
Edward Pollock, Esq., F.R.C.S.
Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S., Treasurer.
Alexander Siemens, Esq., M.Inst.C.E., Secretary.

The Secretary reported the decease of Sir Francis Laking on May 21, and of Sir Joseph Swan on May 27, Members of the Royal Institution, and the following Resolutions of Condolence with the families were passed :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution and Medical Science in the decease of Sir Francis Henry Laking, Bart., G.C.V.O. K.C.B. M.D. LL.D. M.R.C.P., Grand Officer of the Legion of Honour.

Sir Francis Laking was Surgeon Apothecary in Ordinary to the late Queen Victoria, Physician in Ordinary to the late King Edward VII., and also to His Majesty King George V.

He was a Member of the Royal Institution for many years, and took a warm interest in the prosperity and continued prosecution of the objects of the Foundation in the Diffusion of Science and of Useful Knowledge. He also rendered valuable personal services to the Institution by discharging the duties of Manager and Vice-President.

The Managers desire to offer, on behalf of the Members of the Royal Institution, their deepest sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution and Electrical Science in the decease of Sir Joseph Wilson Swan. M.A. D.Sc. F.R.S. Hon.M.I.E.E., Chevalier of the Legion of Honour.

Sir Joseph Swan was well known for his important researches in photographic chemistry, and many valuable advances in the art of Photographic Reproduction were due to the results of his investigations. His name will always be associated in the history of Electrical Science with the carbon filament glow lamp.

Sir Joseph Swan was a Member of the Royal Institution for twenty-nine years: took an active interest in its welfare; and rendered valuable services on the Board of Managers. He delivered two Friday Evening Discourses on "Electric Lighting by Incandescence" (1882), and "Electro Metallurgy" (1892).

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their sincere sympathy with Lady Swan and the family in their bereavement.

The Special Thanks of the Members were returned to Dr. H J. Backer for his present of a Medallion of Professor A. N. P. Franchemont, Hon. M.R.I., coined in memory of the fortieth year of his Professorship and of his seventieth Birthday.

The following Address from the Royal Institution to the University of Groningen, to be presented by Major Edmond H. Hills when he attends as Delegate on the celebration of its Three Hundredth Anniversary, was signed by the President, the Secretary, the Managers, and Officials of the Royal Institution:—

The Members of the Royal Institution of Great Britain, founded in the year 1799 for the Promotion, Diffusion, and Extension of Science and of Useful Knowledge, desire to offer their sincere congratulations to the University of Groningen on the Celebration of the Three Hundredth Anniversary of its Foundation. They are glad to take this opportunity of recognizing the distinguished position occupied by the University, and it is their earnest prayer that it may long continue to flourish and to take a leading part in the education of youth, in the encouragement of learning, and in the advancement of natural knowledge.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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WEEKLY EVENING MEETING,

Friday, June 12, 1914.

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

His Excellency The Hon. Walter Hines Page, LL.D.,
The American Ambassador.

Some Aspects of the American Democracy.

I undertake a brief discourse on some aspects of the American democracy, not with an idea or hope of giving a comprehensive explanation of its workings, for my task is a far more modest one. I shall try only to answer certain questions that have many times been put to me during my residence here, about which, therefore, I assume that there is some curiosity in the minds of Englishmen. I shall not confuse you with subjects of controversy, nor burden you with my own opinions, but try only to give a cue to the understanding of certain large social and political phenomena—phenomena that have appeared these hundred years since the death of the American founder of this Institution, and which, I am sure, would be most interesting to him.

And by the American democracy I do not mean only a method of government—I mean a scheme of society also. For it is of the first importance to understand that government in the United States grows out of, and is continually modified by, the social aims of the people. Government and social aims are inextricably bound together. They are two aspects of the same subject.

Furthermore, government in the United States is not the National Government only. Quite as important tasks fall to local governments as fall to the National Government, and many more tasks—to such local governments as the state, the county, and the city. These pay for and direct the vast educational activities of the nation. The state governments control the suffrage; they create and conduct the courts of justice for all local causes; they levy and collect taxes for all local needs; and in these and in many more important activities each of the forty-eight States is sovereign and supreme. When, for instance, the state of California, or the state of Illinois, decided to give the ballot to every woman who had reached her twenty-first year, no other state nor the National Government could raise objection: this is a subject wholly within each state's control.

The chief political activities reported in the foreign press, and the chief political phenomena observed by most foreign visitors, are naturally the political acts done at Washington ; for these, of course, include the foreign relations of the Government. Naturally, too, the chief social phenomena observed by foreign visitors are of life in a few large cities. But life in the big cities is not the typical nor the dominant life. In spite of the rush to the cities during recent decades, which, for economic reasons, has taken place in the United States as elsewhere in the world, the typical and dominant life in the Republic is still the rural life, and life in the villages and towns and smaller cities. The people in New York, Chicago, Philadelphia, and Boston do not elect a majority of the members of Congress, nor of the forty-eight legislatures of the States : it is the country folk that do these things. Nor do the people in our few big cities produce the great wealth of the country which they handle ; nor do they do the thinking for the mass of the people.

It is necessary for an understanding of the American democracy to keep constantly in mind the millions that dwell on the soil and in small towns, the inhabitants of the great valleys of the Mississippi river and of its chief tributaries. Take it through and through, the nation is yet a rural nation ; and it is the country people who really rule it.

But state differs from state, and region from region in natural advantages, in degrees of well-being, and in the cultivation of the people. In studying them, the problem is to avoid mistaking merely local facts and tendencies for general forces. In this too hurried glimpse, I shall try to show at least one permanent principle —one aim which guides a great number of apparently unrelated activities.

The most striking single fact about the United States is, I think, this spectacle, which, so far as I know, is new in the world : On that great agricultural area are about seven million farms of an average size of about 140 acres, most of which are tilled by the owners themselves, a population that varies greatly, of course, in its thrift and efficiency, but most of which is well-housed, in houses they themselves own, well-clad, well-fed, a population that trains practically all its children in schools maintained by public taxation ; their local governments are wholly of their own making, and they exert the greatest influence also in making the National Government—economically and politically free, every man past twenty-one years, and in a group of states, every woman also with a vote ; and it is the votes of these that make and unmake governments.

This wide-spread spectacle is seldom thoroughly seen by foreign visitors, for it requires years to observe it closely. But, after a man has studied it a lifetime, and become aware of its gigantic strength, and knows its spirit by being a part of it, he readily subscribes to the paradox that there is something better even than good government, and that is government in which all the people have a part ; for individual independence and responsibility are even more precious than ordered pomp and authority, which are too likely to become content with themselves.

In its amalgamation of men—and it does amalgamate all that are near enough akin in their family stocks to be personally acceptable to one another—this democracy has so far produced not a clearly-cut new type such as would satisfy an ethnologist, but a type that combines the qualities of the North European races, of which the British type is the background—the British type that is itself an earlier amalgamation of all these same races. And the American is an amalgamation under the favourable conditions of a mobile social organization, and on a continent yet new enough to offer great freedom of natural opportunity. We believe that this amalgamation will preserve the strongest of all these stocks as your earlier amalgamation did.

After these introductory words, I now direct your attention to one principle that runs through many apparently contradictory political and social activities—a principle that will help to classify and to interpret them. But I warn you that this principle is a mere commonplace to those who only half believe in it, or who give it only an intellectual assent, and are not willing to live their lives by it. When it becomes a rule of conduct, however, it is not a commonplace, but a sort of social and political Decalogue.

The fundamental article in the creed of the American democracy—you may call it the fundamental dogma, if you like—is the unchanging and unchangeable resolve that every human being shall have his opportunity for his utmost development—his chance to become and to do the best that he can.

This is the professed aim of most systems of society; but in the American democracy it is a living creed—the very breath that maintains it. I think you will find that the effort to keep society mobile, so that every man may have his utmost opportunity, runs through our life and explains most of the social, the educational, and the political activities in the Republic. Many activities, movements, ideas, legislative acts, and popular crusades that are apparently unrelated, classify themselves around this continual effort.

Keeping this key in mind, let me very hastily review certain recent events in our social, educational, and political life—to see if the key does not fit them.

First, let me take as an example, the so-called "anti-trust" agitation of the last decade. When certain companies grew into very powerful business organizations, which in the United States are commonly called "trusts," the popular displeasure with them and the popular fear of them found expression in anti-trust legislation, both in many states and in Congress. Under the Congressional Act certain trusts were by the Supreme Court resolved into their constituent parts—into the smaller companies of which they had been built. Other trusts, fearing a similar fate, voluntarily dissolved. It does not serve my present purpose to discuss the industrial gain and loss involved in these dissolutions, nor the success nor the present limitations of the anti-trust law. The law is generally yet regarded as incomplete, and there is a wide difference of opinion about the results of its enforcement. But my point is that these trusts did, according to the finding of the Supreme Court, abridge the fair opportunities of their competitors. They were found by the Court to have lessened commercial liberty and individual freedom of action. And, if I read American opinion aright, this was the real reason of their undoing. Of course, this main question became involved with many other questions. The trusts themselves asserted that they had done a public benefit by reducing the cost of their products to the consumers—in some cases with truth; that they had developed valuable new products from what had before been wasted—in some cases with truth. They every one had brought public benefits of some sort. They asserted, too, that they had been the victims of sheer envy; for success, especially financial success, always produces envy. All this and much more is true. Yet the main matter is that these great companies, in spite of all the benefits they conferred, were thought to have lessened the fair opportunity of competitors, and this was what the law was designed to prevent. The unchanging spirit of the American democracy will not brook this fundamental sin against its main dogma, whatever the incidental benefits may be — or even a supposed sin against it. The door of commercial opportunity must be left open. However crudely the anti-trust law expresses this conviction and this resolution, it does express it. And the determination to keep the door of opportunity open, at any cost, accounts for this whole vast anti-trust agitation.

A similar example of the working of this same principle is the vast volume of railway regulation. Long before the anti-trust agitation, there arose a popular outcry against the railways, an outcry that brought much legislation to regulate them, and that in some cases brought much embarrassment. The explanation is, the popular wrath that was stirred up by the practice of the railways in carrying goods more cheaply for some persons and some companies and some communities than for their competitors—in a word, the practice of abridging somebody's fair opportunity for the benefit of somebody else. Great corporations, when they commit this cardinal sin, are likely to bring punishment not on themselves only, but on many innocent persons. But our democracy has to deal with large territories, great numbers of people, and far-reaching organizations. It cannot discriminate to make its justice exact. But it does keep to certain clearly-felt aims, and it does not pretend to delicacy in all its dealings. It asserts some large principle in an unforgettable way, and its assertion at times reminds one of a volcano, or of a tornado. It may be called the great social force of nature. The main matter is that the power of its protest shall be felt—not who may be incidentally or innocently hurt by it. This is the way, in fact, that righteousness has been maintained in the world from Moses on Sinai to the emphatic voice of the American masses—a thunderous way.

Another example of the working of this principle will be found in the vast educational work of the Republic. The punishment of trusts and railways was negative; it was an effort to remove evils that had already grown up; it took the form of restraining those who were judged to have abridged a fair chance for all alike. But the vast energy that goes to the education of the masses is positive; for there is little need to make sure of the mobility of society unless those at the bottom are able to rise. In fact, the greatest harm done by all systems of privilege is that they tend to reduce some section of society to a condition in which it loses the ability to rise, and finally the desire to rise. The working out of any vital democracy demands the constant reinforcement of society from the rear. There must not be any neglected or forgotten class.

Now, American education proceeded, until a recent period, more or less after the old fashion—a routine elementary system of instruction at the public expense theoretically for everybody, and higher education, more or less of the classical type, for the few. But it became increasingly apparent that this system did not really touch or profoundly affect the mass of the people. The millions profited little by the more or less routine common schools, and still less by the old-fashioned higher education; and a complete change of the aims and methods of popular education has taken place during the last two decades—amid more discussion than, I dare say, any other subject ever underwent in any two decades of human history. The aim now—the movement which may be called the glacial motion of the nation—is in one straight line, and that is towards training every child in the Republic so that nobody and no class shall be forgotten, whatever its needs may be.

For example, it was observed that into the high schools, which prepare boys and girls for college, only a very small proportion of the children from the lower schools ever found their way. The reason, of course, was that they did not wish, or could not hope to go to college. Many of them, in fact, were obliged to earn their livings before they reached the college age. Well, if the few who could go to college received such training as they needed at the high schools, why should there not be high schools for the very much larger class that would never go to college, and that required a training different from the classical or scientific training that led to the college? Thus a forgotten class and a large class was discovered. Rather, it discovered and asserted itself. And high schools were equipped to do this service for this large forgotten class. This, in turn, modified the training given in the elementary schools. But it has not disturbed the old machinery for the old-time classical or humanistic training. This also has been improved.

The conviction of the necessity to train every human creature has become so strong and so general that a very large part of the population gives something (in addition to school taxes) to equip schools of some sort. Large contributions from rich men we hear much about, but a far larger amount is every year given by poorer people in small sums. All this is spent in the equipment and the endowment of schools of every grade and kind. There has been an educational ferment in American life. The colossal proportions of this energy and effort may be seen from such facts as these: There are (in round numbers) 20 million enrolled pupils in the common schools of the United States, taught by 535,000 teachers, maintained at an expense to the tax-payers of 450 million dollars (90 million pounds). There are 12,000 high schools conducted at the public expense, with 1,110,000 pupils, and 2,000 privately-owned high schools with 141,000 pupils. This vast educational activity, differing of course greatly in earnestness and in effectiveness in different parts of the land, I regard as the most important and typical manifestation of the earnestness and wisdom of our democracy. It is its crowning act of self-preservation.

These great benefactions remind me that I often encounter the opinion that the American democracy is in danger from its rich men. It has sometimes been feared by those who become easily alarmed that, in a mobile society, the rich would become a plutocracy, and would find ways to rule—socially and politically—that they would acquire undue power of an enduring kind. We have all heard plausible arguments to prove that this is to be the inevitable fate of the American democracy; and most of us have read novels which give pictures of the complete social and even political surrender to the organized rich. So far from the truth is this, that the democracy has lately at least surely shown something less than favours to the rich. I think that this is the judgment of most thoughtful men. The rich are too easily subjected to suspicion among us. When, for example, one branch of the National Legislature was suspected of being too friendly to the great corporations, public criticism did not cease till most of the suspected friends of wealth were retired from it—some by failure to secure re-election, some by their own motion under the pressure of public disapproval. And surely it is no distinction merely to be a rich man in the United States. The fact that a man is rich gives him no help towards public advancement. There is, in fact, among our very richest men, hardly one who could be elected to the humblest public office. Nor do they gain influence in the social organization—except of their own "set"—merely because they are rich. The justice of these public judgments passed on the rich is a matter of private opinion, and (I may add) in many cases of justified doubt. But this fact surely proves that the danger of plutocracy is wholly imaginary.

In fact, the pressure on the rich to justify their possession of wealth by the right use of it, is so great that most rich men come to a period in their lives when their chief concern is how they may most wisely bestow their wealth for the public welfare. The result is a constant flow of private aid into channels that help forward the building up of the people. We have now an era of private munificence that is perhaps without parallel in all history.

Many evils may befall the American democracy, as doubtless many evils will ; but there must be a complete change in its temper and tendencies before the evils of plutocracy overtake us. And, since we have neither entail nor primogeniture, a great fortune will remain in one family only so long as members of the family have ability to use it wisely enough to conserve it.

And our same fundamental dogma finds many other expressions : the dominant desire that every man and every group of men should have its best chance for development and self-management brings into the American colonial experiments a hesitancy that sometimes seems to be a disadvantage.

Take the American management of the Philippine Islands as an example. There is no doubt but a vast benefit to the eight million people of the Archipelago has been wrought by the American occupation. The story of the sanitary rehabilitation of Manila is one of the bright chapters in modern history, taking a place along with the sanitary triumphs at Panama, which made the construction of the Canal possible. But the question, how long American control should continue, and how fast self-government should be granted, never disappears. The result is embarrassing to those who have in hand the practical task of managing the Islands. I mention this, not to enter into the controversy that has arisen, but only to show that the dominant dogma of the utmost opportunity to every man and to every group, and to every people, asserts itself throughout our whole national life, and no delicate and difficult problem escapes its intrusion. For this reason the question is yet regarded by many persons as an open one, whether a democracy can ever become a successful manager of colonies. My own opinion is, that a democracy is the best of all instruments for building up dependent peoples ; but I am trying to interpret the practical working of public opinion rather than to burden you with my own opinions.

Thus, I hope, I have made it clear by these examples that the main motive in our anti-trust legislation, in our railway regulation, in our educational work, and in our brief colonial experience, is the same—the motive, namely, to give every man, every community, every people, the utmost chance to realize themselves.

So far I have tried to give you one key to an understanding of a wide range of social and political actions and phenomena—the key of this fundamental conviction. Now quite as wide a range of other political phenomena will be understood by remembering not a peculiarity of conviction but a peculiarity of method.

Our democracy does not proceed logically—perhaps no great social organism does. Its forces do not march forward in an ordered column. They advance in a zigzag way. This year public opinion concerns itself with one large problem, and next year with another. All the while there may be a third more important problem that cries out in vain for attention. In the course of time it, too, will be taken up. But there is often a lack of logic in the succession of tasks to which the people address themselves. Thus some abuses get time to fix themselves quite firmly on society, being forgotten for a long period; and some insistent tasks have to wait while the people concern themselves with lesser tasks. For a large public opinion will not often concern itself with more than one problem at a time.

Consider, for example, the problem of municipal government. There was a period within easy memory when American city government was at a low ebb. Mismanagement occurred, not by any means in most of our cities, but in enough conspicuous cities to provoke the comment that American city government had broken down, and even that a democracy could probably never solve this problem. The immediate reasons for this long neglect of an important duty are now pretty obvious, but it would take too long a time to recite them here. The point of my present discourse is, that as soon as any considerable number of people woke up to the real state of things and began in earnest to address themselves to the task, immediate and remarkable improvement set in. There are instances of such complete house-cleaning, of such veritable revolutions, as to inspire confidence in a democracy's successfully undertaking almost any task whatsoever.

Since this municipal house-cleaning was begun, voluntary organizations of mayors and of other special students of municipal management have held regular meetings, have discussed all conceivable aspects of the subject, and have put many new plans into operation. They have studied municipal government in all successfully governed cities in the world. Especially noteworthy is the development of what is called the commission form of city government. The usual habit had been to elect the mayor and other city officers and the council along with State and Federal officers, and the political parties each put up a ticket. A Democrat mayor, therefore, was usually elected in a Democratic city, and a Republican mayor in a Republican city; and thus city administration became a part of State and National politics. To prevent the abuses that this system bred, the commission form of government has been adopted by many towns and cities of secondary size. Citizens put forward five or more men, one to be the head of each of the great departments, such as Finance, Police, Fire, Schools, and vote for them, with this understanding—that the head of each department shall be wholly responsible for his department, and nobody else is responsible for it. It reduces the number of public officers and concentrates responsibility. The plan has worked admirably, and every year more cities adopt it.

Any visitor to the United States of twenty years ago who heard and saw so many evidences of the bad, because neglected, city government, could hardly believe that such a change could come in so short a time—a general change to extraordinary efficiency and beauty. Again, I remind you that this evil was never universal, nor has it yet been wholly cured. But the change that has taken place is one of the most noteworthy advances made in government anywhere during the last two decades.

Another example of long-delayed action because a large democracy does not take up its tasks in logical order, was the postponement for a generation of the necessary amendment of the banking and currency system. A system was devised during the Civil War, more than half-a-century ago, which was skilfully aimed to make a market for Government loans; and to this day a part of our currency is issued against an evidence of national debt. Theoretically, it looks suicidal; but practically, since the National Debt has become very small, it developed many advantages. Still, it was economically wrong and absurd. It was a piece of war legislation to force a market for Government bonds.

One result of the existing banking habits has been to localize and to concentrate credit. This old system remained until this year. For many decades it was difficult to get a sufficiently clear understanding of it in the minds of the mass of men—for it is a technical and involved subject. Yet the leaders of one political party made an agitation for a change, and now the leaders of the other great political party have enacted a new law—a law that many American economists regard as the most important piece of constructive legislation since the Civil War.

However long a problem may be delayed, therefore, such an event as this warrants the conclusion that no important task is likely to be entirely forgotten. A day for its doing will come at last. And, after all, a delay of half-a-century is not long in the life of an economic society that has had many more brand-new duties to do than there has been time to do them. Such an experience teaches us not resignation to a bad condition, for there has never been a day when thoughtful men forgot that this old banking and currency system was temporary; but it does teach us patience. A big democracy need not become impatient any more than a glacier need; for its slow pace never ceases.

Thus a grasp on a few principles and on a few methods will enable us to understand the orbic quality of the American democracy and to classify its achievements and its delays intelligently. For its phenomena are not a series of unrelated events and activities. On the contrary, they show a definite, if sometimes irregular, movement towards definite ends; and, when these definite ends are understood, what may be called the high humanity of our simple scheme of life becomes clear; and clear, too, becomes its peculiar adaptability to the conditions of modern life.

In the study of modern societies, it is a great gain to recognize in the beginning that the only fixed thing is change. There can be nothing stable in a world the chief characteristic of which, now at least, is a continuous readjustment of man's relations to man, and especially in his relations to the mass of men—constant readjustment, that is, in all human relationships, and in all social and governmental problems. Such instances as I have used indicate continuous change—change in educational ideals and aims, change in industrial conditions and therefore in governmental relations to industry, change brought by new physical discoveries, by improvements in transportation, change bringing new problems in every department of activity. Now, when nothing is stationary, the more easily your social and political organization is adaptable to change, the less trouble you will have; in other words, the more mobile you can keep your society, if only you can keep its character true, the better it will fare.

The last appeal, of course, in a democracy is to the will of the majority; and the will of the majority, we have proved over and over again, will come back, after whatever aberrations, to the fundamental striving for the greatest possible freedom of opportunity.

Now, the will of the majority is not without its tyrannies, but its tyrannies can be neither terrible nor enduring so long as no majority can become fixed and come to be regarded by men as a part of the order of nature. A democracy turns to and reverses itself when it awakes to an error. And there is an orderly way at hand always to reverse itself. This constant restlessness keeps it safe. For this reason, the preservation of the mobility of society is of vastly greater importance than either the definite failures or the definite successes of any mere problems of government during any given period. Neither a government nor a society can safely trust to the generosity of its especial beneficiaries; it can trust for its continued well-being only to the easily provoked activity of its victims, and its victims must have power to enforce their protest. A majority is not so likely to abridge the liberty of others as it is to widen its own privileges; and that is what you want. For, as a rule, no class ever gets liberty or opportunity that it does not itself take. Nobody confers liberty or opportunity on it as an alms. Alms-givers try to repair the damage that society has already suffered. They can seldom prevent further damage being suffered: that usually comes from the self-assertion of those who are the victims of discrimination or are in danger of becoming victims.

In other words, we in the United States have constantly to *struggle* for our ideals—for our liberties of opportunity, social, industrial, and political. We cannot rest and feel that they are secure.

We never reach a stage where we can say: "Now our task is done; we have only to hold fast to what we have gained." Rather, we have to work without weariness and without rest to make sure that our majorities shall be wise—in other words, that the whole people shall be trained to trustworthiness of judgment.

One great advantage of this constant vigilance which comes of constant change is that the eyes of men are kept towards the future. There is no time nor chance to look backward. The phrase in the President's inaugural address that roused the whole nation was this: "I call all forward-looking men to my side." No man in the Republic is willing to confess that he is a backward-looking man. The prodigious educational effort all looks to the future—millions of money and the best energies of many of the best men are all spent on making to-morrow better than yesterday. "What shall our children be?" not "What were our forefathers?" is the insistent question.

This is the explanation of the dominant note of hopefulness in American life. Men differ in their judgment of the past and of the present: in their expectation of the future they are one. Nothing matters much if we leave a clear opportunity for all the children of the Republic.

And this buoyancy, this hopefulness, I rate among the very greatest assets of our democracy. It is its spiritual quality. We have a life in which there is no social despondency, no political despair, no national fear.

And we have discovered that this dominant cheerfulness and hopefulness make for unselfish service. The preacher to one of the large American universities told me that of the thousands of young men who had sought spiritual advice from him not one had asked, "What shall I do to be saved?" but that every one had asked, "What can I do to save somebody else?" In a society that does not deceive itself into thinking that it has a fixed and stable basis, the very necessity of self-preservation drives men to help those who need help. Concern for the forgotten man may be altruism, but it is also self-preservation. He may turn up some day as our master. But when the forgotten are helped to an opportunity they must expect no other help. It is a mere truism that the unfortunate members of any society are numerous in proportion to the decline of personal efficiency and self-reliance. In an ideal democracy there would be none of these. In our fairly working democracy they are relatively few.

Thus does our democracy lay heavy burdens on the shoulders of every citizen. No thoughtful citizen can escape the particular responsibility that belongs to him to see to it, in so far as he can, that no class in society is forgotten by society as a whole, for that is where danger begins. A sober gratitude for a chance to work for this ideal runs through the speeches and the state papers and the prayers of the Fathers of the Republic. This same sober gratitude is still the dominant mood of the American citizen; for he believes in the constant improvement of human society, and he knows that the thing which we call government is only one instrument to use in making this constant improvement. Since no democratic government can be better than the people who make it, the first duty of a democracy is the right training of all its people; and this is a far larger task than the management of any mere machinery of government.

[W. H. P.]

GENERAL MONTHLY MEETING,

Monday, July 6, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Mrs. Walter Beevor,
George Gascoigne Blake, Esq.
Mrs. W. H. Crocker,
Kenneth Phipson Hawksley, Esq.
Mrs. K. P. Hawksley,
The Hon. Geoffry Lawrence Parsons,
Edward Sanger-Shepherd, Esq.
Miss H. C. Schott,

were elected Members of the Royal Institution.

The **PRESENTS** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Concrete for July 1914. 8vo.
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Electrical Engineering for June 1914. 4to.
Electrical Industries for June 1914. 4to.
Electrical Review for June 1914. 4to.
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Illuminating Engineer for July 1914. 8vo.
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Journal of Physical Chemistry for June 1914. 8vo.
Junior Mechanics for June 1914. 8vo.
Law Journal for June 1914. 8vo.
London University Gazette for June 1914. 4to.
Model Engineer for June 1914. 8vo.
Musical Times for June 1914. 8vo.
Nature for June 1914. 4to.
New Church Magazine for July 1914. 8vo.
Page's Weekly for June 1914. 8vo.
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Power-User for June 1914. 8vo.
Science Abstracts for June 1914. 8vo.
Terrestrial Magnetism for June 1914. 8vo.
War and Peace for July 1914. 8vo.
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Electrical Engineers, Institution of—Journal, Vol. LII. No. 236. 8vo. 1914.
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Florence, Biblioteca Nazionale—Bulletin for June 1914. 8vo.
Franklin Institute—Journal, Vol. CLXXVII. No. 6. 8vo. 1914.
Geographical Society, Royal—Journal, Vol. XLIV. No. 1. 8vo. 1914.
Geological Society—Abstracts of Proceedings, No. 961. 8vo. 1914.
Hills, Major E. H., C.M.G. R.E. D.Sc. F.R.S. M.R.I.—Academia Groningana, 1614–1914. 8vo. 1914.
Kansas, University—Science Bulletin, Vol. VIII. Nos. 1–10. 8vo. 1914.
London County Council—Gazette for June 1914. 4to.
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Mechanical Engineers, Institution of—Journal, 1914. No. 1. 8vo.
Mersey Conservancy—Report on the Present State of the Navigation of the River Mersey (1913). 8vo. 1914.
Meteorological Office—Monthly Weather Reports for May 1914. 4to.
Weekly Weather Reports for June 1913. 4to.
Geophysical Memoirs, No. 8. 4to. 1914.
International Meteorological Committee: Report of the 10th Meeting, Rome, 1913. 8vo. 1914.
Mexico, Sociedad Cientifica Antonio Alzate—Memorias, Tome XXXII. Nos. 9–10; Tome XXXIII. Nos. 9–10. 8vo. 1914.
Microscopical Society, Royal—Journal, 1914, Part 3. 8vo.
Monaco, Institut Océanographique—Bulletin, Nos. 289–297. 8vo. 1914.
Navy League—The Navy for July 1914. 8vo.
New York, Society for Experimental Biology—Proceedings, Vol. XI. No. 4. 8vo. 1914.
New Zealand, Agent General—Patent Office Journal for May 1914. 8vo.
Statistics, 1912, 2 vols. 4to. 1914.
North of England Institute of Mining Engineers—Transactions, Vol. LXIV. Nos. 3–5. 8vo. 1914.
Paraguay, Consul General for—Picturesque Paraguay. By A. K. Macdonald. 8vo. 1911.
Paris, Société d'Encouragement pour l'Industrie National—Bulletin for May–June 1914. 8vo.
Pharmaceutical Society of Great Britain—Journal for June 1914. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LXVI. Part 1. 8vo. 1914.
Photographic Society, Royal—Journal, Vol. LIV. No. 6. 8vo. 1914.
Physical Society of London—Proceedings, Vol. XXVI. Part 4. 8vo. 1914.
Post Office Electrical Engineers, Institution—Journal, Vol. VII. Part 2. 8vo. 1914.
Royal College of Surgeons—Annual Report on the Museum, 1914. 8vo.
Royal Engineers' Institute—Journal, Vol. XX. No. 1. 8vo. 1914.
Royal Society of Arts—Journal for June 1914. 8vo.
Royal Society of London—Proceedings: B, Vol. LXXXVII. No. 599. 8vo. 1914.
Philosophical Transactions: B, Vol. CCV. No. 317. 4to. 1914.
Royal Society of New South Wales—Journal, Vol. XLVII. Part III. 8vo. 1913.
St. Petersburg, Imperial Academy of Sciences—Bulletin, 1914, Nos 10–11. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXV. No. 6. 8vo. 1914.
Saxon Academy of Sciences, Royal—Abhandlungen: Math. Phys. Klasse, Band XXXIII. Nos. 1–2; Phil. Hist. Klasse, Band XXX. Nos. 2–3. 8vo. 1914.
Berichte: Math. Phys. Klasse, 1913, Nos. 4–5, 1914, No. 1; Phil. Hist. Klasse, 1913, Nos. 3–4. 8vo.
Scottish Geographical Society—Scottish Geographical Magazine, Vol. XXX. No. 7. 8vo. 1914.
Selborne Society—Selborne Magazine for July 1914. 8vo.

Smithsonian Institution—Report on United States National Museum, 1913. 8vo. 1914.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. III. Disp. 5. 4to. 1914.
South Africa, Union of—Agricultural Journal for May 1914. 8vo.
Statistical Society, Royal—Journal, Vol. LXXVII. Part 7. 8vo. 1914.
Swithinbank, H., Esq., J.P. F.R.S.E. M.R.I. (*the Author*)—Aspects of the Cornish Pilchard Fishery, II. (Mera Publications, No. 2.) 8vo. 1914.
Tariff Reform League—Monthly Notes on Tariff Reform for July 1914. 8vo.
United Service Institution, Royal—Journal for June 1914. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. II. No. 3. 8vo. 1914.
Report of the Chief of the Weather Bureau, 1912-13. 4to. 1914.
United States Patent Office—Gazette, Vol. CCII. No. 5—Vol. CCIII. No. 4. 8vo. 1914.
Upsala, Observatoire Meteorologique—Observations Seismographiques, 1907-1912. Par J. Koraen. 8vo. 1914.
Western Society of Engineers—Journal, Vol. XIX. No. 4. 8vo. 1914.
Zoological Society of London—Proceedings, 1914, Part 2. 8vo.
Transactions, Vol. XX. Parts 7-10. 4to. 1914.

GENERAL MONTHLY MEETING,

Monday, November 2, 1914.

Sir James Crichton Browne, J.P. M.D. LL,D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Banister Flight Fletcher, Esq., F.R.I.B.A.

was elected a Member of the Royal Institution.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Palæontologia Indica: New Series, Vol. IV. Memoirs, 2-4; Fifteenth Series, Vol. IV. Part 2, Fasc. 4; Vol. VII. Memoir, No. 1. 4to. 1910-13.
Agricultural Journal, Vol. IX. Part 3. 8vo. 1914.
Agricultural Research Institute, Pusa, Bulletin, Nos. 40-42. 8vo. 1914.
Kodaikanal Observatory Bulletin, No. 40. 4to. 1914.
Memoirs of Department of Agriculture: Chemical Series, Vol. III. Nos. 6-9. Entomological Series, Vol. IV. Nos. 6-8. 8vo. 1914.

British Museum, Trustees—Catalogue of Lepidoptera Phalænæ, Vol. XIII. and Plates. 8vo. 1913.
Catalogue of Ungulate Mammals, Vol II. 8vo. 1914.
Catalogue of Mesozoic Plants: The Cretaceous Flora. Part 1. 8vo. 1913.
Catalogue of Cuneiform Tablets: Supplement. 4to 1914.
Catalogue of Greek Coins: Palestine. 8vo 1914.
Catalogue of India Coins: Gupta Dynaties. 8vo. 1914.
Catalogue of Engraved British Portraits, Vol. IV. 8ov. 1914.
Catalogue of Hindu Books: Supplementary. 4to. 1913.
Monograph of the Sabicea. 8vo. 1914.
Revision of Ichneumonidæ, Part 3. 8vo. 1914

Accademia dei Lincei Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XXIII. 1° Semestre, Fasc. 11. 8vo. 1914.

Accountants, Association of—Journal, Vol. VII. Nos. 32-35. 8vo. 1914.

Aeronautical Society—Journal, Vol. XVIII. No. 71, July. 1914. 8vo.

Alchemical Society—Journal, Vol. II. Part 13. 8vo. 1914.

Allegheny Observatory—Publications, Vol. III. Nos. 13-14. 4to. 1914.

American Academy of Arts and Sciences—Proceedings, Vol. L. Nos. 1-3. 8vo. 1914.

American Geographical Society—Bulletin, Vol. XLVI. Nos. 7-10. 8vo. 1914.

American Philosophical Society—Proceedings, Vol. LIII. Nos. 213-214. 8vo. 1914.
Asiatic Society, Royal—Journal, 1914, July-Oct. 8vo.
Asiatic Society, Royal, Bombay Branch—Journal, No. 77, Vol. XXIII. 8vo. 1914.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIV. Nos. 8-9. 8vo. 1914.
List of Fellows, June 1914. 8vo. 1914.
Bankers, Institute of—Journal, Vol. XXXV. No. 7. 8vo. 1914.
Bibliographical Society—Transactions, Vol. XII. 8vo. 1914.
List of Members, May 1914.
Boston Public Library—Bulletin, Third Series, Vol. VII. Nos. 2-3. 8vo 1914.
Botanic Society, Royal—Botanical Journal, Vol. III. Nos. 2-3. 1914. 4to.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXI. Nos. 17-20. 4to. 1914.
List of Members, 1914.
British Astronomical Association—Journal, Vol. XXIV. Nos. 9-10. List of Members, 1914. 8vo. 1914.
Buenos Ayres—Monthly Bulletin of Municipal Statistics for March-April 1914. 8vo.
Cambridge Philosophical Society—Transactions, Vol. XXII. No. 5. 4to. 1914.
Proceedings, Vol. XVII. Part 6; Vol. XVIII. Part 1. 8vo. 1914.
Canada, Department of Mines—Geological Survey Memoirs, Nos. 19, 22, 32, 39, 40, 45, 47, 48, 49, 52. 8vo. 1914. Summary Report for 1912. 8vo 1914.
Museum Bulletin, No. 2. 8vo. 1914.
Prospector's Handbook, No. 1. 8vo. 1914.
Canada, Royal Society of—Proceedings and Transactions, Third Series, Vol. VII. 8vo. 1914.
Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, Nos. 82-83. 8vo. 1914.
Chemical Industry, Society of—Journal, Vol. XXXIII. No. 13-20. 8vo. 1914.
Chemistry, Institute of—Proceedings, Parts 1-3. 8vo. 1914.
List of Fellows, 1914. 8vo.
History of the Institute, 1877-1914. 8vo. 1914.
Lectures on Explosives. By Wm. Macnab. 8vo. 1914.
Chemical Society—Journal for July-Oct. 1914. 8vo.
Proceedings, Vol. XXX. Nos. 431-432. 8vo. 1914.
List of Fellows, 1914. 8vo.
Chile, Meteorological Institute—Observations, Nos. 4-6. 4to. 1913-14.
Civil Engineers, Institution of—Proceedings, Vols. CXCV.-CXCVI. 8vo. 1914.
Charter, Supplemental Charters, By-Laws, and List of Members, 1914. 8vo.
Cornwall Polytechnic Society, Royal—Eighty-first Annual Report, New Series, Vol. II. Part 3. 8vo. 1914.
Crisp, Sir Frank, Bart., LL.B., M.R.I.—The Genus Rosa. By E. Willmott. 2 Vols. fol. 1910-14.
Dax, Société de Borda—Bulletin, 1914, No. 1. 8vo. 1914.
Dewar, Professor Sir James, LL.D., F.R.S.—Scritti Intorno alla Teoria Molecolare ed Atomica ed Alla Notazione Chimica di S. Cannizzaro. 8vo. 1896.
East India Association—Journal, N.S. Vol. V. No. 3. 8vo. 1914.

Editors—Agricultural Economist for July-Nov. 1914. 8vo.
Amateur Photographer for July-Oct. 1914.
American Journal of Science for July-Oct. 1914. 8vo.
Ateneo Veneto for May-Aug. 1914. 8vo.
Athenæum for July-Oct. 1914. 4to
Author for July-Nov. 1914. 8vo.

Editors—continued.
Chemical News for July-Oct. 1914. 4to.
Chemist and Druggist for July-Oct. 1914. 8vo.
Church Gazette for July-Sept. 1914. 8vo.
Concrete for July-Nov. 1914. 8vo.
Dyer and Calico Printer for July-Oct. 1914. 4to.
Electrical Engineering for July-Oct. 1914. 4to.
Electrical Industries for July-Oct. 1914. 4to.
Electrical Review for July-Oct. 1914. 4to.
Electrical Times for July-Oct. 1914. 4to.
Electricity for July-Oct. 1914. 8vo.
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Ferro-Concrete for July-Oct. 1914. 8vo.
Horological Journal for July-Oct. 1914. 8vo.
Illuminating Engineer for August-Oct. 1914. 8vo.
Journal of the British Dental Association for July-Nov. 1914. 8vo.
Journal of Physical Chemistry for July-Oct. 1914. 8vo.
Junior Mechanics for July-Nov. 1914. 8vo.
Law Journal for July-Oct. 1914. 8vo.
London University Gazette for July-Oct. 1914. 4to.
Marine Engineer for July-Nov. 1914. 8vo.
Model Engineer for July-Oct. 1914. 8vo.
Musical Times for July-Oct. 1914. 8vo.
Nature for July-Oct. 1914. 4to.
New Church Magazine for Aug-Nov. 1914. 8vo.
Nuovo Cimento for March-June 1914. 8vo.
Page's Weekly for July-Oct. 1914. 8vo.
Physical Review for July-Sept. 1914. 8vo.
Power for July-Oct. 1914. 8vo.
Power User for July-Nov. 1914. 8vo.
Science Abstracts for July-Oct. 1914. 8vo.
War and Peace for July-Oct. 1914. 8vo.
Wireless World for Aug.-Nov. 1914. 8vo.
Zoophilist for July-Oct. 1914. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LII. Nos. 234-235. 8vo. 1914.
List of Members, 1914. 8vo. 1914.
Faraday Society—Transactions, Vol. X. Part 1. 8vo. 1914.
Florence, Biblioteca Nazionale—Bulletin for July-Oct. 1914. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, 1914, Disp. 2. 8vo.
Franklin Institute—Journal, Vol. CLXXVIII. Nos. 1-4. 8vo. 1914.
Year Book, 1914. 8vo. 1914.
Geographical Society, Royal—Journal, Vol. XLIV. Nos. 2-5. 8vo. 1914.
Geological Society—Abstracts of Proceedings, No. 962. 8vo. 1914.
Journal, Vol. LXX. Part 2. 8vo. 1914.
Geological Literature, 1912. 8vo. 1914.
List of Fellows, 1914. 8vo.
Geological Survey of Great Britain, The Director—Summary of Progress, 1913. 8vo.
Godfrey, Dr. C. E. (the Author)—Army of the United States in the Anticipated War with France, 1798-1900. 8vo. 1914.
Huntington, Archer M., M.A.—Atlas of Portolan Charts. 4to. 1911.
Initials and Miniatures of the IXth, Xth and XIth Centuries. fol. 1904.
Poem of the Cid, Vols. I. II. and III. 1897, 1901, 1903. 4to.
Cancionero General (1520). 4to. 1904.
Cancioneiro de Resende (1516). 4to. 1904.
Cervantes: Don Quixote, Fasc. 1-2, with 2nd ed. of Fasc. 1. 8vo.

Imperial College of Science—Calendar, 1914–15. 8vo. 1914.
Imperial Institute—Bulletin, Vol. XII. Nos. 2–3. 8vo. 1914.
Iron and Steel Institute—Journal, Vol. LXXXIX. 8vo. 1914.
Carnegie Scholarship Memoirs, Vol. VI. 8vo. 1914.
Johns Hopkins University—American Journal of Philology, Vol. XXXV. No. 2. 8vo. 1914.
University Studies, Series XXXII. No. 2. 8vo. 1914.
University Circular, New Series, Nos. 1–6, 10. 8vo. 1914.
Leland Stanford Junior University—Publications: The Birds of the Latin Poets. Acceleration of Development in Fossil Cephalopoda. A Morphological Study of Some Members of the Genus Pallavicinia. 8vo. 1914.
Life-Boat Institution, Royal National—The Life-Boat for July–Oct. 1914. 8vo.
Linnean Society—Transactions: Botany, Vol. VIII. Parts 3–6; Zoology, Vol. XVI. Parts 1–3. 4to. 1913–14.
Journal: Botany, Vol. XLII. Nos. 286–287; Zoology, Vol. XXXII. Nos. 217–218. 8vo. 1914.
Literature, Royal Society of—Transactions, Second Series, Vol. XXXIII. Part 1. 8vo. 1914.
Report and List of Fellows. 8vo. 1914.
Liverpool, University of—Calendar for 1914–15. 8vo. 1914.
London County Council—Gazette for July–Oct 1914. 4to..
London Society—Journal for July 1914. 8vo.
Manchester, Literary and Philosophical Society—Memoirs and Proceedings, Vol. LVIII. Part 2. 8vo. 1914.
Mechanical Engineers, Institution of—Proceedings, 1914, Parts 1–2. 8vo. 1914.
Meteorological Office—Monthly Weather Reports for June–Oct. 1914. 4to.
Weekly Weather Reports for July–Oct. 1914. 4to.
Hourly Values, 1912, Geographical Section. 4to. 1914.
Meteorological Society, Royal—Quarterly Journal, Vol. XL. No. 171. 8vo. 1914.
Metropolitan Water Board—Eleventh Annual Report. 8vo. 1914.
Metropolitan Asylums Board—Annual Report for 1913. 8vo. 1914.
Microscopical Society, Royal—Journal, 1914, Part 4. 8vo. 1914.
Milan, School of Agriculture—Notizie Regolamenti e Programmi. 8vo. 1914.
National Physical Laboratory—Collected Researches, Vol. XI.
Report for the Year 1913–14. 4to. 1914.
Navy League—The Navy for Aug.–Nov, 1914. 8vo.
New York, Society for Experimental Biology and Medicine—Proceedings, Vol. XI. Nos. 5–6. 8vo. 1914.
New York Academy of Sciences—Annals, Vol. XXIII. pp. 145–53; Vol. XXIV. pp. 1–17, 19–113, 115–170. 8vo. 1914.
New Zealand, Patent Office—Journal, July, 1914. 8vo.
North of England Institute of Mining Engineers—Transactions, Vol. LXIV. Part 6.
Annual Report, 1913–14. 8vo. 1914.
Numismatic Society, Royal—Numismatic Chronicle, 1914, Part 2. 8vo.
Onnes, Dr. H. Kamerlingh—Communications from the Physical Laboratory of the University of Leiden, Supplement, No. 36. 8vo. 1914.
Paris, Office National des Universités Françaises—Les Universités et les Écoles Françaises. 8vo. 1914.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for July, 1914. 4to.
Paris, Société Française de Physique—Journal de Physique for May, 1914. 8vo.
Petroleum Technologists, Institution of—Journal, Vol. I. Part 1. 8vo. 1914.
Pharmaceutical Society of Great Britain—Journal for July–Oct. 1914. 8vo.
Physical Society of London—Report on Radiation and the Quantum Theory. By J. H. Jeans. 8vo. 1914.
Port of London Authority—London, The Port of the Empire. 4to. 1914.
Post Office Electrical Engineers, Institution of—Papers, No. 53. 8vo. 1914.
Journal, Vol. VII. Part 3. 8vo. 1914.

Ralli, Mrs. Stephen, M.R.I.—Political Life of M. Tricoupi, 7 Vols. 8vo. 1907-12.
Rome, Ministry of Public Works—Giornale del Genio Civile for July-Aug. 1914. 8vo.
Röntgen Society—Journal, Nos. 40-41. Vol. X. April 1914. 8vo.
Royal Colonial Institnte—United Empire, Vol. V. Nos. 7-10. 8vo. 1914.
Year Rook, 1914. 8vo.
Royal Engineers' Institute—Journal, Vol. XX. Nos. 2-5. 8vo. 1914.
Royal Irish Academy—Proceedings, Vol. XXXI. (Clare Island Survey) Parts 6-7, 9, 21, 47, 66; Vol. XXXII. A, Nos. 3, 4, B, Nos. 3, 4, C, Nos. 10-13. 8vo. 1914.
Royal Society of Arts—Journal for July-Oct. 1914. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXXIV. Part 2. 8vo. 1914.
Transactions, Vol. XLIX. Parts 3-4; Vol. L. Part 1. 4to. 1913-14.
Royal Society of London—Philosophical Transactions, A, Vol. CCXIV. Nos. 514-519; B, Vol. CCV. Nos. 318-323. 4to. 1914.
Proceedings, A, Vol. XC. Nos. 619-622; B, Vol. LXXXVII. Nos. 599-602. 8vo. 1914.
Royal College of Surgeons—Calendar for 1914. 8vo. 1914.
Sanitary Institute, Royal—Journal, Vol. XXXV. Nos. 7-10. 8vo. 1914.
Sarawak Museum—Journal, Vol. II. Part 1. 8vo. 1914.
Scottish Geographical Society—Royal Scottish Geographical Magazine, Vol. XXX. Nos. 8-10. 8vo 1914.
Selborne Society—Selborne Magazine for Aug.-Nov. 1914. 8vo.
Smithsonian Institution—Miscellaneous Collections, Vol. LXIII. Nos. 1 and 7. 8vo. 1914.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. III. Disp. 6*a*-9*a*. 4to. 1914.
South Africa, Union of—Agricultural Journal for June-Aug. 1914. 8vo.
Statistical Society, Royal—Journal, Vol. LXXVII. No. 8. 8vo. 1914.
Stirling, Professor William (the Author)—Health, Fatigue, and Repose. 8vo. 1914.
Tariff Reform League—Monthly Notes on Tariff Reform for July-Oct. 1914. 8vo.
Tôhoku Imperial University, Sendai, Japan—Science Reports, 1st Series, Vol. III. No. 4. 8vo. 1914.
Toronto University—Studies: Chemical, Nos. 99-100. 8vo. 1913.
United Service Institution, Royal—Journal for July-Oct. 1914. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XXX. Nos. 7-9. 8vo. 1914.
Journal of Agricultural Research. Vol. II. Nos. 3-4. 8vo. 1914.
Farmers' Bulletin, Nos. 559-565. 8vo. 1914.
United States Department of Commerce—Bulletin of the Bureau of Standards, Vol. X. Nos. 2-3. 8vo. 1914.
United States Department of the Interior—Geological Survey: Bulletins, Nos. 540, 543, 546, 547, 550, 552-554, 556-558, 564, 571, 574, 575, 579, 580 A, B, C, D, E, 581 A, B, 585. 8vo. 1914.
Water Supply Papers, Nos. 309, 322-324, 327, 340 A, B, 345 A, B, C, E, F. 8vo. 1914.
Professional Papers, Nos. 81-84; 85 D, E, 90 A, B, C, D. 4to. 1914.
Mineral Resources, 1913, Part 1, Nos. 1-5; Part 2, Nos. 1-13, 15-16, 19. 8vo. 1914.
United States Naval Observatory—Publications, Second Series, Vol. VIII. 4to. 1914.
American Ephemeris and Nautical Almanac for 1916. 8vo. 1914.
United States Patent Office—Official Gazette: Vols. CCIV.-CCVI.; CCVII. No. 1. 8vo. 1914.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1914, Heft 6. 4to.

Vienna, Imperial Geological Institute—Jahrbuch, Band LXIII. Heft 4. 8vo. 1914.

Western Australia, Agent-General—Monthly Statistical Abstract for March–June, 1914. 4to.

Geological Survey Bulletin, Nos. 48, 51, Part 2, 52–54. 8vo. 1912–14.

Western Society of Engineers—Journal, Vol. XIX. Nos. 5–7. 8vo. 1914.

Yorkshire Philosophical Society—Annual Report, 1913. 8vo. 1914.

Zoological Society—Proceedings, 1914, Part 3. 8vo.

List of Fellows. 8vo. 1914.

Zurich, Naturforschenden Gesellschaft—Vierteljahrsschrift, 1914, Heft 1–2. 8vo. 1914.

GENERAL MONTHLY MEETING,

Monday, December 7, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

His Highness The Maharaja Gaekwar of Baroda,
Archer M. Huntington, Esq.

were elected Members of the Royal Institution.

The Secretary reported, That the Managers, at their Meeting held this day, had appointed Professor Charles Scott Sherrington, M.D. D.Sc. F.R.S., Fullerian Professor of Physiology, for a term of three years, in conformity with the Deed of Endowment, the appointment dating from January 14, 1915.

The following Lecture Arrangements were announced ;—

PROFESSOR C. V. BOYS, F.R.S. *M.R.I.* Six Lectures (adapted to a Juvenile Auditory) on SCIENCE IN THE HOME. 1. MECHANICS IN THE HOME; 2. CHEMISTRY IN THE HOME; 3. FLUIDS IN THE HOME; 4. HEAT IN THE HOME; 5. ELECTRICITY IN THE HOME; 6. LIGHT IN THE HOME. On Dec. 29 (*Tuesday*), Dec. 31, 1914; Jan. 2, 5, 7, 9, 1915.

PROFESSOR CHARLES SCOTT SHERRINGTON, M.D. LL.D. F.R.S., Fullerian Professor of Physiology, Royal Institution. Six Lectures on MUSCLE IN THE SERVICE OF NERVE. On *Tuesdays*, Jan. 19, 26; Feb. 2, 9, 16, 23.

PROFESSOR WILLIAM J. POPE, LL.D. F.R.S. Two Lectures on COLOUR PHOTOGRAPHY—SCIENTIFIC APPLICATIONS: 1. PHOTOGRAPHIC APPRECIATION OF COLOUR IN MONOCHROME; 2. PHOTOGRAPHY IN NATURAL COLOURS. On *Tuesdays*, March 2, 9.

PROFESSOR SIR JAMES G. FRAZER, D.C.L. LL.D. Litt.D. Two Lectures on THE BELIEF IN IMMORTALITY AMONG THE POLYNESIANS. On *Tuesdays*, March 16, 23.

HENRY G. PLIMMER, Esq., F.R.S. M.R.C.S. *M.R.I.*, Pathologist to Zoological Society. Three Lectures on MODERN THEORIES AND METHODS IN MEDICINE: 1. MEDICINE AND SCIENCE; 2. IMMUNITY; 3. METHODS AND RESULTS. On *Thursdays*, Jan. 21, 28, Feb. 4.

P. CHALMERS MITCHELL, Esq., D.Sc. F.R.S., Secretary, Zoological Society of London. Three Lectures on ZOOLOGICAL STUDIES: WAR AND EVOLUTION: 1. NATIONS AS SPECIES; 2. STRUGGLE OF SPECIES; 3. STRUGGLE OF NATIONS. On *Thursdays*, Feb. 11, 18, 25.

SIR HERBERT WARREN, K.C.V.O. D.C.L. LL.D., President, Magdalen College, Oxford. Two Lectures on POETRY AND WAR. On *Thursdays*, March 4, 11.

AUBREY STRAHAN, Esq., Sc.D. LL.D. F.R.S., Director, Geological Survey of Great Britain. Two Lectures on LONDON GEOLOGY: 1. THE FORM AND STRUCTURE OF THE LONDON BASIN; 2. THE GROUND BENEATH LONDON. On *Thursdays*, March 18, 25.

RICHARD T. GLAZEBROOK, Esq., C.B. D.Sc. F.R.S., Director of the National Physical Laboratory. Two Lectures on AERIAL NAVIGATION—SCIENTIFIC PRINCIPLES: 1. THE REQUIREMENTS OF THEORY; 2. THE REQUIREMENTS OF PRACTICE. On *Saturdays*, Jan. 23, 30.

HENRY WALFORD DAVIES, Esq., Mus.Doc. LL.D. Two Lectures on MUSIC: 1. TO UNTRAINED LISTENERS (Illustrations by Lecturer); 2. EMERGENCY MUSIC (Illustrations by a small Choir). On *Saturdays*, Feb. 6, 13.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. F.R.S., Professor of Natural Philosophy, Royal Institution. Six Lectures on RECENT RESEARCHES ON ATOMS AND IONS. On *Saturdays*, Feb. 20, 27, March 6, 13, 20, 27.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute Bulletin, No. 43. 8vo. 1914.
Agricultural Journal, Vol. IX. Part 4. 8vo. 1914.
Kodaikanal Observatory Bulletin, Nos. 41, 42. 4to. 1914.
Geological Survey: Records, Vol. XLIV. Part 2. 8vo. 1914.
Lords of the Admiralty—Nautical Almanac for 1917. 8vo. 1914.
Accountants, Association of—Journal, No. 36, Vol. VII. 8vo. 1914.
Allegheny Observatory—Publications, Vol. III. Nos. 15, 16. 4to. 1914.
American Geographical Society—Bulletin, Vol. XLVI. No. 11. 8vo. 1914.
Aristotelian Society—Proceedings, Vol. XIV. 8vo. 1914.
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WEEKLY EVENING MEETING,

Friday, May 8, 1914.

CHARLES HAWKSLEY, ESQ., M.Inst.C.E., Vice-President, in the Chair.

PROFESSOR KARL PEARSON, F.R.S.

Albinism in Men and Dogs.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, January 23, 1914.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR SIR JAMES DEWAR, LL.D. D.Sc. F.R.S. *M.R.I.*,
Fullerian Professor of Chemistry, R.I.

The Coming of Age of the "Vacuum Flask."

TWENTY-ONE years ago the application of vacuum jacketed vessels to the storage and manipulation of liquefied gases or for conserving hot liquids was made in the Laboratory of the Royal Institution. Such vessels are now popularly called "Thermos Flasks." The insulating properties of a vacuum in preventing the influx of heat by the elimination of convection currents had been known to physicists ever since the investigations of Dulong and Petit on the laws of radiation, in the early part of the last century, proved the important part played by the gas particles surrounding a body in dissipating heat, otherwise than by pure radiation. As far back as 1873 a highly exhausted annular metallic vessel was used by the lecturer in calorimetric experiments described in my paper on "The Physical Constants of Hydrogenium." * This naturally suggested that the use of high vacua in the annular space surrounding vessels containing liquid gases would be advantageous. The experiments which led up to the use of glass vessels isolated by a vacuum space are fully described in the Friday Evening Discourse on January 20, 1893.† Some of the early forms of vessels then described are shown in Fig. 1.

One of the primary difficulties was connected with the construction in glass of concentric spherical or cylindrical vessels that would withstand the atmospheric pressure, and bear considerable oscillations of temperature without cracking. This was a serious impediment which was only gradually overcome by the growing demand and by improvements in the blowing and adequate annealing of the glass: some of the various developments of form rising out of the needs of low temperature research are figured in the abstract of the Discourse of January 18, 1901, which is now reproduced.

A sufficient vacuum can be obtained with the ordinary Sprengel pump. Glass flasks so exhausted were quite satisfactory for heat insulation purposes, the convective transference of heat being stopped. If, however, the inner vessels were coated with a bright metallic deposit then the radiation influx is very greatly diminished. This is readily shown by making a half-silvered vessel. The radiation from the silvered and the clear sides respectively is compared by presenting each in turn to a thermopile connected to a galvanometer, whose indications are shown by a moving spot of light on the screen. Two such vessels are filled with liquid air and hot water respectively; scarcely any movement of the spot of light is seen when the silver side is turned to the pile, but on rotating to the unsilvered side a strong

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* Trans. Roy. Soc. Ed. xxvii. p. 167.
† Proc. Roy. Inst., xiv. p. 1.

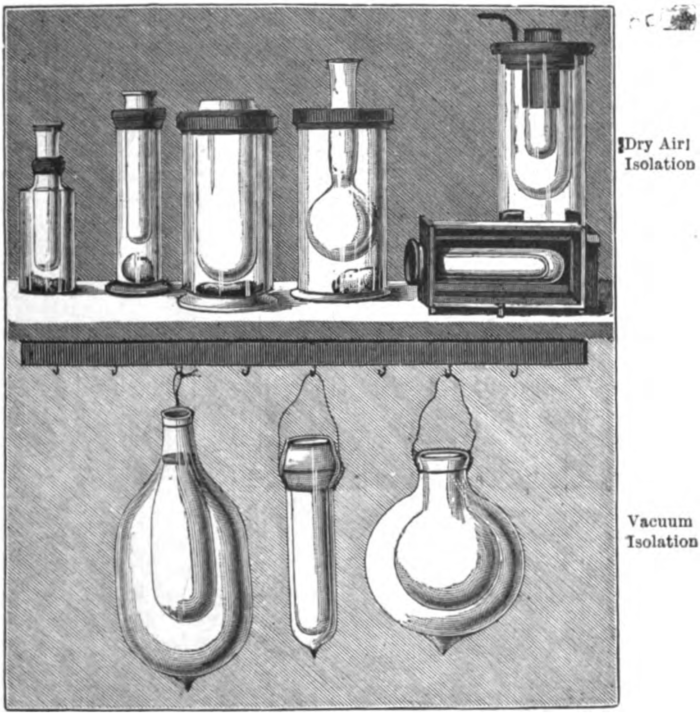

Fig. 1.

The upper vessels in this figure are for dry air isolation, whereby the deposition of moisture at the low temperature was prevented. In each of these old forms it will be noted that a mass of phosphoric anhydride placed in the lower portion is required to absorb traces of water. The vacuum vessels described equally retard the loss as well as the gain of heat (as shown in Fig. 3).

deflection is observed, in one direction with the liquid air vessel and to the opposite side when the vessel of hot water is substituted.

Another aid to the heat insulation of vacuum vessels is the introduction of finely divided powders to the exhausted space: the results of the use of some of these are given in my discourse on "Liquid Air as an Analytic Agent."*

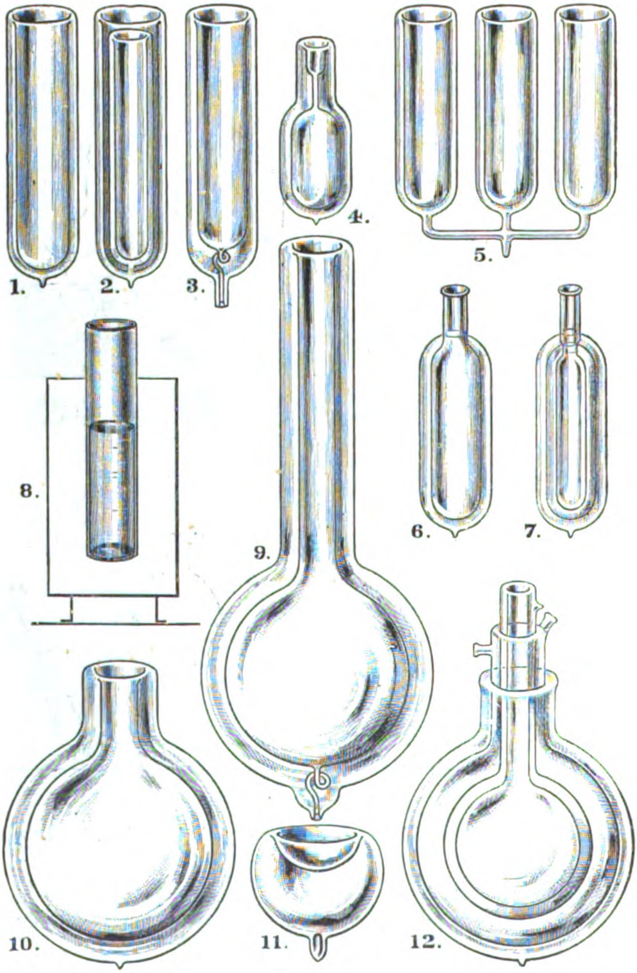

FIG. 2.

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* Proc. Roy. Inst., xv. p. 823.

Another method of producing the requisite high vacua is by means of a mercurial vacuum made by distilling mercury through the annular space while exhausting by a good pump.* On placing a liquid gas in such a flask the mercury vapour left in the annular space is condensed as a bright mirror on the cool inner wall; and if used solely for low temperature work, this is entirely satisfactory, the need for silvering the inner surfaces is then partly obviated.

This action is illustrated in an exhausted globe containing a little mercury. There is a shallow depression in the globe into which a little liquid air is poured (see 11, Fig. 2); almost at once a mirror of mercury is deposited inside on the cooled part and the increased isolation results in a diminution of the rate of evaporation. A difficulty arises, however, if the vacuous space is not of reasonable cross-section, because the vapour of mercury is not only dense, but has a high viscosity, and therefore cannot get freely to the cooled surface, which results in the production of a poor mirror of limited extent. Thus, if another globe is taken having the mercury in a small bulb separated from the main globe by an inch of capillary tubing,† then on cooling a small area with a sponge of liquid air, the vapour saturating the globe is deposited as a localised mirror. If, however, a second area be then similarly cooled no deposit is obtained, because practically no mercury vapour can get in through the capillary; this is proved by decanting a little mercury out of the small bulb into the globe, when instantly a mirror is deposited on the cooled area.

The practical effect of this property is shown by the relative isolations of two cylindrical vacuum vessels in the same condition as to exhaust and both containing a little mercury in the bottom. The annular vacuum space in one is about 1 c.m. wide, while the width of the other is only about 2 mm. Liquid air is poured into both and the relative rates of evaporation measured by connecting successively to each, an indiarubber tube delivering into a gas jar filled and inverted over water. A metronome indicates the time taken to fill the jar with the evaporating air in each case; the result shows that the vessel with the narrow annular exhaust space is worse than the one with the wider space, and on examination this is explained by the smaller extension of the mercury mirror in the narrow-spaced vessel.

A pure water vapour discharge tube provided with two platinum wire electrodes illustrates the condensing action of liquid air in the production of a high vacuum in vacuum vessels for any vapours therein contained. The tube shown was about 5 c.m. diam. and 35 c.m. long. This tube is saturated with water vapour at the temperature of melting ice. On connecting the electrodes to a sparking coil the lilac-glow of hydrogen fills the tube. When a sponge of liquid air is applied in less than a minute the water vapour is condensed; the glow disappears; the tube passes through the phosphorescent stage, and finally becomes too highly vacuous for a discharge to pass. On removing the sponge the phenomena are reversed.

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* Proc. Roy. Inst., xiv. p. 395.

† See Fig. 3, Proc. Roy. Inst., Jan. 12, 1894.

As concrete examples of the extent of the heat isolation, it can be stated that liquid air in 5-litre vacuum flask loses 12·5 grms. per hour, or 8 per cent per day. If contents weigh 3650 grms., trace is left on the twelfth day. Heat exchange per minute per sq. c.m. = 0·007 calories.

Taking radiation law of heat exchange and silver emissivity, heat exchange by theory per min. per sq. c.m. = 0·015 calories, the observed experimental value being 0·01.

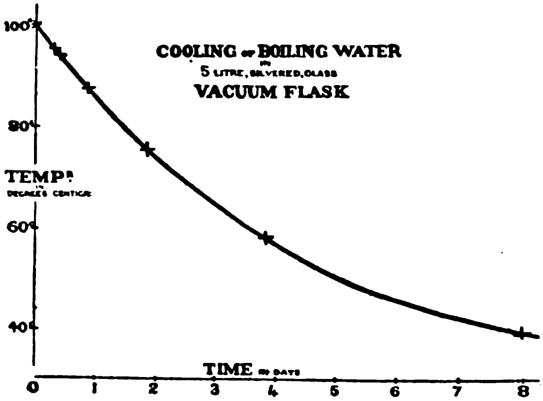

FIG. 3.

1·5 litre silvered vacuum vessel lost 9·5 grms. per hour, or 15 per cent on first day. Trace left on sixth day.

Fig. 3 gives the curve for decrement of temperature from the boiling-point of water in a silvered vacuum vessel, showing the same low rate of loss of heat as compared with gain of heat in the former example.

Some twelve years later a further advance was made by the application of the well-known absorptive power of charcoal. The possibility of the occlusion of gases in porous bodies has been a subject of scientific inquiry for more than two hundred years, and has been investigated by a long line of famous chemists. The Discourse on "New Low Temperature Phenomena," in the year 1905, was devoted to a consideration of the thermal evolution and absorption of gases by charcoal at low temperatures. A year later the application of this knowledge was made to the production of very high vacua, by placing a small quantity of charcoal in a globular recess in the vacuous space of the flask. When such a vessel was filled with liquid air, the charcoal was also cooled and thus absorbed remaining traces of air, producing thereby a vessel with very high heat insulating qualities.

The employment of charcoal made it possible to use metals such as brass, copper, or nickel, instead of glass, for the construction of vacuum vessels, with obvious advantages. Metals give off small quantities of gas for a long time, which diminish the vacuum in such flasks, if it were not maintained by some charcoal, cooled to the low temperature of liquid air, or other liquefied gas, placed in the vessel. Many forms of such vessels have found a ready application to low temperature work, not only for storage and transport, but in cases where rough usage was unavoidable, or where the fractures to which glass is liable might be dangerous; also when high pressures are to be employed along with low temperatures metallic construction is necessary, and only by the addition of charcoal condensation can the necessary heat insulation be secured.

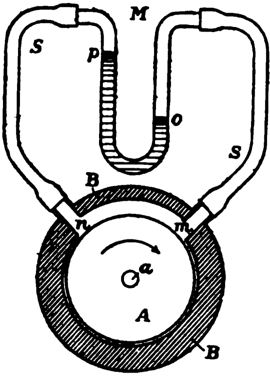

FIG. 4.

The most recent achievement in the attainment of a high exhaust is Dr. Gaede's invention of the Molecular pump, which is capable of producing a vacuum of ·000001 mm. of mercury.

The working principle of this pump is shown in Fig. 4. A is a revolving cylinder fixed on a shaft *a* enclosed in an outer casing B. In the inner side of B a groove *n* to *m* is cut, communicating at either end through the casing with a mercury gauge M. When the cylinder rotates from left to right, or clockwise, the friction set up amongst the molecules of the gas or air contained in the groove causes it to be driven from n to m, and the mercury in the gauge rises to p in the left limb, owing to the diminished pressure, and is depressed to o in the right limb. The difference in pressure is proportional to the number of revolutions of the cylinder and the internal friction of the gas. In practice the grooves are cut in the cylinder, and the tongues, which project into the grooves, are fixed on the casing. The speed of rotation of the central shaft carrying the cylinder is about 8000 revolutions per minute.

The efficiency of this pump is seen when it is used direct on a large discharge tube 2 m. long and 6 c.m. in diameter, provided with electrodes in the manner of a De la Rue tube. In about a minute from starting at atmospheric pressure all stages of discharge at low pressures are traversed, and finally only a flickering phosphorescence is seen. A further test on liquid oxygen shows the manner in which a rapid evolution of gas is overcome. The melting pressure of oxygen is too low to be realised by usual exhaust methods, as was shown in the Discourse of January 20, 1911, when the solidification of oxygen was shown to be possible only by the exhaust produced by charcoal and liquid air. Formerly it was necessary to employ cooling by liquid hydrogen to obtain oxygen in the solid state. If, however, the fitting on the exhaust nozzle of the Gaede pump be directly connected to a vacuum vessel containing some 10 c.c. of liquid oxygen (isolated as usual by being surrounded with an outer vessel of liquid air), three minutes' working of the pump results in the oxygen being transformed into solid state.

Low Temperature Calorimetry.

The application of vacuum tube methods has resulted in bringing the law of Dulong and Petit into relation with the other laws governing the behaviour of the atoms. Working at ordinary laboratory temperatures Dulong and Petit had shown that the atomic heats for the various elements (got by multiplying the specific heat and atomic weight) was a constant of an approximate value six. For working at 50° Absolute (– 223° C.), a liquid hydrogen calorimeter has been constructed in which the specific heat of the various elements can be easily determined in terms of the volume of gas produced in cooling the substances. Under such conditions very different results from those previously obtained have been deduced. The atomic heats exhibit the same rough periodicity of function with atomic weights as is shown by other properties of the elements. A double curve demonstrates the complete parallelism between the atomic volumes and the atomic heats of the principal elements (see Fig. 7). In connection with speculations as to the constitution of the atoms, physicists had been putting different interpretations on the structure of the atom, discussing vortices of electrons, central positive charges, and so forth. It was remarkable, in view of these experiments, that some of them had predicted that the atomic heats must be chiefly determined by the atomic volumes and the elastic constants.

Atomic Specific Heats between the Boiling Points of Liquid Nitrogen and Hydrogen.

The Mean Atomic Specific Heats at 50° Absolute of the Elements, a Periodic Function of the Atomic Weights.

A method of determining the specific heat of substances at low temperatures was described in a paper on "Studies with the Liquid Hydrogen and Air Calorimeter,"* also in the abstract of a Friday Evening Discourse in 1904 on "Liquid Hydrogen Calorimetry,"† where the apparatus then used is illustrated.

Continuing the use of the same method, but with some modification of the apparatus, the investigation has been extended to a large number of inorganic and organic bodies. In this later series of experiments, the measurements of the specific heats of materials by the liquid hydrogen calorimeter were made over a range of temperature from boiling nitrogen to boiling hydrogen, a fall of temperature of some 57° Absolute.

Weighed pieces of the material are cooled to the temperature of boiling nitrogen in a quartz cooling vessel of special design. By a simple mechanical device they are then released from this vessel, and drop into liquid hydrogen in the calorimeter below. The resulting volumes of hydrogen evaporated are measured. From this value and a knowledge of the latent heat of the liquid hydrogen and the mass of the substance, the specific heat can be calculated as follows :—

If V is volume of hydrogen at N.T.P. evaporated by the fall of 1 grm. of the substance through T degrees above the temperature of boiling hydrogen, and s its specific heat, L being the volume of hydrogen evaporated by 1 calorie, then

$$sT = \frac{V}{L}, \quad \textit{i.e.} \quad s = \frac{V}{TL}.$$

The latent heat of liquid hydrogen is taken as 115 calories, and therefore L = 97·05 c.c.; also in these experiments T is constant (57°·5) therefore

$$s = V \times 0{\cdot}0001792.$$

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* Roy. Soc. Proc., A, lxxvi. p. 325.

† Roy. Inst. Proc., xvii. p. 581.

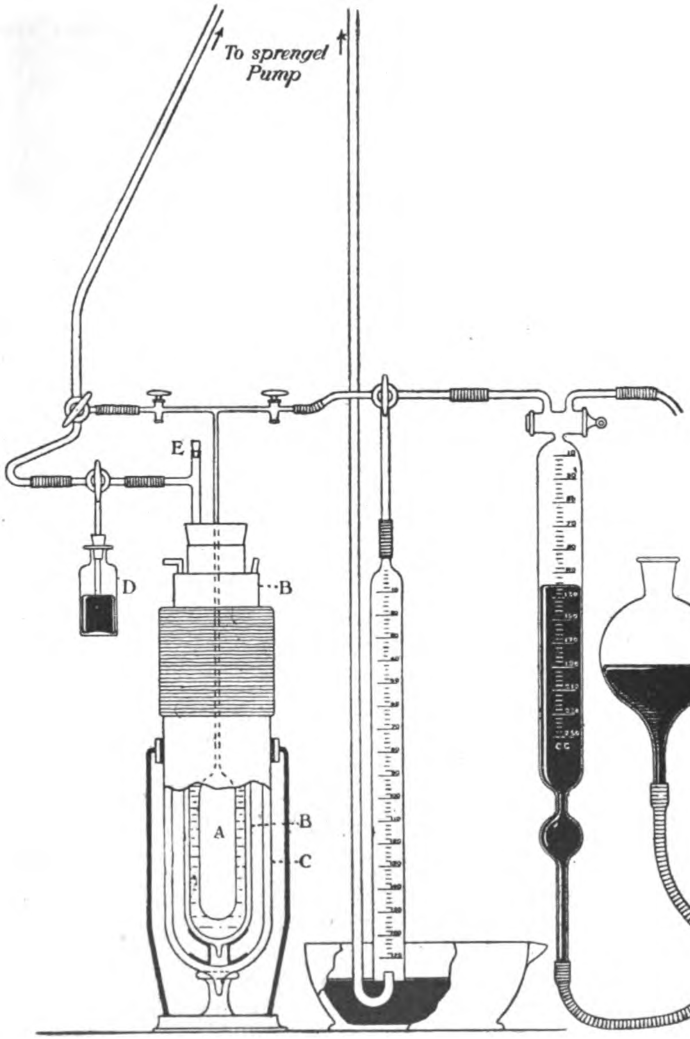
To sprengel Pump
E
D
B
A
B
C
CC

Fig. 5.

Thus the hydrogen volumes measured are reduced to V at N.T.P. and simply multiplied by the constant factor 0·0001792, thereby giving the specific heat.

In an earlier Paper* the value of the latent heat of hydrogen was taken as 122·92. Five observations were given varying ±5 about this mean value. This was determined on the basis of 0·0291 being accepted as the mean specific heat of lead from 15° C. to 20° Abs. Several Willard Gibbs vapour pressure formulæ, however, calculated from the vapour tensions of liquid hydrogen, give a mean value of 115 for the latent heat of hydrogen, so that the specific heat of lead over this range would seem to be 0·0272.

In connection with the computation of the latent heat of liquefaction of hydrogen it is essential to know the value of the specific volumes of saturated hydrogen between 15° and 20° Abs. This was determined in the following simple manner: a carefully calibrated bulb A was charged with hydrogen to saturation at liquid hydrogen temperatures, and the volume of the hydrogen so required determined by pumping out the bulb. The figure (Fig. 5) shows the bulb A immersed in liquid hydrogen in vacuum vessel B, which is further isolated in the usual way in an external vacuum vessel C of exhausted liquid air. The hydrogen evaporating from the liquid was used to saturate the space to the same pressure as existed in B, the tube dipping into the bubbling bottle of hydrogen D was lengthened to more than barometer height when the liquid hydrogen pressure was reduced below atmospheric to obtain temperatures below the boiling point; for this purpose the exhaust was connected by a stopcock on E. The various connecting stopcocks and collecting and measuring apparatus shown indicate the necessary manipulations.

The values obtained at the boiling point and near the melting point respectively are shown in the following Table:—

SPECIFIC VOLUME OF SATURATED HYDROGEN.

Temperature	Pressure	Vol. of Unit Weight (Specific Volume)	Ratio to Density of H. at the same Pressure and 0° C.
20·4° Abs.	760 mm.	747·4	14·94
15·3° „	115 „	3914	18·84

However, the relative weights of liquid and saturated vapour thus determined, only involve a correction of about 1% in the calculated latent heat, which is within the experimental variations.

Taking the value of 115 for the latent heat of hydrogen, the resulting value of the specific heat of lead from 80° Abs. to 20° Abs. is now 0·02399. This is the mean value of a series of nearly

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* Roy. Soc. Proc., A., lxxvi., p. 325.

30 observations. The greatest variations from this mean value were 0·0247 and 0·0233, but the majority of the values varied to a much less extent. This is equivalent to an atomic specific heat of 4·965. It may be mentioned that a computation from Nernst and Lindemann's* real specific heats of lead within the same range of temperature gives the mean value 5 18.

In the observations with the liquid air calorimeter the specific heats were calculated in each case by direct comparison with lead observed at the same time, as explained in the earlier papers. This is most convenient because liquid air varies in composition on standing, and therefore in the volume evaporated by unit amount of heat.

LIQUID HYDROGEN CALORIMETER.

The liquid hydrogen calorimeter is a glass cylindrical bulb vacuum vessel A (Fig. 6) of 50 c.c. capacity, silvered, with ½ cm. slit. The inner diameter is 3 cm. On to the neck, contracted to about 1·7 cm., is sealed a glass tube B of equal diameter and 30 cm. long. This projects about 8 cm. through the brass coned fitting cap F of an ordinary slit silvered vacuum vessel in which it is supported. A side delivery tube, 1 cm. wide, provided with a stopcock D of 8 mm. bore, is sealed near the top of B. A short length of rubber tubing on the neck of F makes a gas-tight joint with B. To minimise splashing, and to reduce the impact of the falling pieces, a thin strip of german silver or lead E, bent out near the top into a shoulder about 1 cm. square, stands centrally in the calorimeter A. The strip is cut from a thin tube of about the same diameter as the calorimeter neck. A short length of the tube is left above the shoulder, and supports the strip by fitting loosely into the neck of A. The shoulder is arranged just above the level of the liquid hydrogen in A, which is at least three-quarters full. Some such device is essential in the use of this form of the liquid hydrogen calorimeter.

The calorimeter in its turn is immersed in liquid hydrogen in the supporting vacuum vessel C, the neck of the calorimeter being 8 to 10 cm. below the liquid hydrogen surface. This vacuum vessel C is only slightly wider than the lower part of A, and is provided with a coned cap F, whereby it is also supported and completely immersed in a wider vacuum vessel G containing exhausted liquid air. G is also fitted with a brass coned cover, fitting vacuum-tight to the cap F on C. Both caps are pierced by two thin tubes, one for fitting on to the filling syphons, the other, bent at right angles, serves for connecting to the exhaust in the case of the liquid air vessel, and in the case of the liquid hydrogen vessel to the stopcock leading the evaporating hydrogen through the upper part of the apparatus.

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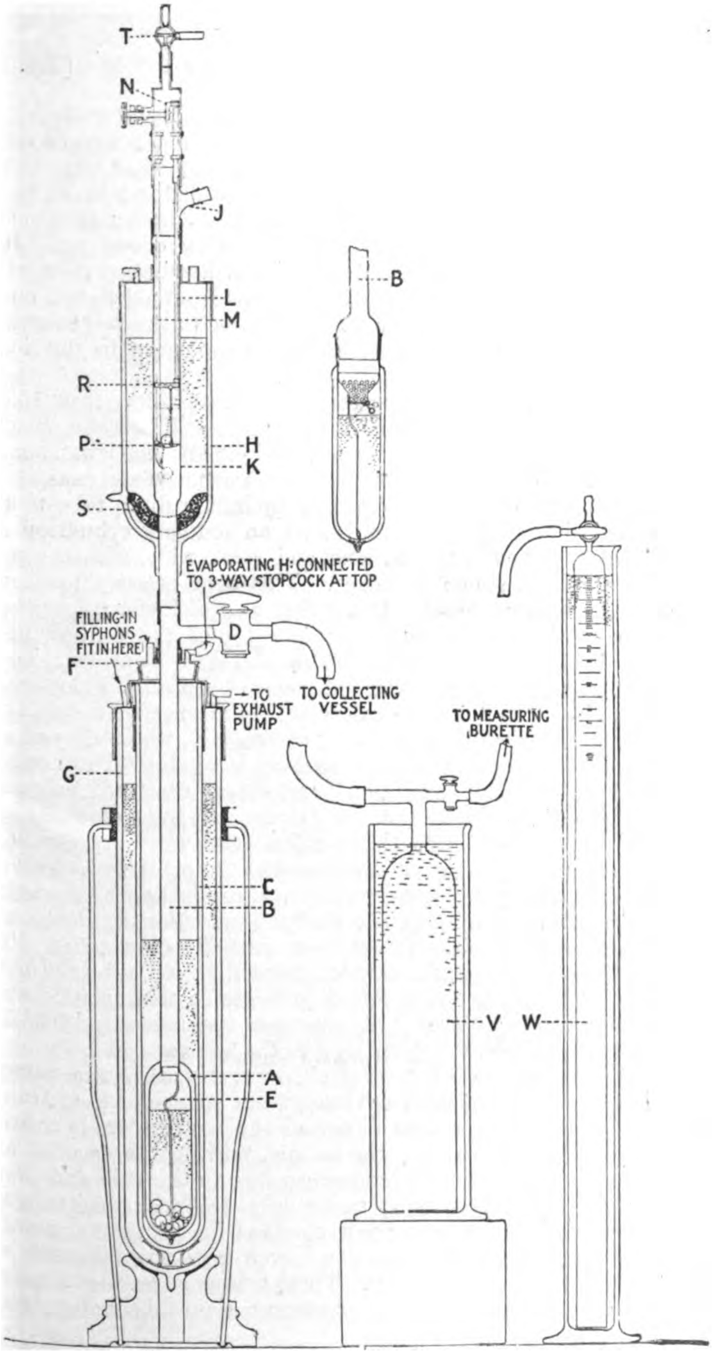
* Sitzungsber. d. Berl. Akad., 1911, p. 494.


T
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EVAPORATING H: CONNECTED TO 3-WAY STOPCOCK AT TOP
FILLING-IN SYPHONS FIT IN HERE
D
F
→ TO EXHAUST PUMP
TO COLLECTING VESSEL
TO MEASURING BURETTE
G
C
B
V
W
A
E

Fig. 6.

This arrangement thus charged only needs a little liquid air sucked in every one and a-half hours. The liquid hydrogen vessel will not need replenishing for at least four hours. The level of the liquid hydrogen in the calorimeter does not fall 1 cm. in six hours with constant use. The bulk of the materials added roughly compensates for the volume of the liquid hydrogen evaporated. It is important that this level should not materially change, since, after striking the shoulder, bodies move more slowly, are deflected on to the cold wall, and low results are obtained due to the longer cooling of the materials in the vapour before being immersed in the liquid hydrogen.

The isolation of the calorimeter was such that less than 10 c.c. of hydrogen gas evaporated from it per minute. The whole vacuum vessel combination is supported between cork-lined spring jaws mounted on a heavy metal base, on which the outer vacuum vessel rests.

The cooling vessel H is connected by india-rubber tube to the top of the calorimeter. It consists of an ordinary cylindrical slit silvered vacuum vessel, 30 cm. long and 7 cm. wide, with a central axial open tube K sealed in below. This tube passes through the liquid in the vacuum vessel. It has the same diameter below as the neck tube of the calorimeter. Near the top of the central tube a side tube J, of about the same diameter, and some 3 cm. long, serves for the introduction of the weighed pieces of material, which are all cooled previously to the temperature of liquid air, and then fall on to a thin metal pan P fitting loosely the tube K, where they remain about fifteen minutes. P is supported by being hinged to two thin ebonite rods, L and M, fixed to a brass fitting cemented on to the top. The rod L is not fixed directly to the disc but to a metal ring R. From the ring R two thin vertical steel wires are connected freely to two points on the circumference of the pan below. This rod and the attached ring can be given a vertical motion by a crank N in the top fitting, thereby tipping the pan and releasing the piece of material, which then falls freely down into the calorimeter. The level of the pan is approximately one-third the vertical height of the cooling vessel. Quartz was found to be safer than glass for the construction of this vessel. A high vacuum was maintained by filling with charcoal a cross-tube S, opening to the annular space.

At the temperature of boiling nitrogen, the convection currents in the central tube of such a vessel, when connected to the calorimeter below, have no serious effect on the temperature in the tube at a reasonable distance from the bottom, provided the central tube be not wide. The difference of temperature in the tube and in the surrounding liquid was found to be only 0·5° when the tube was 1·5 cm. wide. With a larger pattern vessel the width of the central tube was increased to 2·2 cm., and even here the difference was under 3° at the level of the pan. These temperatures were measured by a small helium thermometer, consisting of a 4 c.c. bulb to which was sealed a small mercury manometer of fine capillary tubing. The thermometer was filled with pure helium to 273 mm. pressure at 0° C. The reading of the mercury manometer thus gives, with slight corrections previously determined, the absolute temperature. It is scarcely necessary to add that, by exhausting the liquid nitrogen, a lower initial temperature than 78° Abs. can be secured.

The hydrogen evaporating from the liquid in the vacuum vessel C, in which the calorimeter is immersed, is employed in the interval of observations to maintain a hydrogen atmosphere through the neck of the calorimeter and the connected measuring tubes. Risk of solid air in the calorimeter neck is thus obviated. A simple arrangement of a three-way cock T, connected at the top of the brass fitting on the central tube of the cooling vessel, allows this to be manipulated. The hydrogen thus passes continually in at the tubular top of the fitting on the central tube of the cooling vessel, and out through the stopcock on the calorimeter neck, and through the cocks to the measuring vessels. When an observation is to be made, the three-way cock is turned to allow the hydrogen current to escape to the laboratory, thereby closing off the calorimeter, which now only connects to the collecting and measuring vessels V and W.

V consists of a glass tube 8 cm. in diameter and 40 cm. long, open at the bottom and provided with a wide T-piece at the top. The tube is immersed to the neck in water in a glass cylinder, and is counterpoised by a weight and cord running over a pulley just above. It is thereby readily raised during the time gas is being evaporated from the calorimeter; this ensures that no back pressure is produced. One arm of the T-piece is open and connects to the stopcock D on the calorimeter neck; the other is provided with another small stopcock and connects to a 200 c.c. gas burette W similarly immersed in water. This latter stopcock is closed during the collecting of the gas from a dropped piece of material. This being completed, the calorimeter stopcock is closed while the evaporated hydrogen gas is measured by transference to the burette, the slight continual evaporation from the calorimeter meanwhile finding a vent through the three-way cock T at the top of the cooling vessel, which is now turned on. These arrangements are necessary to secure the minimum impediment to the evaporating hydrogen, which is usually evolved in less than 10 seconds, any temporary back pressure being fatal to concordant results. At least 15 seconds are allowed for collecting the gas given off, and even longer, in some cases, with badly conducting bodies.

A slightly different form of calorimeter vacuum vessel was used on some occasions, but without modifying the results. Instead of the constriction at the top of A, a ground conical neck was used of the same diameter as the inner tube. This neck was fitted with a similar ground conical hollow tube sealed on to the tube B.

Anhydrous glycerin was used to make the ground joint tight at the low temperatures. The wider neck in some ways simplifies the preliminary manipulations, and allows a more efficient arrangement to be securely fixed for preventing splashing, and breaking the fall of the bodies dropped into the calorimeter. The arrangement is shown in the side sketch B (Fig. 6). It consists of a light counterpoised trap door hinged at the lower end of a conical brass or german silver tube, supported in and fitting to the inner tube of the calorimeter. A small lead ball on a wire soldered to the trap door keeps this closed until struck by a falling sphere, while shutting it again immediately after the sphere passes through. The conical tube above the trap door is pierced with several small holes to leave a free passage for the evaporating hydrogen. The addition of a gauze filter to intercept spray was found to be impracticable, the resistance introduced causing back pressure. In later work the calorimeters were constructed of quartz, a little charcoal in the bottom of the annular space serving to maintain a very high vacuum isolation.

As far as possible the materials used were cast in the forms of spheres about 8 mm. diameter, and for this purpose the use of an ordinary bullet mould was found convenient. In the case of liquid bodies, the mould was first cooled by liquid air. Frequently liquids were frozen into solid cylinders in thin glass tubing, and pieces cut off after removing the glass mould. The metallic materials were in some cases fused into buttons of convenient weight in an exhausted quartz tube. The lead, however, of which many pieces were required, were cut from rod, and subsequently squeezed in a small spherical mould.

Volatile bodies were weighed at a low temperature on a light german silver pan supported by a thin platinum wire suspension from the balance pan about 2 cm. above the level of liquid air contained in a wide deep vacuum vessel. Some materials would not make coherent bullets or cast sticks. These were filled into very thin walled cylindrical metal capsules of equal weight, so that a preliminary determination of the volume of hydrogen evaporated by the metal of the capsule gave the correction. They were then cooled on an aluminium dish floating on liquid air, filled with the fluid, and weighed separately. Materials which could not be fused were compressed hydraulically into small blocks and cut up into pieces of suitable dimensions.

At least three pieces of every substance were dropped. The results rarely varied among themselves by more than 2 to 3 per cent. Very frequently the agreement was within 1 per cent. In order to ensure good results, uniformity of shape and size in the pieces of material used is desirable, so that the manner of release and fall shall be comparable: because in the use of this instrument the materials had to pass through a region of the neck, between the cooling vessel and the calorimeter, where a considerable gradient of temperature exists. It was necessary that, as far as possible, the bodies used should be subjected to this variable region in the same manner, so that the amount of heat absorbed in transit should be nearly the same. This value was determined in the following manner. The warm portion of the calorimeter neck between the cooling vessel above and the vacuum vessels below was cooled by being surrounded with liquid air placed in a simple temporary fitting. Observations with various kinds of bodies were then made. The comparison of the reduced volumes now obtained with the values given by the ordinary use of this particular form of the instrument gives the correction factor. Many confirmations of the specific heats were made with this addition to the instrument. In the latest form of the calorimeter the neck is always surrounded with liquid nitrogen, thus abolishing all heat correction. This improved calorimeter gives results differing but slightly from the corrected values of the old instrument. The value of the atomic heat of lead is determined before and after any series of experiments as a check on the constancy of the instrument.

The values of the specific heats given will include any heat of transformation of glassy or crystalline modifications or other comparable change produced by the cooling to 20° Abs. Another effect is produced by some materials when used in the form of hydraulically compressed blocks. Such as are porous exhibit in varying degrees the phenomenon of heat evolution due to liquid hydrogen passing into the capillary spaces, thereby rendering the observed specific heat too great unless the proper correction is made. Any air occluded during the preliminary cooling of the porous material to liquid air temperature before being introduced to the hydrogen calorimeter is also included. Some of this air would be replaced by hydrogen through diffusion while in the hydrogen atmosphere in the central tube of the cooling vessel of boiling nitrogen : but the true remedy is the preliminary cooling of such porous bodies in a hydrogen atmosphere at the temperature of liquid air. That heat evolution continued in many cases for some time after the dropping of the material was shown by an increased rate of evolution of hydrogen from the calorimeter. As stated above, this is normally less than 10 c.c. per minute. In some cases the introduction of porous material increased this for some time to more than 30 c.c. per minute.

The values set out in Table I for the mean atomic specific heats of 53 elements at about 50° Abs. represent the results of some 200 calorimetric observations. When plotted in terms of their atomic weights, they reveal definitely a periodic variation resembling generally the well-known Lothar Meyer atomic volume curve for the solid state. The relations between the two curves is shown in Fig. 7. If experiments were similarly made between the boiling points of hydrogen and helium then in all probability the atomic specific heats would be all very small and nearly constant.

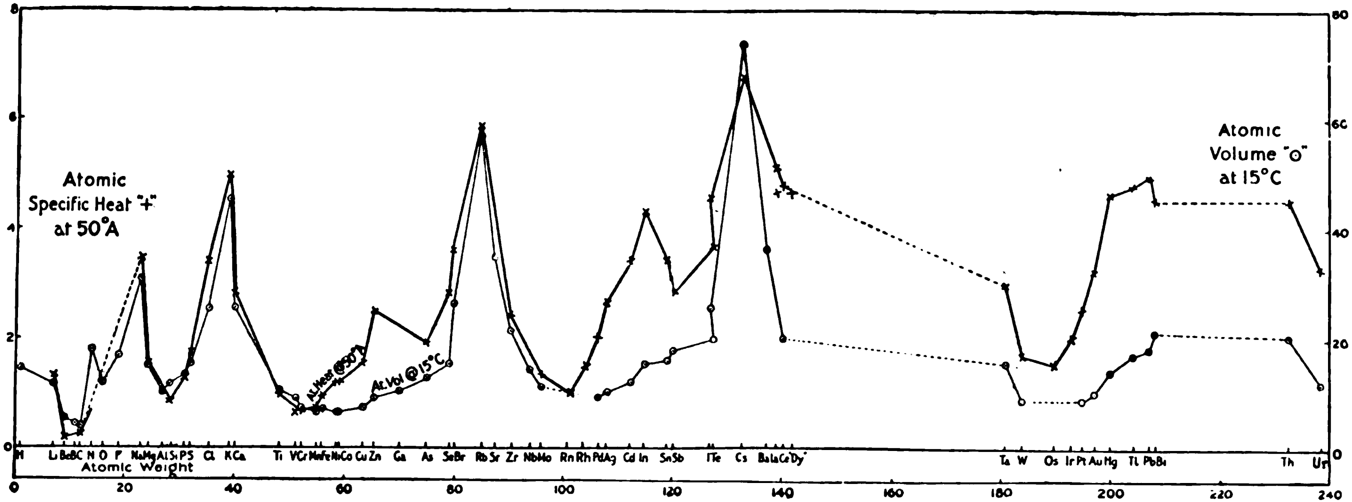
Atomic
Specific Heat "+"
at 50°A
Atomic
Volume "O"
at 15°C
At. Heat @ 50°A
At. Vol @ 15°C
H Li Be B C N O F Na Mg Al Si P S Cl K Ca Ti V Cr Mn Fe Ni Co Cu Zn Ga As Se Br Rb Sr Zr Nb Mo Rn Rh Pd Ag Cd In Sn Sb I Te Cs Ba La Ce Dy Ta W Os Ir Pt Au Hg Tl Pb Bi Th Ur
Atomic Weight
0
20
40
60
80
100
120
140
160
180
200
220
240
0
2
4
6
8
0
20
40
60
80

Fig. 7.

TABLE I.

Element	Atomic weight	Specific heat	Atomic heat
Lithium	7·03	0·1924	1·35
Glucinum	9·1	0·0137	0·125
Boron	11·0	0·0212	0·24
Carbon (Acheson graphite)	12·0	0·0137	0·16
Diamond	12·0	0·0028	0·03
Sodium	23·0	0·1519	3·50
Magnesium	24·4	0·0713	1·74
Aluminium	27·1	0·0413	1·12
Silicon, fused, elec. cruc.	28·4	0·0303	0·86
,, crystallised	28·4	0·0303	0·77
Phosphorus (yellow)	31·0	0·0774	2·40
,, (red)	31·0	0·0431	1·34
Sulphur	32	0·0546	1·75
Chlorine	35·45	0·0967	3·43
Potassium	39·15	0·1280	5·01
Calcium	40·1	0·0714	2·86
Titanium	48·1	0·0205	0·99
Chromium	52·1	0·0142	0·70
Manganese	55	0·0229	1·26
Iron	55·9	0·0175	0·98
Nickel	58·7	0·0208	1·22
Cobalt	59·0	0·0207	1·22
Copper	63·6	0·0245	1·56
Zinc	65·4	0·0384	2·52
Arsenic	75·0	0·0258	1·94
Selenium	79·2	0·0361	2·86
Bromine	79·96	0·0453	3 62
Rubidium	85·1	0·0711	6·05
Strontium, impure	87·6	0·0550	4·82
Zirconium	90·6	0·0262	2·38
Molybdenum	96·0	0·0141	1·36
Ruthenium	101·7	0·0109	1·11
Rhodium	103·0	0·0134	1·38
Palladium	106·5	0·0190	2·03
Silver	107·93	0·0242	2·62
Cadmium	112·4	0·0308	3·46
Tin	119	0·0286	3·41
Antimony	120·2	0·0240	2·89
Iodine	126·97	0·0361	4·59
Tellurium	127·6	0·0288	3·68
Cæsium	132·9	0·0513	6·82
Barium, impure	137·4	0·0350	4·80
Lanthanum	138·9	0·0322	4·60
Cerium	140·25	0·0330	4·64
"Didymium"	142	0·0326	4·63
Tungsten	184	0·0095	1·75
Osmium	191·0	0·0078	1·49
Iridium	193·0	0·0099	1·92
Platinum	194·8	0·0135	2·63
Gold	197·2	0·0160	3·16
Mercury	200	0·0232	4·65
Thallium	204·1	0·0235	4·80
Lead	207	0·0240	4·96
Bismuth	208	0·0218	4·54
Thorium	232·5	0·0197	4·58
Uranium	238·5	0·0138	3·30

PROPORTIONS OF DIFFERENT GASES LEFT UNCONDENSED AT 20° ABSOLUTE.

As an example of the further application of liquid hydrogen temperature, made possible by the aid of vacuum vessels, determinations had been made of the actual amount of material left uncondensed by liquid hydrogen, not only in atmospheric air, but in some earth gases and in the gaseous products of respiration. The investigation is not yet concluded, but some interesting evidence of the extensive distribution of hydrogen has been obtained. The principle of the method is as follows:—Measured volumes of the samples to be examined are passed after careful purification when necessary into an apparatus of known volume consisting essentially of a McLeod gauge as described in connection with the measurement of the production of helium from radium,* to which is sealed a condensing bulb cooled in liquid hydrogen. The gases under investigation are passed through this bulb, where all condensable material is retained, while the portion uncondensable by the liquid hydrogen passes on to the McLeod gauge, whereby its partial pressure is measured and its relative proportion to the total sample determined. This uncondensable would consist of helium, neon and hydrogen. The amount of the hydrogen alone was deduced in some cases by measuring the difference in partial pressure obtained when treatment by hot copper oxide was included in the first purification. All samples were subjected to liquid air temperature before being measured, to remove moisture and carbonic acid and general volatile impurity. In the case of respired air especially this is very necessary. As an example of the methods employed to prepare the necessary gas samples, the apparatus shown in Fig. 8 was used for the treatment of "Chemical Nitrogen" (as contrasted with "Air Nitrogen" obtained from the treatment of air by various reducing agents). The principle of the method employed was the liquefaction of purified nitrogen; produced from the action of nitrite of sodium and chloride of ammonium in solution in presence of bichromate of potassium; and the fractionation of the condensed product.

The flask A, of 500 c.c. capacity, was one-third filled with the reaction mixture. The neck of A fitted by ground joint to the 2-way stopcock B; one side of this opened to the air through a bubbler (not shown), whereby escape was allowed until the free space in A was well swept out with the evolving nitrogen. The other connection of B was sealed to two purifying bulbs containing respectively caustic soda, granules, and concentrated sulphuric acid as bubbler, with glass wool between to catch spray. Connection was then made to a 25 c.m. length of ordinary combustion tubing containing a roll of copper gauze partly oxidised, which when

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* Proc. Roy. Inst. XXIX. 733.

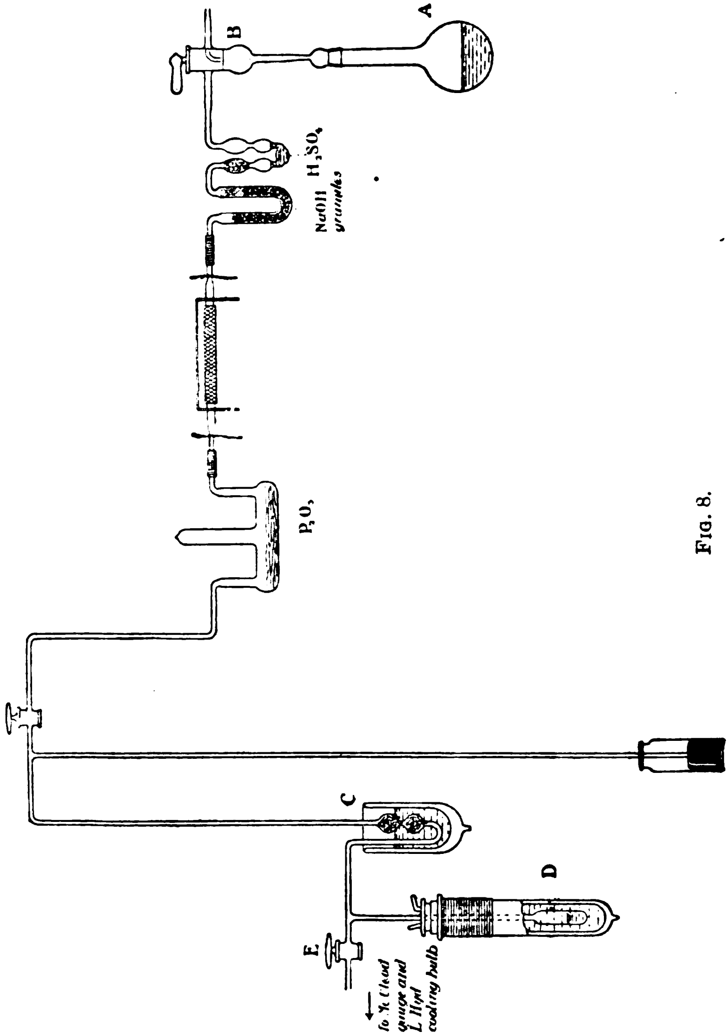

A
B
NaOH granules
H_2SO_4
P_2O_5
C
D
E
to McLeod gauge and L Hyd cooling bulb

FIG. 8.

treated by fan-shaped Bunsen removed traces of oxygen and hydrogen. Following this was a phosphoric anhydride bulb, to dry the gas roughly, to which was sealed a stopcock with "barometer" side tube dipping into a small bottle of mercury to measure the pressure. The remaining part of the apparatus was a bulbed U-tube C loosely packed with asbestos wool, cooled in liquid air, to dry and purify the gas and the condensation bulb D on side tube with a stopcock E connecting the McLeod gauge and liquid hydrogen cooling bulb. D was immersed in a vacuum vessel of liquid air arranged with vacuum tight coned metal fitting for exhausting liquid air to produce the requisite low temperature for condensing and fractionating off the nitrogen into the gauge. Only the least volatile part was examined.

On treating air with various reducing agents to remove oxygen, there is proof that measurable quantities of hydrogen appear which must be derived from the simultaneous decomposition of water. It will be noticed that the total volume of uncondensable gases in pure air as shown in the following table amounts to about one in forty thousand of the volume originally taken, while the hydrogen considered alone is of the order of one in two million. The uncondensable part in chemical nitrogen or oxygen shows how difficult it is to get rid of traces of helium and neon, and old liquid air, which ought to be very pure oxygen for this purpose, yet contains 15 times more uncondensable at 20° Absolute than the chemical product. Again this must come chiefly from adhering neon.

PROPORTION OF UNCONDENSABLE AT 20° ABSOLUTE IN GASES FROM VARIOUS SOURCES. (TABLE I.)

Material	Gas Pressure left of 760 mm. (Unit 10^{-4} mm.)	Parts per Million Volumes
Chemical nitrogen	0·33	0·043
Chemical oxygen	0·23	0·030
Air (pure)	179	23·6
Air (H removed)	173	22·8
Soil gas (6 in. deep)	167	22·0
Gas from fresh liquid air	66·4	8·74
Gas from old liquid air	5·1	0·67
Gas from liquid air machine	252	33·2
Gas from rain water	119	15·7
Gas from deep well water	127	16·7
Gas from surface well water (chalk)	224	29·5
Gas from earth brine	270	35·5
Gas from King's Well, Bath	11520	1516·0
Gas from King's Well (H removed)	10830	1425·0
Argon (theoretical value)	19000	2513
,, Commercial Sample 1	57800	7606
,, H removed ,,	13000	1710
,, Commercial Sample 2	2850	375
,, H removed ,,	2655	349

PROPORTION OF UNCONDENSABLE GAS AT 20° ABSOLUTE. AIR-NITROGEN OBTAINED IN DIFFERENT WAYS. (TABLE II.)

Reagent for removing Oxygen	Gas Pressure left of 760 mm. (Unit 10^{-4} mm.)	Parts per Million Volumes
(Theoretical value)	226	29·7
Hot copper and copper oxide	228	30·0
Yellow phosphorus (combustion)	220	28·9
Red phosphorus (combustion)	392	51·6
Red phosphorus (combustion), H removed	212	27·9
Ferrous oxide and water	1260	166·0
Ferrous oxide and water, H removed	203	26·7
Phosphorus (yellow) and water	524—1410	68·9—186
Phosphorus (yellow) and water, H removed	212	27·9
Copper and ammonia solution	707	93·0
Copper and ammonia solution, H removed	210	27·6
Sodium hydrosulphite	219	28·8
Cuprous chloride, acid solution	226	29·7
Pyrogallate (alkaline)	284	37·4
Pyrogallate (alkaline), H removed	211	27·9
Chromous chloride, acid solution	520—200,000	68·4—26,300
Chromous chloride, acid solution, H removed	236	31·1

By pursuing the same methods it has been found that hydrogen was steadily given off in the lung gases. The lecturer's production was about 100 c.c. a day; but that of one of the assistants gave about a quarter of a litre. Experiments have also been made on animals, from which it appears that hydrogen is being given off in respiration by warm-blooded animals. Hydrogen must be therefore escaping from the earth or is being oxidised in the upper air; unless we assume the proportion of hydrogen in the air is increasing in the lower state. Probably both processes are in progress. A summary of the results obtained by the analysis of lung gases is given below:—

PROPORTION OF UNCONDENSABLE GASES AT 20° ABSOLUTE IN RESPIRED AIR. (TABLE III.)

Samples: Different Persons taken at Royal Institution	Gas Pressure left of 760 mm. (Unit 10^{-4} mm.)	Parts per Million Volumes	
		Total Uncondensable	H Only
A	217–234	28·6–30·8	5·0–7·2
B	196–220	25·8–29·0	2·2–5·4
C	235	30·9	7·4
D	351	46·2	22·6
E	321	42·2	18·8
F	295	38·8	15·3
G	247–444	32·5–58·4	9·0–34·9
G hydrogen out	182	23·9	—
G at home	228–393	29·9–51·6	6·3–28·0
Animal	223–278	29·3–36·5	5·7–12·9
Ordinary air	179	23·6	0·8

As regards these Tables, it may be noted that all three give the records of similar processes of analysis. In II. air is taken; oxygen is removed in the various ways indicated; nitrogen is frozen out at 20° Abs., and the presence of minute quantities of rare gases and hydrogen remaining uncondensed is measured. In I. the sources of the gases were varied, and large changes in the quantity of uncondensable gases recorded. In III. several samples of lung gases were examined, and the hydrogen and other gases differentiated, the volume being found to be considerably greater than that in ordinary air, the variation from the mean quantity of about twenty-three volumes per million being due to the presence of hydrogen produced in the body. The method of taking a sample of respired air is shown in Fig. 9.

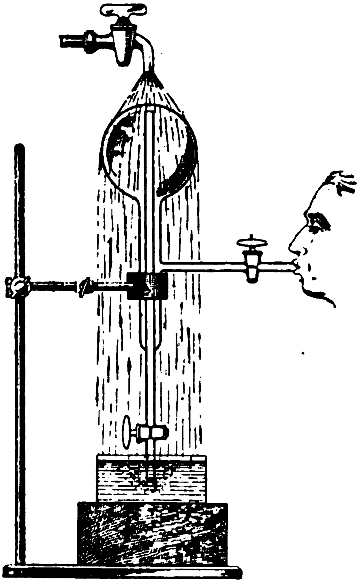


Fig. 9.

The flask has a ground cone for direct attachment to the purifying part of the apparatus employed, whereby the uncertainties introduced by india-rubber connections are avoided. Such precautions are very necessary where minute quantities of hydrogen, helium and neon are being measured. [J. D.]

Mr. W. J. Green, B.Sc., of the Davy Faraday Laboratory, has given valuable assistance in the conduct of the investigation; and Mr. J. W. Heath, F.C.S., in the Lecture Arrangements.

WEEKLY EVENING MEETING,

Friday, April 3, 1914.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. D.C.L. LL.D. D.Sc. F.R.S., in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. Pres.R.S. M.R.I., Professor of Natural Philosophy, R.I.

Further Researches on Positive Rays.

As I have on several occasions described the apparatus used to apply positive rays to chemical analysis, it will not be necessary this evening to enter more fully into the details of the method than to say that it consists :—

1. In electrifying the gases to be analysed by passing an electric discharge through them when at a low pressure.
2. A fine stream of positively electrified particles is obtained by inserting a long and very fine tube in the cathode.
3. The different kinds of particles forming this stream are separated by exposing them to strong electric and magnetic forces, arranged so that the deflection due to the magnetic force is at right angles to that due to the electric. The lighter particles are more deflected than the heavier one, and the original stream is split up into as many secondary streams as there are different kinds of particles.

These particles when they strike against a photographic plate leave their record, and can thus be photographed. The trace left by any particular kind of particle on the plate is the arc of a parabola, and arcs of the different parabolas on the plate correspond to different kinds of particles. [Examples of these photographs were shown ; one of these is given in Fig. 1.]

I will begin this evening by referring to some questions of technique. A great deal depends, as might be expected, on the character and qualities of the photographic plates used in these investigations. The effects these plates have to record are not due to light, but to the impact of particles against them ; these particles cannot penetrate beyond a very small distance, so that the front of the film is the only part which is brought into action. Thus, to get the best effects the film should be exceedingly thin and contain the maximum amount of silver. The old Daguerreotype (a silver plate exposed to iodine vapour) would satisfy these conditions, but as the film is not transparent the measurement of the parabolas offers great difficulties. I have got the best results with some plates brought out by Dr. Schumann for studying the "Schumann rays," which are light rays far in the ultra-violet, and so easily absorbed that they are stopped by a layer of air only 1 mm. thick. The instructions drawn up by Schumann for preparing these plates are given in Baily's "Spectroscopy" (2nd Edition), and following these one of my assistants has had considerable success in preparing these plates. I may say that the more closely we followed Schumann's instructions the better were the results; all the variations we introduced were failures. The films have their peculiarities; they are no good for some time after they have been made, they improve on maturing, and may turn out good or bad. When they are good they make a wonderful difference in the experiments, for an exposure of a minute or two will give as good a picture as one of an hour with some of the older plates. The saving of time is important not only for itself, but because it is easier to keep the experimental conditions constant for short exposures than for long ones, and thus to get sharper lines. The plates after exposure show brilliant colours due to the reduced silver. A slide of a photograph of the positive rays through a mixture of chloroform and chlorine was shown showing 21 parabolas clearly defined on the plate; these included parabolas due mercury (200), CCl_4 (152) $CHCl_3$, mercury with two charges, CH_2Cl_2, Cl_2, a line belong to a substance with a molecular weight 60 not identified CH_3Cl, CO_2, a line corresponding to molecular weight 40, chlorine, CO, chlorine with two charges, etc., lines corresponding to molecular weights 19 and 17, oxygen, nitrogen, carbon.

The choice of the developer is a matter of considerable importance; the plate just shown was developed with pyro. methol. A plate taken under similar conditions, but developed with hydroquinone, will be thrown on the screen, and you will see that the number of lines is much less and the definition very inferior to that developed with pyro. methol. As examples of the results obtained by Schumann plates I will show slides showing the positive ray spectra of HCl, $SnCl_4$, and one of hexam taken by my son, Mr. G. P. Thomson, and remarkable as showing on the negative side of the photograph CH, OH, C_2, and C_3; hitherto only atoms had been observed on the negative side, and not molecules, whether of elements or compounds. I will show later on another example of the occurrence of a compound molecule PH_3 on the negative side of the plate, indicating that the molecule of this compound can carry a charge of negative electricity.

I will now pass on to consider some applications of the method, and will take first the question of the origin of the line corresponding to the atomic weight 3; this must be either a new element or a polymer of hydrogen analogous to ozone. This line occurs when the gas which is the subject of the positive ray analysis is that which comes off when metals and other substances are exposed to bombardment by cathode rays, or that given off when metals are heated to a very high temperature; these gases are known to contain hydrogen.

In order to get the conditions as simple as possible I tried the bombardment of ice, made from water which had been boiled for a long time so as to expel any gases which might be dissolved in it. This water was placed in a discharge tube which was immersed in liquid air, the vessel pumped by a Gaede pump to take away all the gases which were not condensed by the great cold, and the block of ice bombarded for some hours by cathode rays. The rays bored a large hole in the ice, as you will see when I break this bulb which contains ice which has been bombarded. The gas driven off from the ice by the bombardment contains comparatively large quantities of X_3, the source of the line in question. The bombardment of frozen ammonia NH_3 is also a source of X_3, and it would seem that it must be derived in some way from the hydrogen in these compounds. The connection of X_3 with hydrogen is also indicated by the fact that X_3 is found in hydrogen through which an electrodeless discharge had been passed for several hours.

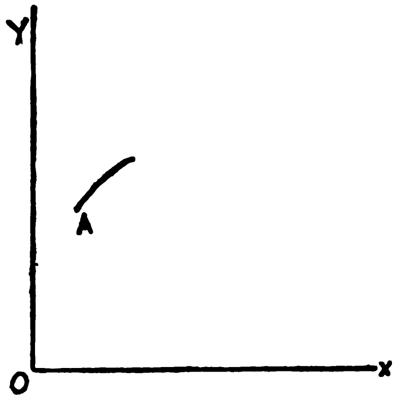


Fig. 1.

I have also obtained X_3 from gas which had not been subjected to electric currents by greatly heating crystals of phosphorium iodide, PH_4I; the gas obtained in this way showed X_3 very distinctly. The photographs obtained with this gas were also interesting, because they showed on the *negative* side a parabola corresponding to the molecular weight 34, PH_3; the existence on the negative side of molecules either of elements or compounds is, as I said before, very exceptional.

There is further evidence which, though indirect, seems to me of very considerable strength. The horizontal distance of the head of the parabola from the verticle line OY, passing through the undeflected spot O, is inversely proportional to the kinetic energy of the particle hitting the plate at A. This kinetic energy is due to the work done by the electric field in the discharge tube on the particle on its way to the cathode, and this work will depend on the electric charge and not on the weight of the particle; thus different particles if they had the same charge would have the same horizontal displacement, and we find as a matter of fact that the heads of the parabolas corresponding to the different elements, in the great majority of cases, are in the same vertical line. Now let us suppose that the gas we subject to analysis contains some H_3; this in its course to the cathode will get the normal amount of kinetic energy. If after passing through the cathode it broke up into H, and H_2 and the H_2 retained the charge, the H_2 would appear on the H_2 parabola; but the kinetic energy of this particle would not be the normal energy, because when the H_3 broke up one-third of its energy would go to H, leaving only two-thirds for the H_2. As the horizontal displacement is inversely proportional to the energy, the displacement of these particles will be 1·5 times the normal displacement; and I have here a photograph which shows the X_3 line accompanied by a bright spot on the parabola corresponding to H_2 at 1·5 times the normal horizontal distance.

I will now consider the question of the production of helium by the bombardment of metals and salts to which I drew your attention at a Friday Evening Discourse a year ago. I have made many experiments on this subject since last year, and have found that when helium is produced as the result of bombardment the supply eventually fails when the bombardment is prolonged for a long time. We must remember that the positive ray analysis can detect the helium in one-fifth of a cubic centimetre of air, although there are only about four parts of helium to 100,000 of air. If, therefore, a trace of air were left in the discharge tube, and the nitrogen and oxygen absorbed by the charcoal cooled with liquid air, the helium would be left as a residual gas, and would show itself on the photographs. Thus, when metals were bombarded by cathode rays the helium might come from air condensed on their surfaces and liberated by the bombardment. The case of the bombardment of salts was more difficult, for salts which gave off helium on bombardment would often continue to do so after they had been dissolved in water, and reprecipitated by evaporating the solution and recrystallising or precipitating the residue. The helium in this case can hardly be an impurity in the salt, and comes, I think, from air imprisoned in the fissures in the crystals or adhering to the surface. This view is supported by the fact that the results of these experiments are very irregular; sometimes more helium was found after redissolving the salt than before, and sometimes bombarding one specimen of a salt several hours daily for a fortnight did not yield more helium than another specimen of the same salt gave after a bombardment lasting for a few hours. On the other hand, when X_3 is obtained by bombarding hydrogen-containing substances the supply does not cease after long bombardment.

I will now pass on to another application of the positive rays, the investigation of intermediate and transient products in chemical reactions. The whole process by which any particle leaves a record on the photographic plate does not take more than a small fraction of a millionth of a second, so that it is very suitable for the detection of compounds which have a very transient existence. For example, the question has been raised whether, when hydrogen and oxygen combine to form water, the first stage in the process is the formation of the compound H_2O_2. This is not a very favourable case for the method, since the molecular weight of O_2 is so near that of H_2O_2 that there might be a danger of mistaking the parabolas due to these substances. I have, therefore, tried in preference the oxidation of alcohols, esters and nitrous oxide, but in none of these cases have I been able to detect the existence of intermediate products.

I have here a photograph of the positive ray spectrum of $NO + O_2$; it shows the known oxides of nitrogen, but I cannot find any trace of NO_3. It is probable, I think, that the first stages of chemical combination may be condensation round nuclei, such as traces of water vapour or ammonia, the whole forming a system too heavy to take part in the electrical discharge, and therefore not being registered on the plate; and that by the time the molecules get free from the nucleus the chemical reaction has been completed. The investigation of this chemical embryology, as it may be called, has only just been begun, and it would be premature to draw any positive conclusions at this stage of the inquiry. Another point on which we may expect to get information from the positive rays is the mechanism of ionisation. When a gas is ionised by cathode rays, does the process involve the splitting of the molecules into atoms, or is it confined to the removal of an electron from one of the atoms in a molecule leaving the connection between the two atoms in the molecule unbroken? Another question which arises is, what is the difference between the process of ionisation by cathode rays and that of positive rays? In the positive ray photographs we find positively charged atoms and positively charged molecules of most of the elements, and the alterations in the proportion of atoms to molecules may throw some light on the nature of ionisation. To test whether the cathode rays could split the molecules up into atoms, I tried the following experiment:—I had in the discharge tube a subsidiary Wehnelt cathode from which a large number of electrons were ejected when it was raised to a red heat, while none were ejected when it was cold, and I took positive ray photographs (1) when the Wehnelt cathode was hot, and (2) when it was cold. If the cathode rays did not split the molecules up into atoms, the effect of heating the Wehnelt cathode would be to increase the intensity of the lines due to the molecules compared with that of the lines due to the atoms.

I could not discover from the photographs any evidence of an increase in the relative intensity of the molecular lines. I hope that the examples I have given this evening may show that the positive ray method can be applied to a great variety of problems in physics and chemistry, and that its development may assist the progress of these sciences.

[J. J. T.]

WEEKLY EVENING MEETING

Friday, January 29, 1915.

EDWARD POLLOCK, F.R.C.S., Vice-President,
in the Chair.

DUGALD CLERK, D.Sc. F.R.S. M.Inst.C.E. M.R.I.

Gaseous Explosions.

THE investigation of gaseous explosions is of interest to Chemists, Physicists, and Engineers. The Chemist studies the laws of combination and dissociation; the Physicist deals with modes and rates of inflammation, variation of specific heat, maximum temperatures attained, and laws of radiation and cooling; while the Engineer considers both chemical and physical effects as bearing on the practical operation and thermo-dynamics of the internal combustion engine. He also interests himself in the analogous phenomena of inflammable dust explosions as found in coal-mine and flour-mill accidents.

The matters of interest are obviously numerous and complicated; and it is accordingly necessary to limit their consideration to a few points. The points selected will be dealt with as they bear more particularly on the engineering problems of the internal combustion engine.

In 1907 the British Association, at their Leicester Meeting, appointed a Committee of Investigation. This Committee has been at work ever since, and much light has been thrown by the experiments of its members upon the facts connected with gaseous explosions as occurring in closed vessels and within engine cylinders having moving pistons. It is now proposed to describe some of the work of the Committee, dealing first with the phenomena of rising temperature, and second with that of cooling after explosion.

When a mixture of coal gas and air is ignited within a strong closed vessel, it is found that the pressure rises rapidly, attains a maximum, and falls relatively slowly. To this rapid pressure rise is due the term gaseous explosion.

Such explosions are obtained with mixtures of all inflammable gases or vapours with air or oxygen, and very similar phenomena occur with like mixtures of inflammable dust.

The rise and fall of pressure is studied by means of sensitive indicator devices, which produce tracings on a rotating drum.

Fig. 1 gives a series of such indicator diagrams by Clerk from gaseous explosions obtained with mixtures of air and coal gas, where $\frac{\text{air}}{\text{gas}}$ varies from 4 to 12.

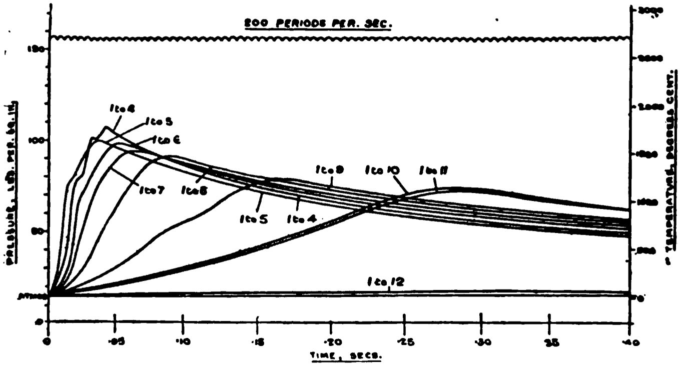


FIG. 1.—EXPLOSION IN A CLOSED VESSEL. London Gas, 1900 (Clerk).

Analysis of Coal Gas.

Hydrogen . . .	45·2	Carbonic oxide .	10·3
Marsh gas . . .	29·9	Oxygen . .	0·5
Unsaturated hydrocarbons	5·1	Nitrogen . .	10·0
			100·0

The following is a Table of the results :—

EXPLOSION IN A CLOSED VESSEL (Clerk 1900).

Mixtures of Air with London Coal Gas.

Temp. before explosion . . . . 16° C.
Pressure before explosion . . . . 14·8 lbs. per sq. in.

Mixture		Max. Pressure above atmosphere in lbs. per sq. in.	Temp. of Explosion from observed pressure	Time of Explosion
Gas	Air			
1 vol.	12 vols.	4	—	—
1 vol.	11 vols.	58	1150° C.	0·290 sec.
1 vol.	10 vols.	60	1155° C.	0·305 sec.
1 vol.	9 vols.	65	1270° C.	0·155 sec.
1 vol.	8 vols.	77	1475° C.	0·087 sec
1 vol.	7 vols.	80	1565° C.	0·067 sec.
1 vol.	6 vols.	85	1660° C.	0·055 sec.
1 vol.	5 vols.	87	1710° C.	0·042 sec.
1 vol.	4 vols.	93	1830° C.	0·045 sec.

Broadly the rate of pressure rise becomes slower as the air proportion increases. The time elapsing between the beginning of pressure rise and the attainment to maximum pressure is termed the time of explosion; and with a mixture of four of air to one of gas it is 0·045 second, and with eleven of air to one of gas 0·29 second. These experiments were made in an explosion vessel 7 inches diameter and 7 inches long. The pressure rise is caused by the formation of flame at the ignition point—in this case an electric spark—and the spread of this flame throughout the vessel increases the temperature of the gaseous contents and thus increases the pressure, as in a closed vessel the volume remains constant. The rate of pressure rise is thus, broadly, the measure of the rate of the travel of the flame through the mass; and if it be assumed that maximum pressure is attained when the whole vessel is filled with flame, then it is possible to determine the flame velocity by these measurements.

In this case the electric ignition points were placed in the centre of the cover at one end of the cylinder, and so a travel of 7 inches is necessary to practically fill the whole vessel with flame. Calculated in this manner the flame velocity for the weak mixture is 2 feet per second, and for the strong mixture 13 feet per second. Other experiments at the Massachusetts Institute of Technology, Boston, and also at the Royal College of Science, London, give very similar results. Ordinary internal combustion engines of the stationary type, when adjusted for the highest economy, usually draw into the cylinder a mixture of 9 volumes air to 1 volume coal gas, and the flame velocity for this mixture, as tested in the closed vessel, is found to be about 4 feet per second and the time of explosion 0·155 second. If a large explosion vessel be used and ignition be started at one end of the cylinder, it is obvious that the flame must travel a longer distance in order to fill the vessel, and consequently the time of explosion will be increased. By experimenting with vessels of different dimensions this has been found to be true. For example, Prof. Hopkinson, of Cambridge, has made experiments with a large vessel 23·4 inches diameter and 28·5 inches in length, having a capacity of 6·2 cubic feet, which show for a mixture of air 9 and gas 1 a flame velocity of 5 feet per second and a time of explosion of 0·26 second. The time of explosion is thus 0·26 second for the large vessel and 0·155 second for the small. In the large vessel experiments, ignition was started at the centre of the vessel. These experiments were made with an initial pressure atmospheric, but tests with similar mixtures by Messrs. Bairstow and Alexander at 34½ lbs. per square inch above atmosphere show no material change in velocity. Prof. Petavel, too, has made tests at the very high initial pressures of over 1000 lbs. per square inch, rising to between 9000 and 10,000 lbs. per square inch after explosion, and he found the rate of inflammation to be about 10 feet per second for a mixture of 6 gas and 1 air. Within wide limits it is evident from numerous experiments that the velocity of the spread of flame is independent of the initial density.

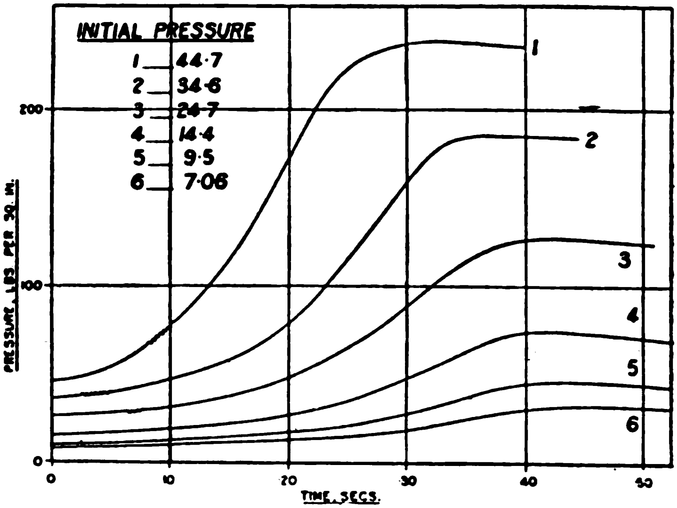


FIG. 2.—EXPLOSION CURVES OF GAS AND AIR MIXTURE containing 9·5 per cent of coal gas at varying initial pressures between 7 and 45 lbs. per sq. in. absolute. (Bairstow and Alexander).

EXPLOSION IN A CLOSED VESSEL. (Bairstow and Alexander).

Initial pressure varied from 7·06 lbs. to 44·7 lbs. per sq. in. absolute.
Initial temperature . . atmospheric.
Mixture containing 9·5 per cent gas and 90·5 per cent air.

Mixture		Initial pressure. lbs. per sq. in. abs.	Initial temp. °C.	Max. pressure observed, lbs. per sq. in. abs.	Time to reach max. pressure in seconds	Max. temp. of explosion °C.
Air	Gas					
1	0·103	44·7	16·5	238	0·33	1270
1	0·105	34·6	18·6	185	0·35	1280
1	0·104	24·7	20·0	126	0·41	1220
1	0·107	14·4	21·0	74	0·44	1235
1	0·104	9·5	21·5	46	0·50	1150
1	0·107	7·06	22·0	33	0·50	1110

The time of explosion not only varies with the proportion of inflammable gas or vapour in the mixture and with the dimensions of the explosion vessel, but it is also affected by the initial temperature and the chemical composition of the gas or vapour. If density of a given gas and air mixture and the dimensions of the explosion vessel and arrangement of the igniting point remain constant, then increase in the initial temperature increases the rate of the spread of the flame. Any change in the chemical composition of the gas or vapour also changes the flame velocity. Increase in hydrogen proportion generally increases flame velocity; but it is also affected by the nature of the gases forming a diluent. Even when hydrogen proportion remains constant, the presence of carbonic acid gas as a diluent instead of nitrogen changes the rate of flame propagation quite materially. Before considering these matters in detail, however, it is desirable first to state the nature of the rising line. In all diagrams from closed vessel experiments the rate of rise of pressure is first slow; then occurs a rapid increase towards the maximum pressure, but the rate of increase diminishes rapidly as the maximum pressure is approached. This is evident from the examination of Figs. 1, 3 and 4.

Fig. 2 shows a number of rising curves taken by Messrs. Bairstow and Alexander from gas and air mixtures containing 9·5 per cent of coal gas, the initial pressures varying between 7 and 45 lbs. per square inch absolute.

Fig. 3 is a diagram taken by Prof. Petavel in a spherical bomb at an initial pressure of 77·3 atmospheres or 1136 lbs. per square inch and a maximum explosion pressure of 646 atmospheres or 9508 lbs. per square inch, air gas ratio six.

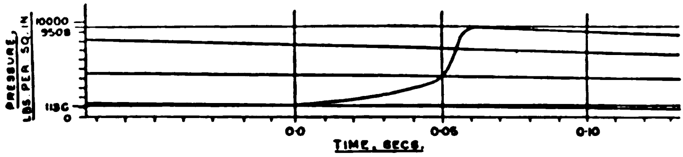


FIG. 3.—RISE OF PRESSURE DURING EXPLOSION. (Petavel.)

Spherical enclosure capacity, 551·9 c.c. Temperature of enclosure: before firing, 18° C.; after firing, 24° C. Initial pressure, 77·3 atmos. (1136 lbs. per sq. in). Maximum explosion pressure, 646 atmos. (9508 lb. per sq. in.). Ratio $\frac{\text{Air}}{\text{Gas}} = 6 \cdot 0$. Ratio $= \frac{\text{Maximum pressure}}{\text{Initial pressure}} = 8 \cdot 36$.

Fig. 5 shows the rising lines of mixtures of gas and air varying from 7·79 per cent gas to 14·98 per cent gas at an initial pressure of 35·5 lbs per square inch absolute, also taken by Messrs. Bairstow and Alexander.

Fig. 6 shows a diagram taken by Prof. Hopkinson in the large vessel referred to, the mixture being 9 of air to 1 of gas. On this diagram not only is the pressure rise shown, but temperature rise at different points is also indicated on the same diagram.

In all these cases the pressure rises slowly at first and proceeds more and more rapidly until maximum pressure is approached, when it proceeds slowly again.

In Petavel's experiments, Fig. 4, at 0·5 second after the start of the ignition, the rate of rise suddenly increases 9 times and then slacks off again before maximum pressure.

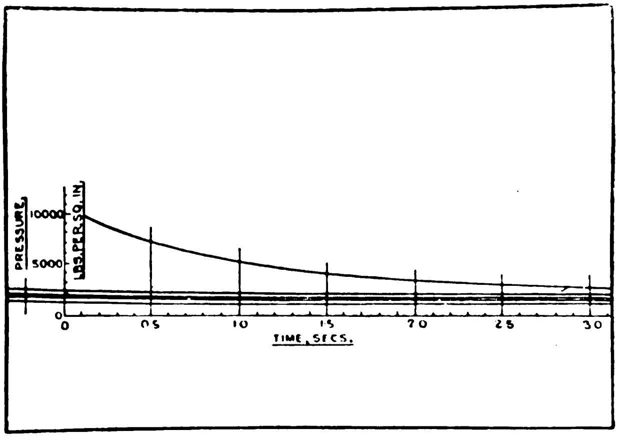


FIG. 4.—FALL OF PRESSURE AFTER EXPLOSION. (Petavel.)

Spherical enclosure capacity, 551·9 c.c. Temperature of enclosure: before firing, 21° C.; after firing, 27° C. Initial pressure, 74·38 atmos. (1094 lbs. per sq. in.). Maximum explosion pressure, 654 atmos. (9618 lbs. per sq. in.). Ratio $\frac{\text{Air}}{\text{Gas}} = 5·71$. Ratio $\frac{\text{Maximum pressure}}{\text{Initial pressure}}$ = 8·8.

In Messrs. Bairstow and Alexander's diagrams, Fig. 5, mixture 10·72 per cent gas, a rise of 50 lbs. of initial pressure takes about 0·6 second, while the next 100 lbs. is completed in 0·04 second at the rate of 50 lbs. 0·02, that is, three times the original rate. Hopkinson's Fig. 6 shows a rise in 0·1 second of only 3 lbs.; the next 0·1 second causes a rise of 33 lbs., a rate of 11 times.

At maximum pressure it will be observed that for a very short period no fall occurs, and when fall does take place it is at first slow. All gaseous explosions in closed vessels show similar characteristics.

Now, what is happening on this rising line? Reasoning from such diagrams it was long ago recognized that the rapid rise was caused by the spread of the flame, and it was believed that nothing could check the increasing rate but actual contact with the walls ; so it was considered that when the rate of rise of pressure became less towards maximum pressure, this was due to the fact that the vessel at this point was entirely filled with flame. The first part of the rising line occurred while the vessel contained part flame, part gas mixture as yet untouched by the flame. The flame spread relatively slowly at first, but as it spread it compressed the unburned gases into a smaller space before it and raised the temperature by compression. This raised the rate of flame travel and also diminished the distance through which the flame had to travel. As a consequence, after the start of the

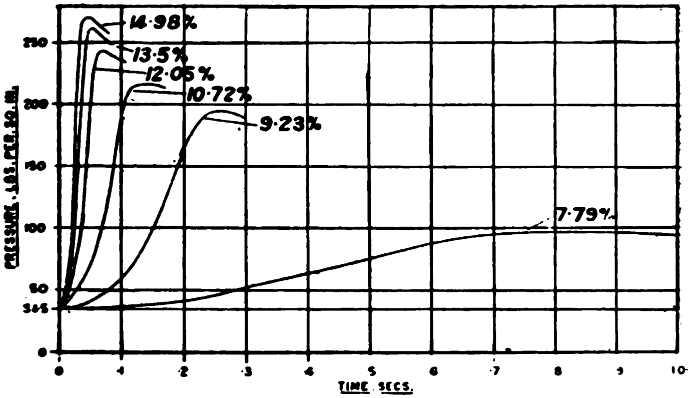


Fig. 5.—Explosion and Part Cooling Curves of Various Mixtures of Gas and Air at initial pressure of 34·5 lbs. per sq. in. absolute. (Bairstow and Alexander).

Explosion in a Closed Vessel. (Bairstow and Alexander).

Initial pressure . . . . 34·5 lbs. per sq. in.
Initial temperature . . . . 17° C.
Mixture containing from 7·79 per cent to 14·98 per cent gas.

Fraction of total volume occupied by gas, per cent	Maximum pressure observed, lbs. per sq. in. above atmos.	Time to reach maximum pressure, in seconds	Maximum temperature of explosion, ° C.
7·79	97	0·8	540
9·23	193	0·24	1350
10·72	210	0·13	1540
12·05	245	0·07	1790
13·5	263	0·05	1930
14·98	272	0·04	2010

flame its rate of movement continually increased until the whole of the vessel was filled. Had combustion been complete at this stage, then no further rise of pressure could have occurred. It was evident, however, that although every part of the vessel was filled with flame, the chemical combination had not ceased. The flame, at first very rapid in its rate of combination, reduced as combination proceeded, because of the increasing presence of carbon dioxide and steam-forming diluents to the inflammable gases still remaining to be combined with the excess of oxygen. It showed, indeed, that although a gaseous explosion is quick, yet chemical combination, even under such circumstances, slows down at high temperatures just as it does in other chemical combinations occuring at low temperatures. Although

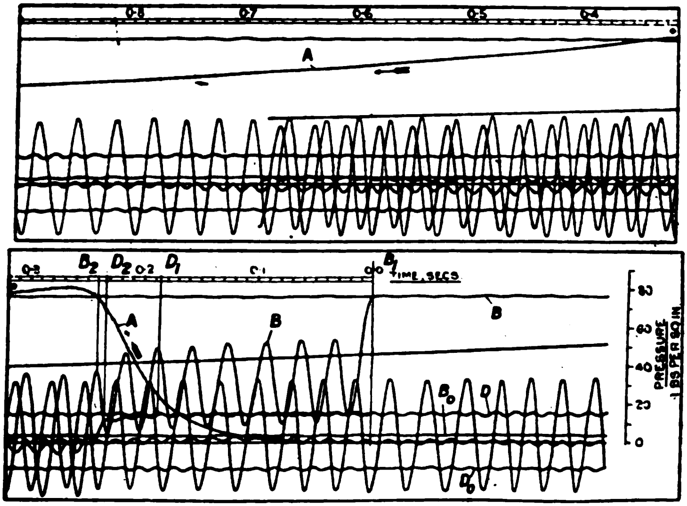


Fig. 6.—Explosion and Cooling Curve of Gas and Air Mixture (1 Gas and 9 Air) at Atmospheric Pressure, with temperatures reached by platinum resistance thermometers. (Hopkinson.)

Clerk thus inferred many years ago that at a certain point on the rising line the flame had completely filled the vessel, yet this was not definitely proved until Hopkinson made his experiments, which will now be described.

Hopkinson's large explosion vessel is shown at Fig. 7. It was, as has been stated, 23·4 inches diameter and 28·75 long. Electric ignition was effected by a spark passed at the point A in the centre of the vessel. Three platinum wire resistance thermometers were arranged respectively at B, C, and D—B close to the spark, C 10 centimetres within the wall of the vessel (4 inches), and D one centimetre within the wall of the end (0·4 inches). The pressure rise in the vessel and the temperature rise at the three thermometers were measured; the pressure by an optical indicator and the temperatures by the movement of galvanometer needles—all recorded photographically on a rotating drum. By this ingenious arrangement it is possible to find the position on the rising pressure line when the flame reaches the points D and C.

Referring to Fig. 6, the spark is passed at the zero point on the upper scale of the drum. The zero of pressure is shown on the right-hand side. The spark passes at zero, and for some considerable time, although the indication of the thermometer B shows a rapid increase of temperature by departing from the line B, yet the rise of pressure on the pressure line A is very small. It is obvious from the nature of the line that the rate is rapidly increasing for about 0·22 second from the passage of the spark. It then diminishes, and the rise proceeds more and more slowly until maximum pressure is attained at 0·26 second. At 0·22 second the vessel is very nearly filled with flame, and just over 0·23 second a very sudden rise of temperature occurs at the thermometer B. This indicates that the flame has reached that thermometer, and so the vessel is then completely filled. The pressure, however, rises considerably after that. Prof. Hopkinson considers that the vessel is entirely filled with flame

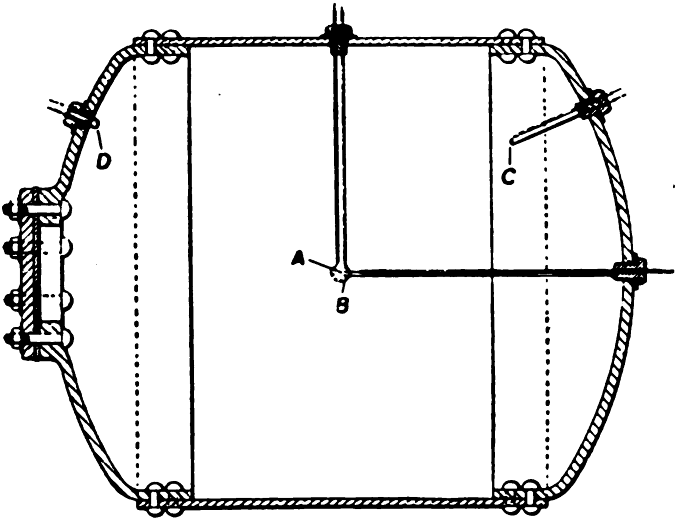


Fig. 7.—Hopkinson Explosion Vessel.

when the pressure rises 70 lbs. per square inch above atmosphere, and the maximum pressure is not attained until 82 lbs. per square inch is reached. Combustion is thus proceeding for about 0·03 second after the whole vessel is filled with the flame. This experiment of Prof. Hopkinson thus proves in a conclusive manner the correctness of the earlier deduction that part of the pressure rise in a closed vessel was accomplished after the flame had spread completely through the vessel. The same experiment proves that in this mixture the velocity of the spread of the flame between the points B and D is 5 feet per second.

In these experiments it was invariably found that the platinum-wire thermometer B at the centre of the vessel always melted before maximum pressure was attained. As the melting point of platinum is 1750° C., it follows that in the centre the temperature always rises above that point. The thermometers C and D were never fused with this particular mixture, so that the temperatures there were necessarily lower.

In considering a gaseous explosion it has been long recognized that the core of gas would be hotter than the outer layers which are in actual contact with cold metal walls, but Hopkinson's experiments showed a discrepancy in temperature which could not be accounted for in this way. As the results of his measurements he considers the distribution of temperature in his vessel at the moment of maximum pressure to be roughly as follows :—

	Degrees C.
Mean temperature (inferred from pressure) . . .	1600
Temperature at centre of vessel, thermometer B . .	1900
Temperature 10 cm. (4 in.) within wall, thermometer C	1700
Temperature 1 cm. (0·4 in.) from wall at end, thermometer D	1100 to 1300
Temperature 1 cm. (0·4 in.) within wall at side . .	850

The mean temperature throughout the whole vessel inferred from the change of pressure is thus 1600° C., and the estimated temperature at the centre of the vessel, thermometer B, 1900° C., while the temperature 4 inches within the wall at thermometer C is 1700° C., and the temperature 0·4 inches from the end of the wall thermometer D from 1100 to 1300°, while another thermometer, not shown in the diagram, 0·4 inches from the wall at the side showed a temperature of 850° C. This experiment thus shows very great variations of temperature in the flame filling the cylindrical vessel.

With regard to thermometers B, C, and D, it is obvious that temperature difference is not due to difference in heat loss. But little heat, for example, can have been lost at the central thermometer B, as no part of the flame there generated can have touched the walls much before maximum pressure. Although the flame has lasted longest at B, yet that thermometer being right in the centre of the ignited gases is protected against loss. The thermometer C also is 4 inches within the walls, and the flame reaches it much later than it touches B. D also, although near the walls, has only been in contact with the flame for a very short period before maximum, so that some other explanation than heat loss must be found to account for the great temperature differences. The temperature in the side thermometer, 850° C., is easily accounted for by heat loss, as the flame touches the side long before it reaches the ends, the diameter being 23·4 inches, while the length is 28·75 inches. Hopkinson accounts for the differences in the following manner:—When the flame strikes the thermometer B it is burning nearly at atmospheric pressure. The volume ignited at first does not disturb the pressure sufficiently to require consideration. As the flame progresses the pressure increases to 6½ atmospheres absolute. The flame temperature therefore of about 1200° C., which is attained at atmospheric pressure, is increased by the compression towards the centre as pressure rises, so that so far as the centre thermometer is concerned, the gas is treated as if a cylinder had been filled with flame at 1200° C. and then rapidly compressed to 6½ atmospheres pressure. This compression accounts for the rise of temperature above the temperature due to combustion; that is, flame has been produced first, then it has been adiabatically compressed from about 1200° C. to 1900° C. At the point C the same reasoning applies, but the range of compression is somewhat less, so the temperature only rises to 1700° C. At the point D the gases are compressed, as in a gas engine cylinder, to 6 atmospheres before ignition occurs; and, consequently, the subsequent rise of half an atmosphere is not sufficient to appreciably disturb the temperature. The temperature there is therefore between 1100 and 1300° C., simply the temperature attained by explosion without subsequent compression. At the side thermometer the flame reaches the walls when the pressure in the vessel is about 2 atmospheres. The temperature then instantly rises to 1200° C. or 1300° C., and at once begins to fall, so that by the time maximum temperature has been attained it has fallen to 850° C.

Half a second after maximum pressure, the distribution of temperature is very different; convection has now had time to take effect. The distribution of temperature is broadly as follows—

	Degrees C.
Mean temperature inferred from pressure . . .	1100
Mean temperature, exclusive of layer 1 cm. thick at walls, determined by long platinum wire from B to D	1160
Temperature at centre of vessel, thermometer B .	1100 to 1200

In this experiment the thermometer D has been dispensed with, and a thermometer consisting of a long platinum wire stretching from B to D has been used to get the mean temperature between these points. Another thermometer B has also been used at the centre.

The centre temperature is then found to vary from 1100° C. to 1200° C., while the mean temperature from B to D is 1160° C., and the mean temperature inferred from the pressure is 1100° C. Here the temperature differences are very much smaller than at the moment of maximum pressure. The mass of gas during cooling may be described as a hot core in which the temperature is approximately uniform, varied only accidentally by currents and surrounded by a thin layer wherein the temperature falls to the temperature of the walls. Prof. Hopkinson considers that if such a layer were ½ c.m. thick, and that if the fall of temperature were uniform, the mean temperature inferred from the pressure would fall short of that of the hot core by the observed amount, that is, 60° C.

These experiments are most interesting and valuable in aiding the understanding of what is happening on the rising line. Obviously this experiment proves very clearly that even in an absolutely non-conducting vessel temperature differences will be found at the moment of maximum pressure, apart altogether from any question of heat loss. In such a vessel the point of maximum temperature would always be the point where the flame originated, because at that point a flame at the full combustion temperature will be produced, and this combustion temperature will be raised by compression from the initial pressure to the maximum pressure of explosion. Now that it has been proved, it is quite obvious that this temperature rise by compression should occur; but no one thought of it or predicted it, and it remained for Hopkinson to discover it by these interesting experiments.

Explosion within Engine Cylinders having Moving Pistons.—It has been long known that the times of explosion in closed vessel experiments were much longer than the times found within engine cylinders having moving pistons. Indeed, it is evident that an engine, say, of about 24 inches diameter cylinder, approximating to the capacity of Hopkinson's explosion vessel, could not possibly be operated in any commercial way if the time of explosion had been as long as 0·26 second, the time found by Hopkinson for an economical mixture. Allowing 40 degrees of the circle of revolution of a crank for the ignition period in an engine, one revolution would require to take 2·34 seconds, so that the engine could not revolve at even as low a rate as 30 revolutions per minute and still ignite the gases in time to produce effective results. Engineers long recognized that the rate of the travel of flame usually found in closed vessel experiments was much lower than that in engine cylinder tests. In a paper published in the Proceedings of Civil Engineers in 1882, Clerk made the following statement:—

"The author has found it possible to ignite a whole mass in any given time between the limits of one-tenth and one-hundredth part of a second, by so arranging the plan of ignition that a small volume of gaseous mixture is first ignited, expanding and projecting a flame through a passage into the mass of inflammable mixture, and thus adding to the rate of ignition the mechanical disturbance produced by the entering flame. He has succeeded by this means in producing maximum pressure in one-hundredth part of a second in a space containing 200 cubic inches. This rate of ignition is too rapid, and would not give the engine time to take up the slack in bearings, connecting-rods, etc. But by firing a mixture with varying amounts of mechanical disturbance almost any time of ignition can be obtained between one-hundredth and one-tenth of a second. It does not matter whether the mixture used is rich or weak in gas; the rich mixture can be fired slowly and the weak one rapidly, just as may be required. The rate of ignition of the strongest possible mixture is so slow that the time of attaining complete inflammation depends on the amount of mechanical disturbance permitted."

From this it will be seen that even in 1882 it was known that mechanical disturbance was necessary in order to get the maximum pressure in the time required for running an engine effectively. It was then known that the rate of ignition was slow even in a strong mixture, and that the time of attaining complete inflammation depended upon the amount of mechanical disturbance permitted. It was known that the rate of spread of flame was much greater in engines than in closed vessels. In experiments made by the author at that time he found that mechanical disturbance could be caused by means of a projection of flame through a passage into the mass of the mixture; and it was supposed that the higher rate of flame propagation in ordinary internal combustion engines was due partly to this flame disturbance and partly to the higher temperature of compression before ignition.

The examination of indicator diagrams from gas engines and petrol engines has shown that the rate of flame propagation varies in different engines, from 35 to 100 feet per second. Experiments made with a horizontal gas engine 22 inches diameter and 34 inches stroke running at 160 revolutions per minute with Mond gas, suction gas, and town gas, all at 160 revolutions per minute, show that with Mond gas the explosion period required 45 degrees of the circle, with suction gas 40 degrees, and with town gas 25 degrees. The respective times of explosion were $\frac{1}{21}$th, $\frac{1}{24}$th, $\frac{1}{38}$th of a second, and the respective flame velocity as well 47, 54, and 85·5 feet per second. In these experiments the maximum flame temperatures were nearly the same, but of the different gases used Mond gas had the slowest ignition and town gas the fastest. In this particular engine the inlet velocity of the charge through the valve throat was 160 feet per second. In another horizontal gas engine of 9 inches diameter by 17 inches stroke running at 200 revolutions per minute the charge velocity was about 100 feet per second. In this engine 25 degrees crank angle was required for explosion. The time of explosion was $\frac{1}{45}$th second, and the rate of flame propagation was 37½ feet per second.

Fig. 8 shows in section the 22 by 34 inches engine, and Fig. 9 shows the igniting arrangements. Ignition was by magneto and low tension make and break. Two igniters were used, one at each side of the charge admission port. The distance from the igniter centres to the piston was 2·25 feet.

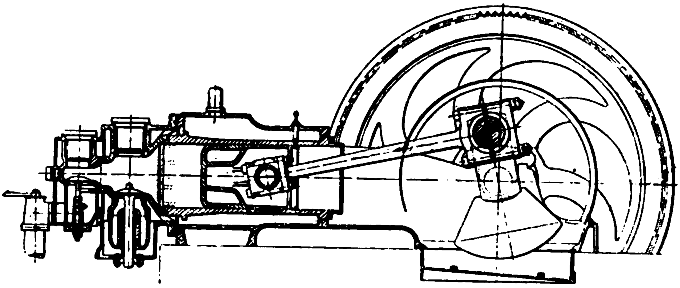


Fig. 8.

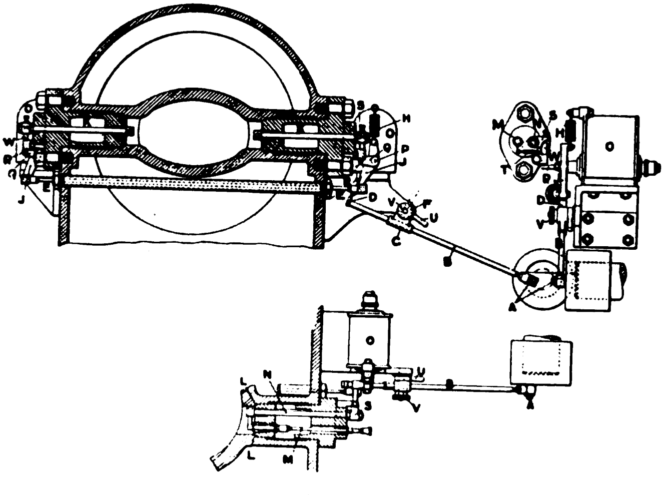


Fig. 9.

Figs. 10, 11, and 12 show diagrams from which the period of explosion has been determined.

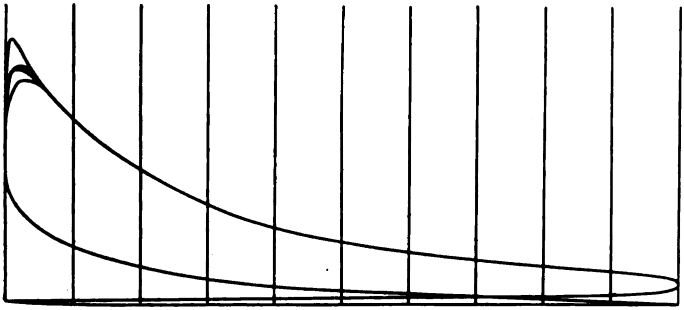


FIG. 10.—22″ DIA. × 34″ STROKE CYLINDER.

Full load. 160 revs. Mond gas. Period of explosion about 45° circle.

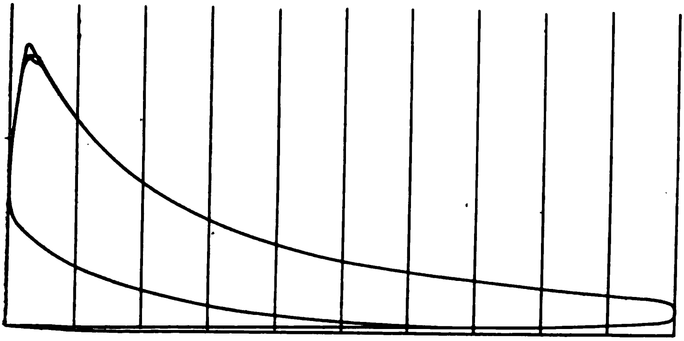


FIG. 11.—22″ DIA. × 34″ STROKE CYLINDER.

Full load. 160 revs. Suction gas. Period of explosion 40° circle.

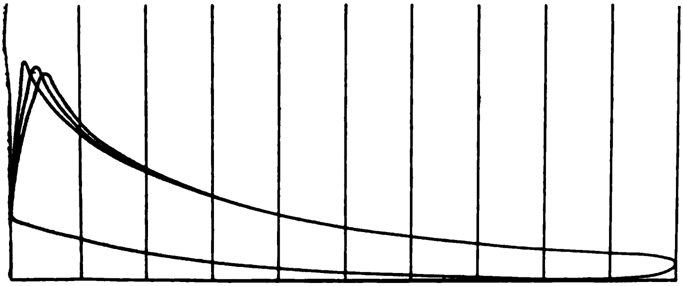


FIG. 12.—22″ DIA. × 34″ STROKE CYLINDER.

Full load. 160 revs. Town gas. Period of explosion about 25° circle.

Fig. 13 shows in section the 9 by 17 inches engine, and Fig. 14 is a right-angle card from which the time of explosion has been determined. In this engine the flame travels 10 inches from the igniting plug to the piston.

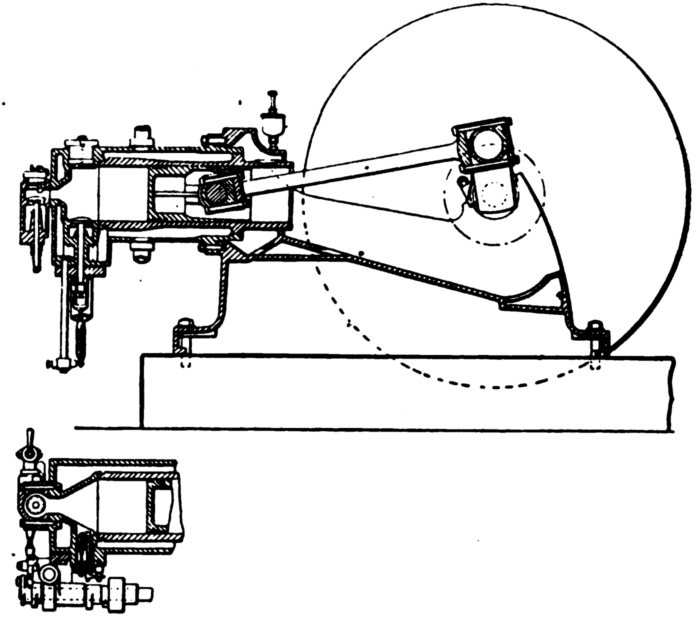


FIG. 13. - SECTIONS OF "R" ENGINE. (National Gas Engine Co., Ltd.) Institution of Civil Engineers' Committee Tests.

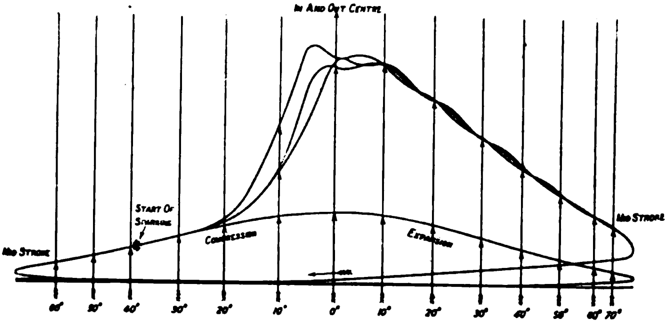
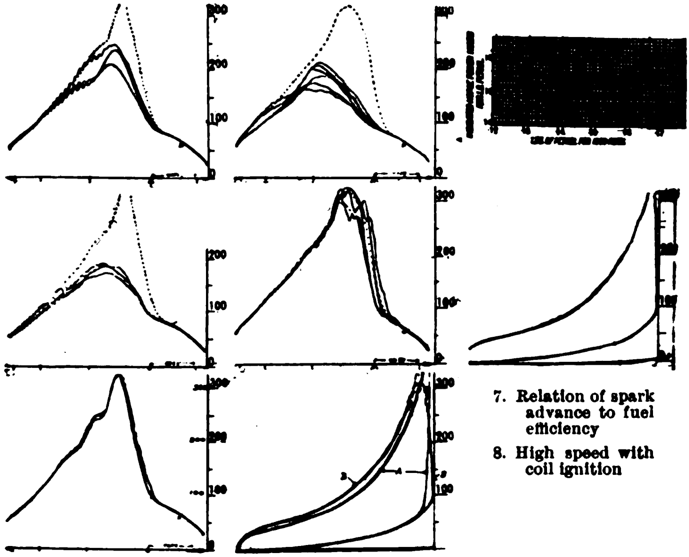


FIG. 14.—DIAGRAM 90° OUT OF PHASE.

9″ dia. × 17″ stroke engine. Period of explosion, 30″ crank axle.

Fig.15 is a right-angle card taken by Dr. Watson from a petrol engine running at a high velocity. In that card the explosion period is $\frac{1}{270}$ of a second; and, assuming the flame to travel 4 inches before striking the walls, the velocity of the flame is 90 feet per second. It was well understood that the flame velocity was higher in engines than in closed vessels, and mechanical disturbance was assumed, but the mechanical disturbance was believed to be that due to the admission of the flame and the projection into the mass as a jet. Early in 1912, simultaneous experiments were made by Clerk and Hopkinson by different methods to determine the nature of the mechanical disturbance causing this increase of rate. Clerk's experiments were made with the engine shown in Fig. 13. This figure shows vertical and horizontal sections. The engine was fitted with trip gear, which enabled a charge to be taken in, compressed and expanded for any desired number of cycles without ignition and ignited under compression, when the desired number of alternate compressions and expansions had been completed. The object of the experiments was to compare the rate of inflammation ignition immediately after



7. Relation of spark advance to fuel efficiency
8. High speed with coil ignition

4a. Spark over inlet valve
4b. Spark over exhaust valve
4c. Both sparks together

5a. Spark over inlet valve
5b. Both sparks together
6. Fixed and variable ignition point

Fig. 15.

admission and compression under full load with the engine hot. in the ordinary manner of working a four-cycle engine, with the rate of inflammation after the charge had been compressed and expanded several times before ignition. The idea was that turbulence was caused within the cylinder by admission of the charge at a high velocity through the inlet valve, and that this turbulence persisted during the compression stroke, so that in the ordinary running of the engine the flame was projected into a moving mass of gas, and so the flame velocity was much more rapid than if the charge had been still. The alternate compression and expansion before ignition was intended to allow the turbulence to subside, so that ignition started in a still mass of gaseous mixture and not in a moving one. The experiments prove that inflammation at the end of the third compression stroke took 2·5 times the period of the ordinary Otto cycle explosion. From Fig. 13 it will be seen that the engine was fitted with two electrical igniters, one in the cover above the charge inlet valve and the other in the side of the combustion space near the piston. Tests were made using these igniters alternately. To try the experiment the engine was first run at full load till the water jacket, piston, and other parts had become hot. Diagrams were then taken under load of the ordinary Otto cycle explosions. Then the

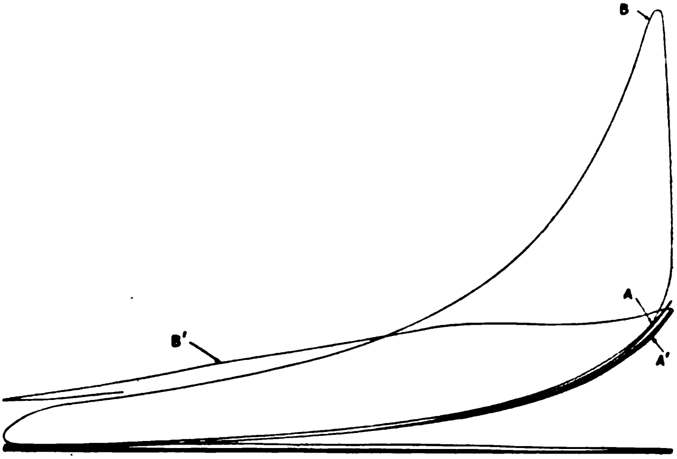


FIG. 16.—TURBULENCE.

Mixture of one volume of coal gas and 9·3 volume of air and other gases ignited by back igniter of engine. Line A to B indicates ignition of charge, fired upon first compression stroke, which takes 0·037 second. Line A' to B' indicates ignition of charge fired upon third compression stroke, which takes 0·092 second.

trip gear was operated, keeping the engine running at the same speed and taking exactly the same charge, and the ignition was arranged to take place on the third compression instead of the first. Two complete additional revolutions thus intervened between the state of turbulence during ordinary ignition and the state at the experimental ignition. It was then found that the diagrams showed the time of rise of temperature from the beginning of the rise leaving the compression line to the maximum temperature was from 2·2 to 2·8 times that of the ordinary temperature rise in the usual running of the engine. The appearance of the indicator cards show in a marked manner the great difference in ignition rate caused by allowing the turbulence of ignition to die down.

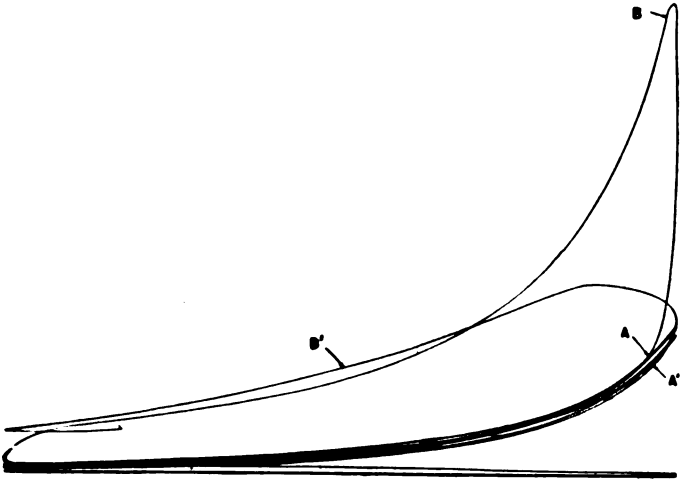


Fig. 17.—Turbulence.

Mixture of one volume of coal gas and 9·3 volume of air and other gases ignited by side igniter of engine. Line A to B indicates ignition of charge fired upon first compression stroke, which takes 0·033 second. Line A' to B' indicates ignition of charge fired upon third compression stroke, which takes 0·078 second.

Figs. 16 and 17 show diagrams taken in this manner, and it is very clear that in this engine, were it not for the increased rate of flame propagation due to charge turbulence, economical running at the usual speed of 180 to 200 revolutions would be impossible. In these turbulence experiments the time taken is not what has been previously called the time of explosion as determined by maximum pressure within the engine, but the time to attain maximum temperature, which in the case of the trapped charges, as will be seen, occurs well on in the stroke. In ordinary engine running it is better to take the time of maximum pressure as the measure of the time of the explosion, but where differences are so great as these experiments show, it is more accurate to compare maximum temperature points.

These experiments clearly showed that in the ordinary engine cylinder the velocity of the flame through the mass depends very largely on the residual turbulence due in the first instance to the charge velocity at entering. From this it would be expected that the higher the charge velocity became, the higher became the rate of ignition. Experiments show that this is the case. Where the charge velocity through the valve is high, the rate of ignition is rapid; and in petrol engines, where the charge velocity sometimes rises as high as 150 feet per second, the flame velocity approaches 90 feet per second. In the large gas engine Fig. 8, as has been stated, the charge velocity is about 160 feet per second, but the rate of rotation of the engine is only 160 revolutions per minute. The residual turbulence is accordingly less than it would be in the case of a petrol engine, where the rate of revolution varies from 1000 to 2000 per minute.

Prof. Hopkinson also made tests of turbulence by a series of closed vessel experiments. The vessel used was cylindrical, and about one foot diameter and one foot long. The cylindrical portion was completely lined with a helix of copper strip, the turns being insulated from one another. Means were provided for recording the resistance of this strip during the progress of an explosion whence the rise of temperature and therefore the rate of flow of heat into the copper could be determined. A small fan like a screw-propeller was fitted at the centre of the vessel, the axis of the fan being along the axis of the cylinder. An optical indicator was fitted to the vessel. The spindle of the fan passed through a gland in one of the end covers, and it could be driven at any speed up to 5000 revolutions per minute, thus setting the gas in the vessel into motion. The general character of the motion would be an eddy, the gas passing along the axis of the cylinder on the inside and then outside in contact with the cylinder walls. Cambridge coal gas and air were used at atmospheric temperature before firing. The mixture was fired by a spark near the centre of the vessel. The cylinder was filled with a given mixture of gas and air, and the fan was run in all cases for a few minutes, in order to ensure the mixture of the gas and the air. Diagrams were taken with two mixtures, one mixture containing 15 per cent of gas, the other 10 per cent.

Figs. 18 and 19 show the diagrams obtained with these two mixtures.

Diagrams Figs. 20 and 21 are deduced from Figs. 18 and 19, and give the relation between the rate of heat loss and the temperature of the gas. Comparing the pressure curve A in Fig. 19 with the curves B, C, D, it will be noticed that the effect of the agitation of the gas at the moment of firing is to greatly reduce the time of explosion.

This is in agreement with Clerk's experiments just referred to on the gas engine. A similar reduction, but in a less marked proportion, occurs with a 15 per cent mixture, as shown in Fig. 18. It will be noticed the acceleration, when the fan is running at 2300 revolutions per minute, is almost as great as when it is running at twice that speed; and from this it may be inferred that a very moderate amount of movement may have a very great effect upon the time of explosion. The copper temperature curves in Figs. 18 and 19 show

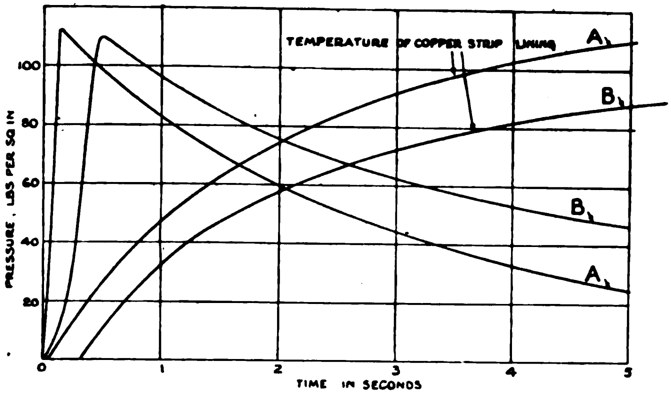


Fig. 18.—15 Per Cent Mixtures.

A. Fan at 3600 R.P.M. B. Fan at rest.

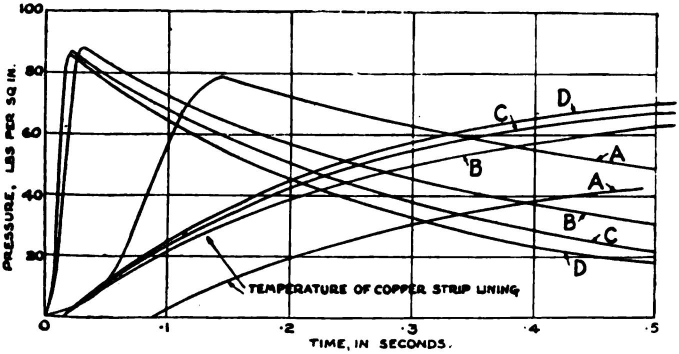


Fig. 19.—10 Per Cent Mixtures.

A. Fan at rest
B. Fan at 2300 R.P.M.
C. Fan at 3600 R.P.M.
D. Fan at 4500 R.P.M.

that at a given time after the moment of ignition the heat loss is much greater when the fan is running than when the gas is at rest; but this is mainly due to the acceleration of the spread of the flame. The specific effect on the rate of heat flow produced by the

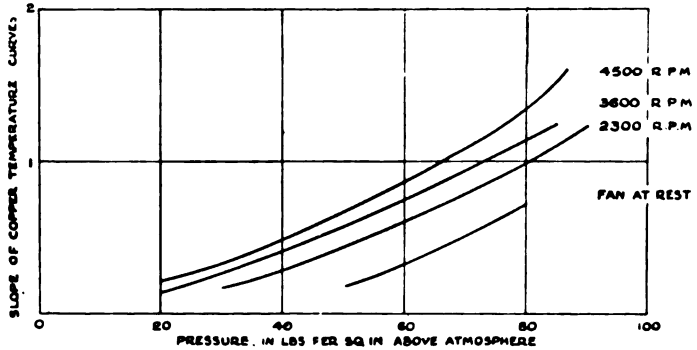


FIG. 20.—EFFECT OF SPEED OF FAN ON RATE OF HEAT-LOSS. 10 PER CENT MIXTURES.

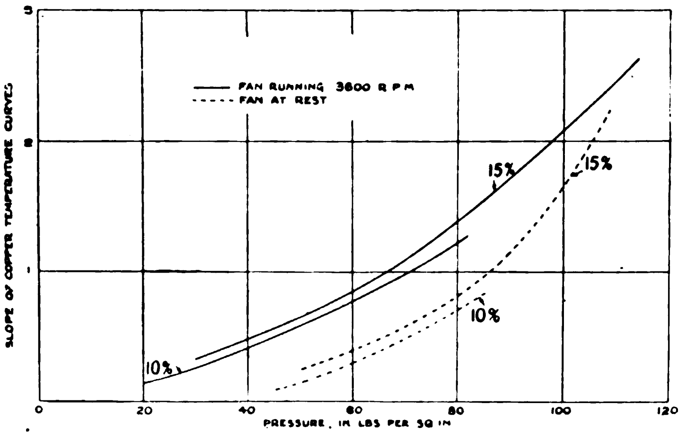


FIG. 21.—RATE OF HEAT LOSS.

turbulence is shown in Fig. 20, whence it appears that with a fan speed of 4500 revolutions per minute this rate is nearly doubled at a pressure of 75 lb. per square inch. The added heat flow consequent on turbulence seems to be roughly proportional to the speed of the fan. It will be seen from Fig. 21 that at a given pressure, and with a given speed of the fan, the rate of heat flow from the products of combustion of 10 per cent and of 15 per cent mixtures is nearly the same. The weaker mixture seems to lose heat a little less rapidly, but this may possibly be due to a difference in the relative amounts of heat lost from the copper to the backing in these two cases. No correction has been made in the figures for this loss. It will be noticed that at the higher gas temperatures corresponding to 100 lb. per square inch and over, the heat loss with 15 per cent mixtures is less affected by turbulence than at lower temperatures. This Prof. Hopkinson ascribes to the influence of radiation, which he states is probably unaffected by the motion of the gas, and is a very important cause of loss when the gas is hot. Experiments were made to determine the relation between the velocity of the air and the speed of the fan. The velocity of the air was estimated by the cooling effect on a platinum wire heated by an electric current, and they show that at a fan speed of 2500 revolutions per minute the velocity of air near the walls is probably greater than that obtaining in a gas engine at the end of the suction period. From this it is inferred that the turbulence in the engine is insufficient to produce any great effect on heat loss. It will be noted that in Fig. 18 the time of explosion with the fan running at 3600 revolutions per minute is about one-fourth that obtaining with the fan at rest. The time of explosion with the fan in motion is about 0·0125 second, and with the fan at rest 0·05 second. In Fig. 18 at 2300 revolutions the time is about 0·03 second; while at rest it is 0·14 second.

That residual turbulence exists at the end of the ordinary compression stroke in a gas-engine cylinder has also been proved by Hopkinson by similar experiments on an engine driven by belt, in which experiments the rate of heat flow was determined at the end of the first compression and also at the end of subsequent compressions. For this purpose a platinum wire was mounted in the combustion space of an engine of 7-inch diameter cylinder and 15-inch stroke. The wire was heated by an electric current. Within moderate limits of temperature the heat loss from such a wire is proportional to the temperature difference between it and the surrounding gas. The ratio between heat loss and temperature difference is a measure of the effective conductivity of the gas, and depends on the temperature density and state of motion. If the first two factors are the same, then the effective conductivity depends only upon the state of motion, and may be taken as the measure of its amount. The engine was motored round so as to compress and expand charges of air, the gas supply being cut off, and comparative measurements of effective conductivity at the top of compression were made, first, with the engine valves working in the ordinary way, and, second, with the valves closed, so that the same charge was continually compressed and expanded, and there was therefore no turbulence resulting from suction. It was found that at 240 revolutions per minute conductivity was more than 60 per cent greater in the first case than in the second; while at 60 revolutions per minute the difference was only 20 per cent.

In these comparative experiments the temperature and density of the gas were the same, and the difference could only be due to the motion. From measurements of heat loss from a similar wire in a closed vessel, Hopkinson infers that the motion of the gas with a fan speed of 2500 revolutions per minute is probably considerably greater than that obtaining in the gas engine. At this speed the heat flow at a temperature of 1600° C. or over is increased by an amount of the order of 25 per cent.

These experiments of Clerk and Hopkinson clearly prove the importance of what may be called Suction Turbulence in determining the time of explosion and also the rate of cooling in all internal combustion engines.

Determination of Temperatures.—So far the question of the rising line and the phenomena of ignition only have been dealt with. Before proceeding further it is necessary to say something on the question of temperature determinations. Upon an accurate knowledge of temperature and temperature distribution in closed vessel experiments and in gas engine cylinders depend all conclusions as to properties of the gases, heat flow to the walls, and efficiencies possible under the varying conditions. Accordingly, much consideration has been given by the Members of the Committee to all questions relating to the determination of temperatures under varying conditions. In closed vessel experiments the initial pressure and temperature of the gas mixture is known with great accuracy. Accordingly, if it be assumed that no volume change occurs due to chemical action, the temperature determination is easy, because absolute pressures are then proportional to absolute temperatures. It is only necessary then to determine the ratio between absolute pressure before explosion and absolute pressure at explosion to get the mean temperature of the mass of gas at the highest pressure point. Unfortunately, chemical change introduces complications, and in ordinary coal gas and air experiments which have been dealt with here the result of combustion is to produce a contraction of volume. In ordinary gas engine mixtures the contraction is not great—it amounts to about 4 per cent—but it must be allowed for, and it is difficult to determine with accuracy, because so far no absolute knowledge exists as to the precise stage which the combustion has reached at the maximum temperature of an ordinary gaseous explosion. Allowing for all uncertainties, however, it may usually be taken in closed vessel experiments that combustion has certainly consumed about 80 per cent of the gases present at the point of maximum pressure. Applying this proportion to the total contraction possible, a close approximation to the maximum temperature can be obtained. In this way the combining gases are themselves used to form a gas thermometer. In closed vessel experiments many conclusions can be deduced from temperatures obtained in this way. A direct thermometric determination. however, has long been desired, and experiments such as Hopkinson's already described endeavoured to make the direct determination by platinum resistance thermometers. Such thermometers, however, are subject to the melting point of platinum, and cannot be used with accuracy when near that point, and of course cannot be used at all for temperatures above the melting point. Prof. Coker has accordingly made experiments with thermo-couples formed from alloys of platinum with rhodium and iridium. These thermo-couples were made of alloy of from $\frac{5}{10000}$ to $\frac{8}{10000}$ of an inch thick, and they have withstood the temperature of explosion near the walls of a gas engine cylinder for hours and even days, provided the engine was not overloaded and the temperature raised seriously above 2000° C. Prof. Coker determined gas engine flame temperatures by such couples, the actual temperature measurement being made by observing the change in the electromotive force produced in the couple by a change in temperature. Devices were arranged whereby small changes in electromotive force could be accurately measured.

Fig. 22 shows Prof. Coker's arrangement for determining temperature in a gas engine cylinder in a diagrammatic way. The battery B and resistances R_1 and R_2 are arranged in circuit, so that the fall of potential between the extreme points of a bridge wire BW can be adjusted to one milli volt. This is tested by the electromotive force of a cadmium cell C, which can be opposed to the battery electromotive force by means of the upper key K_1, and allowance for the known temperature variation of the electromotive force of the standard cell was being made by an adjustable contact-maker D. The thermo-couple H has one lead connected to the lower lead K_2, and the other to a set of resistances S in the main circuit, each of which gives a difference of potential of one milli volt when the adjustments are correct. During an observation the battery electromotive force opposes that of the couple, and the readings of the bridge wire and step resistances taken together measure the electromotive force of the couple when the galvanometer G shows a balance. By this method, Prof. Coker determined the temperature of explosion in a National Gas Engine with a weak mixture as 1690° C., but he considered his instrument to have a lag, and he believed the actual temperature attained by the flame at half an inch from the walls of the cylinder to be above 1850° C. and less than 1950° C. In later experiments he came to the conclusion that with a similar engine using weak mixture and giving an explosion at every cycle at about ¾ load the temperature of 1800° C. was reached at a distance of half an inch from the walls. He also determined the exhaust temperature by a similar instrument; the maximum temperature in the exhaust pipe after leaving the engine was about 500° C., and the temperature drop of the gases from the moment of opening the exhaust valve to the temperature of the gases in the exhaust pipe was 150° C. Professors Callendar and Dalby also determined the temperature of explosion in a gas engine cylinder, but they did not attempt to measure the maximum temperature by any thermo-metric arrangement. In closed vessel experiments, as has been pointed out, it is easy to determine accurately the initial temperature before explosion, but in actual gas engine experiments, what is called the suction temperature presents serious difficulties of measurement. If the suction temperature—that is, the temperature of the charge just after it has filled the cylinder—be known, and also the pressure of the charge, then, considering the gas as its own thermometer, all the other temperature points can be calculated. Professors Callendar and Dalby proceeded by determining the temperature of the charge in the inlet port of the engine cylinder. To do this they applied the platinum resistance thermometer to the determination of both suction and compression temperatures by the use of an ingenious attachment to the charge inlet valve of a four-cycle engine.

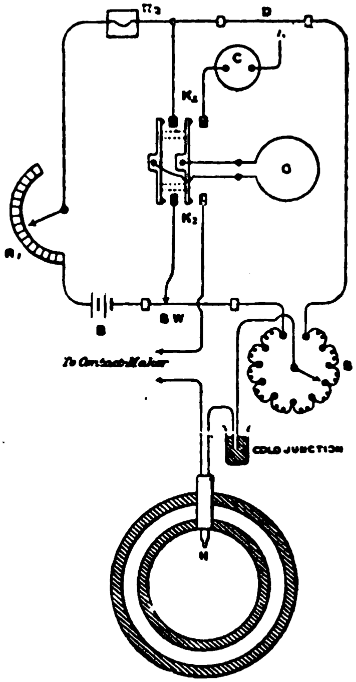


Fig. 22.—Thermo-Electric Bridge.

Fig. 23 shows a section of this valve, from which it will be seen that a small additional valve T is provided at the centre of the main valve A, which carries within it the platinum loop of a Callendar platinum resistance thermometer. The loop is shown at P, and the wires leading to it are carried through the valve stem and pass at B to the necessary recording instruments. In this ingenious device the valve carrying the resistance thermometer is reciprocated by a separate cam gear, and it is opened at the particular period of the stroke during which temperature is to be taken. It is closed before explosion takes place, so that the temperature of suction or compression can be measured at any period of the cycle, and yet the platinum loop is moved away before the cylinder is filled with flame. By this device it is possible to obtain accurate measurements of temperatures during any part of the compression stroke, and indeed during most parts of the expansion and exhaust stroke. The thermometer valve cam operates quite independently of the charge inlet cam, so that the ordinary action of the inlet valve A is not interfered with. In order that the wire may assume change of temperature rapidly, it is necessary that it should be thin; and, accordingly, to read temperature for any particular point, other arrangements are required which measure resistance for a minute period of time. To accomplish this, Prof. Dalby has invented an ingenious percussion contact-maker, which makes contact just at the instant when temperature requires to be measured. The galvanometer measurement is given by a succession of small currents, one for each succeeding stroke. In this arrangement it is necessary to keep up consecutive ignitions when experimenting as to temperature. Callendar and Dalby found the suction temperature to vary with the conditions of running from about 95° C. on light load trials to about 125° C. at maximum load, with the

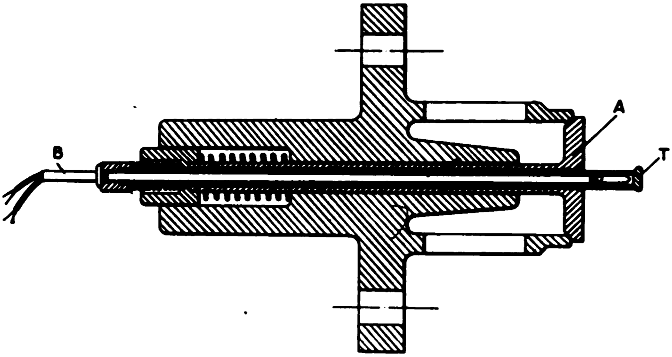


FIG. 23.

external air temperature nearly 20° C., and the water jacket temperature 27° C. In the engine experimented on the compression was somewhat low, and at full load the mixture used was very rich, the proportion of gas to air in the entering charge being one to 5·8. Other measurements were made at a point 80 degrees crank angle from the beginning of the compression stroke, and with a compression pressure of 18·5 lb. per inch absolute at the point of measurement, the temperature here was found to be 111° C. With a richer mixture and the engine running at a lower speed, the corresponding temperature was 130° C. Calculating from these suction and compression temperatures values, Callendar and Dalby find with a rich mixture flame temperature at the point C as 2500° C., mixture drawn into the engine being one of gas to 5·8 of air. With one of gas to 7·1 of air the temperature at C is nearly 2250° C. The exhaust temperature—that is, the temperatures at the point D—in the case of the rich mixture was 870° C., and in the poorer mixture 800° C. Professors Callendar and Dalby are continuing their experiments with improved apparatus.

Clerk has also made temperature determinations in gas engine cylinders for experimental purposes by another method of determination of suction temperature. It is only applicable, however, to isolated indicator diagrams from single explosions and expansions taken under experimental conditions, where the engine experimented on is driven by means of an electric motor. The arrangement adopted is shown diagrammatically at Fig. 24. In these experiments an engine of 9 inches cylinder and 17 inches stroke was used. It is indicated at A. The inlet valve is connected to a reservoir B, and the engine is driven by means of an electric motor C. The reservoir B is filled with the particular mixture of gas and air which is to be experimented upon, and a thermometer D is placed in the pipe communicating between the cylinder A and the reservoir B. The engine valves are controlled by trip gear, so that the inlet may be held up until wanted. After the reservoir B and the cylinder A and the connecting pipes have been filled with the mixture to be experimented upon, the engine is driven by the electric motor at a specified speed, and the gases are alternately taken into the cylinder from B and discharged into B from the cylinder. The charge thus enters and leaves the cylinder many times under pressures only slightly above atmosphere, and so repeatedly passes over the thermometer D. That thermometer, which is one of the ordinary mercury type, attains a certain temperature, which remains fairly constant after a certain number of revolutions. The temperature is a mean between that of the gases passing from B to A and passing from A to B. This temperature then is taken as the temperature of a charge within the cylinder. The cylinder is kept by water jacket at about atmospheric temperature. In this way Clerk believes the temperature of the charge to be determined to within 1° C. Trip gear is applied at the proper time, and the charge is compressed, ignited and dealt with as required experimentally. Knowing the charge temperature in this way, it is possible to calculate with considerable accuracy the temperatures at the four points. In this method there is no complication due to hot gases being left in the cylinder; the temperature of the whole charge which is used for experimenting is measured by repeatedly passing over the thermometer. Temperatures have been calculated of the points A, B, C, and D in this manner, and Fig. 25 shows a diagram taken by an optical indicator with all the necessary data. It will be seen that the temperatures are 20° C., 214° C., 1380° C., and 651° C. This method gives good results, but is only available where single charges are studied. It, however, allows of

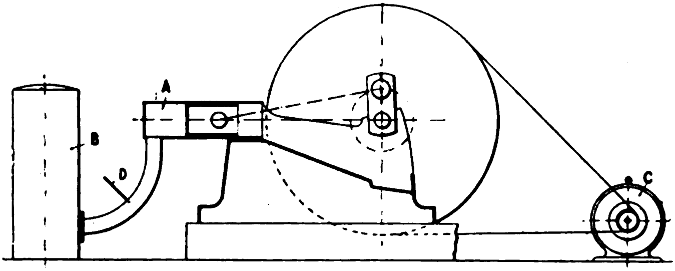


Fig. 24.

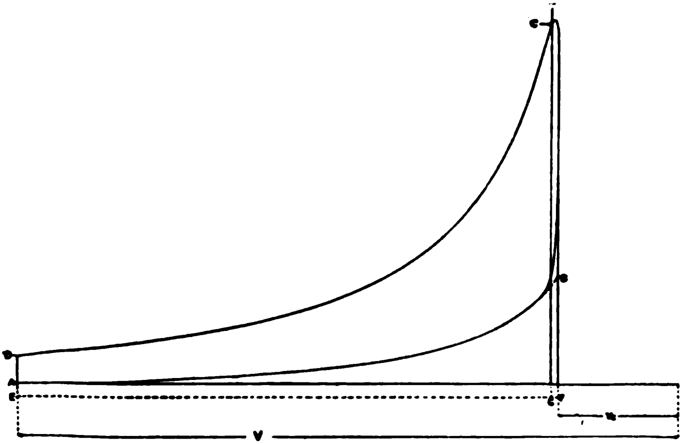


Fig. 25.

great accuracy in the determination both of mixture used and temperatures attained.

By these experiments more accurate knowledge is now available of the actual temperatures and temperature distribution in gaseous explosions, both in closed vessels and in engines. The knowledge of temperatures has enabled specific heat determinations to be made, and also heat flow determinations, by various methods which need not be here described. Knowledge as to heat flow and the conditions of its variation is accumulating, and further determinations of specific heat of different gases are necessary. Work of this nature is in progress by Members of the Committee, and engines are being designed to be fitted up in the Engineering Laboratory of the Imperial College of Science and Technology for the purpose of continuing the experiments in a more systematic and sustained manner. The work is very laborious, and has necessitated the design and invention of many new instruments. Optical indicators have been much improved by the work of the Committee; but details of this work must be reserved for a future occasion.

The work of Callendar and Hopkinson, with David, his pupil, on radiation from gaseous explosions is, however, of very high interest, and this address may be concluded with a short description of their results.

Prof. Callendar's experiments were made to determine the proportion of radiation from a Bunsen flame to the total heat of combustion. The particular Bunsen used was of the Mêker type. It had a nickel grid of three centimeters in diameter, and consumed gas at the rate of 0·185 cubic feet per minute. In burning in the air the total heat of combustion was disposed of in two ways—first, the heat radiating from the flame, which passed at the velocity of light through the surrounding atmosphere; and, second, the heat ultimately lost from the flame by mixture of the hot gases with the atmospheric air of the room. The ratio of air to gas by volume was determined, and experiments were made in which that ratio varied from five to one, and finally air was cut off entirely. The air-gas ratio was that of the mixture entering the burner, and made no allowance for air drawn in by the flame, so that the actual mixture radiating would contain more air than the figures given. It was found that as the value diminished the radiation increased down to $\frac{\text{air}}{\text{gas}} = 2 \cdot 5$. At 1·5 the radiation again diminished, remained practically constant at one, and when air was entirely cut off and nothing but a luminous flame was burning it again rose. The following figures show this very clearly:—

Total radiation p.c.	10·5	12·3	14·0	15·0	14·1	14·6	17·0
Ratio $\frac{\text{air}}{\text{gas}}$ by volume	5	4	3	2·5	1·5	1	0

With the proportion of five air to one of gas by volume, together with the addition of air from the atmosphere, the flame had oxygen more than sufficient for complete combustion, and, consequently, it was short. The radiation here was 10·5 per cent of the total heat of combustion. The radiation attained a maximum at 2·5 air to one volume gas of about 16 per cent. This was exceeded, however, as the total radiation without air was 17 per cent. For a wide range of mixture the intrinsic radiance or the radiation emitted per unit area of surface varied but little. Intrinsic radiance varies with the temperature of the flame and the depth of it. It is usually assumed to vary with temperature according to the fourth power of the temperature, but Professor Callendar states that this has been criticized, and the law of increase may be less. Callendar made further experiments with six similar Mêker burners, and found a great increase in radiation due to the depth of flame from which it proceeded. The radiation observed, for instance, with one flame deep was 68 units per square centimetre. With six flames deep it rose to 282. The flames were quite similar and almost homogeneous. Callendar says that "it will be observed that the flame is surprisingly transparent to its own radiation. It is very commonly assumed that because a flame absorbs precisely those radiations which it emits, and absorbs them in the same proportion as it emits them, the flame would therefore be practically opaque to its own radiation, so that the radiation proceeding from the interior of a mass of homogeneous flame might be neglected, and the total radiation assumed proportional to the surface. The best observations show that this is very far from being the case, owing to the relatively wide separation of the radiating and absorbing molecules." From these experiments Callendar finds that an infinite thickness of radiating gas under the same conditions as the Mêker flame described would give an intrinsic radiation of 473 units per square centimetre; and the experiments show that with a thickness of 100 centimetres (about 40 inches) of such flame the intrinsic radiance has reached within less than half per cent of this limit. One hundred centimetres at atmospheric pressure corresponds to about five centimetres, or two inches, thick at twenty atmospheres; so that at the pressures attained in gas engine cylinders, the limit of radiation is nearly reached in a sphere or combustion space of two inches diameter. From this it follows, as Callendar clearly shows, that in combustion chambers of all practical dimensions, using the same temperature and nature of gases, the radiation loss per unit surface is equal. In reasoning from these experiments Professor Callendar comes to interesting conclusions as to the distribution of heat loss in time in the combustion chambers of engines.

Hopkinson prepared a cylindrical cast-iron explosion vessel of 30 cm. diameter by 30 cm. long (11·8 in. by 11·8 in.), the whole of the interior surface of which was plated with silver; and he compared the result of exploding a mixture containing 15 per cent of Cambridge coal gas, first with the vessel polished as highly as possible, and, second, with the surface blackened over. Precautions were taken to ensure that the mixture in the comparative experiments should be of identical composition. Fig. 26 shows superposed the pressure records obtained by an optical indicator with one such comparison. From this it will be seen that there is an increased maximum pressure of between 3 and 4 lb. per square inch in the record with the polished surface; and further, the blackened surface cools the hot gaseous contents more quickly. The gas temperature is higher with the polished surface by about 70° C., near the temperature of 2000° C., which, at the high volumetric heat at that temperature, amounts to about 5 per cent of the whole heat value of the inflammable gas.

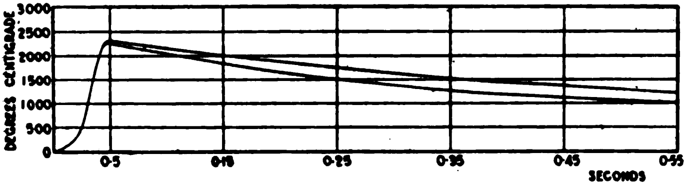


FIG. 26.

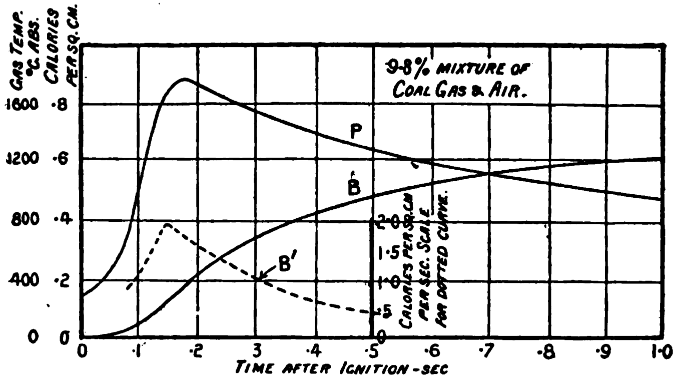


FIG. 27.

Other experiments were made with a platinum strip bolometer, which measured the radiation passing from the explosion vessel through a fluorite window, which clearly proved that in 0·05 second taken to attain maximum temperature and pressure, this mixture lost nearly five per cent of the total heat in radiation.

Fig. 27 shows the results of experiments by David with the apparatus indicated. The explosion and cooling line is clearly shown, and the temperature of the gaseous contents is marked on a scale at the side. The lower rising line indicates the bolometer temperature, and the scale for that line shows the heat passed by radiation into the bolometer at each point. The lower dotted line shows the change of rate of the passage of heat through the fluorite window; and it is interesting to observe that the maximum rate of heat flow by radiation occurs before the maximum temperature of the burning gases is attained. Chemists and physicists have long held that radiation from combining gases proceeds more rapidly at the moment of combination than at other times of equal temperature.

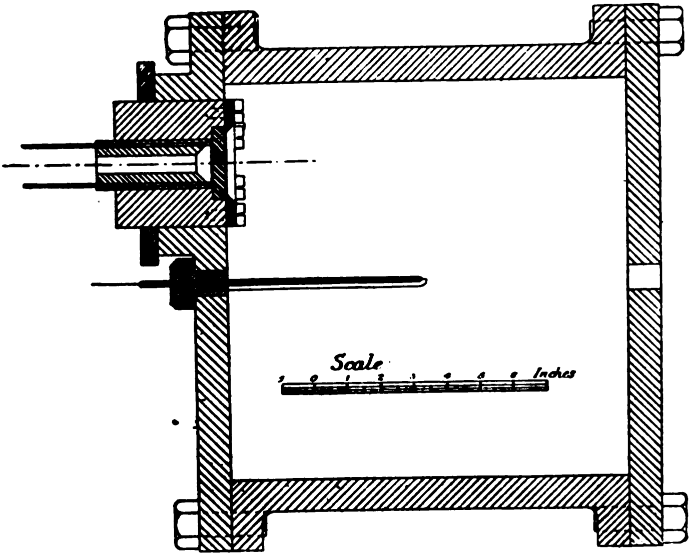


Fig. 28.

This is clearly shown by the diagram. It is evident that, given equal conditions of temperature, radiation is greatest when chemical combination is proceeding. The diagram shows, too, that the rate of radiation rapidly falls with the fall of temperature, but radiation at the end of half a second is still quite considerable.

Fig. 28 shows in section the explosion chamber fitted with David's bolometer and also with a platinum resistance thermometer, to determine the mean temperature from the centre to the ends by Hopkinson's method already described.

Fig. 29 shows an explosion diagram and a bolometer temperature diagram taken with the apparatus.

From these determinations it is possible to roughly estimate the heat loss on the explosion line in an ordinary gas engine using the most economical mixture and a maximum temperature of about 1800° C. With time of explosion $\frac{1}{30}$th second under ordinary compression, a 3 per cent loss may be expected.

It was long ago observed by Hirn, Bunsen, and others, that the rise of temperature in gaseous explosions could not be calculated from the then assumed specific heat of the constituent gases and the known calorific value of the inflammable gas. The deficit of temperature was found to be about 50 per cent, and many attempts were made to explain this deficit, Hirn advocating the theory of heat loss on the rising line and Bunsen supporting the idea of a limit to temperature due to dissociation. Later, the French observers Mallard and Le Chatelier maintained that at least part of the deficit could be accounted for on the assumption of increase of specific heat of the gases. Investigations of the Members of the Committee have dealt not only with the points which have been here discussed, but with all these questions—heat loss on the rising line, specific heat of the constituent gases, heat loss on the falling line, and dissociation of the combining gases. Specific heat work has been in progress by Clerk, by Callendar and his pupil Swann, and much of this work has not yet been published. Dissociation has been discussed by Dr. J. A. Harker, Professor Smithells and Dr. Bone, and both internal energy and dissociation have been discussed by Hopkinson. Ignition temperatures of gases have been dealt with by Professor Harold Dixon, and Dr. Watson has studied the nature of the exhaust gases from the petrol engine. Many experiments, too, have been made on the law of cooling and heating of gases under compression in cylinders by Hopkinson, Dalby, Callendar, and Clerk. As a result of this work the conclusion has been arrived at that so far as explosions in internal combustion engines are concerned dissociation has but little to do with the limit reached. This limit is partly due to increased specific heat at high temperatures, to heat loss to the walls, and to radiation from the explosion. Varying specific heat and increasing radiation account for most of the deficit. Allowing for all these things,

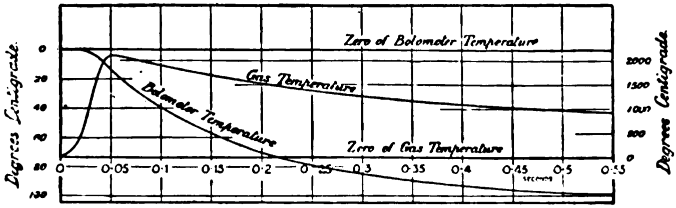


FIG. 29.

however, it appears to be now established that combustion is not quite complete even at maximum temperature, and Watson's experiments on the spectrum of an explosion flame appear to support this view. All these matters are still under examination, and it is hoped that in the near future a much more complete knowledge may be gained than at present exists. Much is known in a qualitative way, and some quantitative knowledge has been attained, but much still remains to be done in this way of quantitative determinations of matters at first apparently so simple as specific heat.

[D. C.]

GENERAL MONTHLY MEETING,

Monday, February 1, 1915.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

The following statement was read by the Secretary :—

CAXTON HOUSE, WESTMINSTER,
LONDON, S.W.
1st February, 1915.

It has come to my knowledge that a short declaration on my part, with regard to my position as a British Subject, and with regard to my sentiments about the war, would be acceptable to the Managers of the Royal Institution. I, therefore, venture to lay before you the following statement :—

I was born in Hanover, both my parents having been British Subjects until Hanover was separated from the United Kingdom in accordance with the provisions of the Salic Law.

In the year 1866, the Kingdom of Hanover was annexed by Prussia, and thereby universal military service was introduced.

In July 1867, I first came to England, and I went back to Germany in 1868 to absolve my military duties, but I was not accepted on account of my shortsightedness. I had, however, to present myself again when the war against France broke out in July 1870, and I joined a regiment of the line for one year.

In October 1871, I returned to London, where I have resided ever since.

As it became probable that I should spend my life in this country, I resolved to follow the example of the late Sir William Siemens by becoming a naturalized British Subject.

To carry out this plan, I obtained my formal release from German Citizenship in March 1878, and my certificate of naturalization (No. 2671) was filed at the Home Office on the 16th of August, 1878.

Thus I have been a subject of the Kingdom of Hanover for 19 years, of

Prussia for 12 years, most of which time I spent out of Germany, and of the United Kingdom for over 36 years, nearly all of which I spent in London.

As a consequence my sympathies in this war are entirely on the side of the Allies, and I cannot express strongly enough my abhorrence of the way in which Germany has disregarded international treaties and conventions, as to the conduct of hostilities, and more particularly of the raids made on unfortified watering places in this country.

I should like to add that in return for the advantages I have enjoyed as a British Subject, I have done my best to serve this country whenever an opportunity for doing so presented itself to me.

(Signed) ALEX. SIEMENS.

The Managers reported, That the Resident Professor had, after his Friday Evening Discourse on January 22, presented to the Institution, on behalf of Dr. H. D. Rolleston (a grand-nephew of Sir Humphry Davy) a Bust of Sir Humphry Davy modelled by Samuel Joseph in 1822. The Bust was bequeated to him by a grand-daughter of Dr. John Davy (brother of Sir Humphry) who was Inspector-General of Army Hospitals.

Sir James Crichton-Browne, the Treasurer, unveiled and received the Bust. On behalf of the Members the Treasurer took the opportunity of expressing their profound appreciation and gratitude to Dr. H. D. Rolleston for handing over so generously to their keeping the Bust of the great genius who first created the reputation of the Royal Institution as a world centre for Chemical and Electrical Research.

The Managers further reported, That Dr. H. D. Rolleston had presented Sir Humphry Davy's patent of Baronetcy, and some of his Medals and Books, which were also bequeathed to Dr. Rolleston by a grand-daughter of Dr. John Davy.

Resolved, That the Members desire to convey to Dr. H. D. Rolleston their Profound Thanks for these most valuable Gifts (intimately connected with the Royal Institution and of great historic and scientific interest) which he has so generously presented to the Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

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Kodaikanal Observatory Bulletin, No. 44. 4to. 1914.
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WEEKLY EVENING MEETING,

Friday, February 5, 1915.

DONALD HOOD, C.V.O. M.D. F.R.C.P., Vice-President, in the Chair.

PROFESSOR ARTHUR W. CROSSLEY, D.Sc. F.R.S. For.Sec.C.S.

Science and Industrial Problems.

"THE industrial development to which I have referred would never have been possible, if this domain had not been studied in a most thorough and scientific manner in the laboratories of our factories, for it must be remembered that pure scientific research work, carried out in the laboratory, is the soul of industrial prosperity."

These words are not my own, but were spoken as recently as last June by one of the most prominent industrial chemists of this or any other time ; prominent in the sense of possessing great scientific ability, and also in the more material sense of possessing abundance of the fruits of industrial success.

The coupling of the words science and industry in the title of a lecture has, in this country, frequently been made the occasion for drawing attention to the lack of appreciation, on the part of our chemical manufacturers, of the necessity for applying scientific method to industrial problems, but it is not my intention to touch this side of the subject this evening. I will content myself with saying that many of our manufacturers have shown themselves to be strangely deficient in their appreciation of science in the past, and there has been a lamentable and mutual distrust on the part of manufacturers and professorial chemists, for which state of affairs the latter and the institutions to which they belong cannot be regarded as entirely blameless. The research worker in a scientific institution in this country—more is the pity—works simply as an individual towards the realization of his own original ideas, which in general have little or nothing to do with trade problems. According to prevailing beliefs, it is altogether undignified for a professor to have anything to do with trade : he must not approach either it or that awful thing called a patent, otherwise both he and his institution will thereby become defiled. But to the professor falls the duty of training students, who are to go out into the world as technical chemists ; and how can he be expected to do this efficiently, when he is discouraged from having any association with the manufacturer and his wants ? What a different tale could be told of the present position of chemistry in this country if all the research departments of our chemical schools, each working in harmony with some industrial chemist, had devoted at least some part of their energies to the pure scientific research work, carried out in the laboratory, which is the soul of industrial prosperity!

This country possesses a small army of efficient research chemists, and if the manufacturers, with whom the initiative must rest, would but say the word, it seems quite certain that the capacities of these research workers would be willingly devoted to helping to overcome difficulties, which may be encountered at the present time, in attempting to perfect methods and processes in use, or in starting new enterprises.

To apply the discovery of some comparatively simple chemical fact, made with perhaps a gram or even a fraction of a gram of substance, to the founding of an industry, dealing with tons and multiples of tons of the same material, is frequently, if not always, impossible for any one man to accomplish. It requires the organized efforts of many men, from the ranks of physicists, chemists, and last (but by no means least) engineers. We have reason to know at the present time that the word organization has a powerful meaning, but nowhere is its meaning more powerful than when used in conjunction with the word research; it is organized scientific research work which is the soul of industrial prosperity.

This is the point which I wish to develop somewhat this evening, and I will endeavour to illustrate what is meant by scientific method, and how it is applied to the solution of industrial chemical problems. Not very long ago I had the honour of delivering in this room two lectures on the "Utilization of Atmospheric Nitrogen," or the methods by which nitric acid and ammonia may be made directly from the free nitrogen in the atmosphere. The chemistry of the process as regards nitric acid is simplicity itself, and may be represented by three simple equations:—

$$N_2 + O_2 = 2NO; \qquad 2NO + O_2 = 2NO_2;$$
$$2NO_2 + H_2O = HNO_2 + HNO_3$$

or, stated in words, nitrogen and oxygen are caused to combine forming first nitric oxide, and then nitrogen peroxide, which is absorbed by water, yielding a mixture of nitrous and nitric acids, and this mixture can be separated into its constituents.

It would be difficult to select a better example of what is meant by organized research than is to be found in the history of the development of these simple chemical reactions to the founding of a great industry. Though the chemical facts relating to the process have been known for years, it was Lord Rayleigh in 1897 who first demonstrated the possibility of thereby producing nitric acid in proportion to the electric energy expended; but, as has been the case with so many brilliant discoveries of English scientists, no practical use has been made of it in this country, and though its development into an industry has been partly due to the cheap water power obtainable in Norway, the determining factors have been the indomitable skill and perseverance of Norwegian physicists, chemists, and engineers.

Lord Rayleigh showed that electric sparks would cause the union of nitrogen and oxygen with the formation of nitric oxide; but it was the Norwegian physicist, Birkeland, in conjunction with his countryman the engineer Eyde, who, by the gradual development of laboratory experiments, were first able to demonstrate that furnaces could be built capable of dealing with from 700,000 to 800,000 litres of air per minute. As the air after passing through the furnaces contains little more than one per cent of nitric oxide, the problem of oxidizing this gas and absorbing the resulting nitrogen peroxide is a very difficult one; for the more dilute the gas, the more difficult the operation. But organized research proved irresistible, and now no less than 98 per cent of the nitrogen peroxide is absorbed in a series of five large towers by either water or sodium carbonate solution, giving rise to a mixture of nitrous and nitric acids. The separation of these two substances required the intervention of yet another type of experimenter, namely, the physical chemist, or one who especially studies the ground which cannot be allotted strictly to either physics or chemistry, but is common to both.

It was found that with increasing concentration nitrous acid becomes more and more unstable in aqueous solution, so that a further process takes place, resulting in the formation of more nitric acid and nitric oxide :—

$$3HNO_2 = 2NO + HNO_3 + H_2O$$

and, under certain conditions of temperature and amount of water present, the instability of nitrous acid becomes so great that only nitric acid is formed, the practical result being that in the first tower not only the strongest nitric acid, but an acid free from nitrous acid is produced.

The nitric oxide formed as in the above equation is then oxidized, and again absorbed, so that the amount of nitrous acid increases in each succeeding tower, until in the fifth tower, where sodium carbonate solution is used as the absorbent, nearly pure sodium nitrite is produced.

These are some of the main points which have been overcome by the joint and organized efforts of many men. There may be added the difficulties of a physical-geographical, and engineering nature, which had to be surmounted in order to bring under complete control a waterfall with a perpendicular drop of over 500 feet: to dam a mountain lake so as to transform it into a reservoir holding 840 million tons of water, capable of generating continuously in the valleys below about 300,000 electrical horse-power; and to build a power-station and factory in the wilds of Norway.

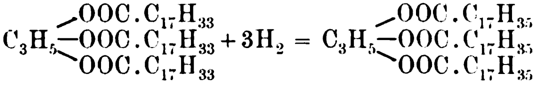
This evening I wish to direct your attention more particularly to another industry of still more recent birth, but which again proves the power of organized research. I refer to a process known as the "Hardening of Fats," which has been selected for a variety of reasons. It must appeal to the scientist and to the householder alike, though it may be for quite different reasons; it is a new industry; it is largely carried on in this country; it gave rise to a law-suit as to patent rights, on which account it does not interest us in the very least; but inasmuch as all those who profess law are not at the same time skilled chemists, it became necessary to have the process submitted to exhaustive chemical examination, and by considering the evidence handed into court, a clear insight can be gained into the scientific method applied by chemists in providing answers to the questions raised by the patent. This evidence, together with further information derivable from other sources, makes it possible to follow fairly clearly the lines of the research work which gave rise to the industry. The last thing which I am desirous of doing is to call in question any of the legal proceedings, or to appear to take sides with one or other "parties to the case." I merely wish to draw from this law-suit whatever evidence I can in illustration of the application of science to the solution of industrial problems.

Oils and fats are from the chemical standpoint of similar structure, being glycerides of certain acids or compounds of the alcohol glycerol with principally palmitic ($C_{16}H_{32}O_2$), stearic ($C_{18}H_{36}O_2$), and oleic ($C_{18}H_{34}O_2$) acids. The glycerides tripalmitin and tristearin are solids, whilst triolein is a liquid at ordinary temperatures. Palmitic and stearic acids are known as saturated acids, whereas oleic acid, which differs from stearic acid only in containing two atoms of hydrogen less, is called an unsaturated acid. When a glyceride contains a large proportion of tripalmitin or tristearin, it will be a solid at ordinary temperatures and is known as a fat, whereas if the proportion of triolein is large the resulting substance will be liquid at ordinary temperatures, and is called an oil.

The hardening of fats is therefore from the theoretical point of view an extremely simple process. It consists in adding to the glyceride of an unsaturated acid the necessary number of atoms of hydrogen to convert it into the glyceride of a saturated acid. It follows from what has just been said that an oil is thereby converted into a fat, and the process would be more correctly spoken of as the "hardening of oils," or the conversion of the unsaturated acids which they contain into saturated acids. For example, oleic acid is converted by the addition of two hydrogen atoms into the solid stearic acid:—

$$CH_3.(CH_2)_7.CH:CH.(CH_2)_7.COOH + H_2 = CH_3.(CH_2)_{10}.COOH,$$

and the liquid glyceride, triolein, is converted into the solid glyceride tristearin :—



$$C_3H_5\begin{cases}OOC.C_{17}H_{33}\\OOC.C_{17}H_{33}\\OOC.C_{17}H_{33}\end{cases}+3H_2 = C_3H_5\begin{cases}OOC.C_{17}H_{35}\\OOC.C_{17}H_{35}\\OOC.C_{17}H_{35}\end{cases}$$

Chemical saturation as such is not, however, of particular interest to the manufacturer, who looks rather to the point at which the addition of hydrogen will convert an oil into a workable solid fat.

Though by weight the amount of hydrogen required for this process is not great, by volume it assumes enormous proportions. One ton of oleic acid requires roughly 79,000 litres or 2800 cub. ft., and one ton of triolein requires 75,900 litres or 2680 cub. ft. of hydrogen.

The question of producing hydrogen in sufficient quantity for carrying out this process is an interesting one in itself, but it must suffice to say that the question has tested the combined skill of chemists, physicists, physical chemists, and engineers, with the result that not only is the fat-hardening industry established, but such things as airships filled with hydrogen are known, and the latest of all chemical industries, namely, the synthetic production of ammonia gas from nitrogen and hydrogen, has sprung into existence.

It is not easy to obtain any accurate idea of the amount of oils which are thus treated annually, but it may be mentioned that hydrogen is being produced by the Linde Company's method (Zeit. angew. Chem., 1913, xxvi. (3) 814) in sufficient amount to harden 100,000 tons of fat annually. There is, further, a factory in Norway capable of producing 4500 cub. metres of hydrogen by an electrolytic method, and of hardening 150 tons of whale oil every 24 hours (Offerdahl, see later).

The question naturally arises, Why has the hardening of fats assumed such large proportions ? The answer being that so much of the natural supply of hard fats is now employed in the making of margarine and other foodstuffs, that the amount of such fats available for soap-boiling has been considerably encroached upon, and hence it has become necessary to find other sources of hard fats for the latter purpose.

One of the principal oils now submitted to the process of hardening is whale oil, of which the world's supply in 1912 amounted to 1,200,000 barrels, more than half of which was obtained from Norway (Offerdahl, Ber. deutsch. Pharm. Ges., 1913, xxiii. 558). Ten or fifteen years ago only small amounts of whale oil were used in the tanning industry or for lubricating, its taste and smell prohibiting its employment for other purposes. More recently larger quantities have been worked up for the isolation of glycerol, but the oil is now almost entirely hardened to a substance melting between 40° and 50°, which possesses neither taste nor smell.

The idea of hardening fats is not a new one, and many attempts have been made in the past to find commercial methods for the conversion of oleic acid into stearic acid, such as the action of iodine or sulphuric acid or by the electric discharge (for an outline of and references to these processes see Carleton Ellis, J.S.C.I., 1912, xxxi. 1155), but it was the work of the two French chemists, Sabatier and Senderens, which directly led up to the hydrogenation of oils in presence of reduced nickel or other catalyst. Sabatier and Maihle (Ann. Chim. Phys., 1909 (8), xvi. 73) have shown that oleic and elaidic acids, when treated with hydrogen in presence of reduced nickel at 280–300°, are almost entirely transformed into stearic acid; but in these experiments and quite generally in the application of Sabatier and Senderens' reaction, substances are only used in the state of vapour. Notwithstanding this and the fact that these authors lay stress on the point that the catalyst should not be allowed to become moistened with the substance under treatment (Ann. Chim. Phys., 1905 (8), iv. 335), the present methods for the hardening of oils must be acknowledged to be the direct outcome of the researches of Sabatier and Senderens.

The first patent dealing with the hydrogenation of oils in the liquid form was taken out by Normann (Eng. Pat. 1,515, 1903), who, after referring to the work of Sabatier and Senderens, states: "I have found that it is easy to convert by this catalytic method unsaturated fatty acids into saturated acids. This may be effected by causing the vapours of fatty acid, together with hydrogen, to pass over the catalytic metal, which is preferably distributed over a suitable support, such as pumice-stone. It is sufficient, however, to expose the fat or the fatty acid in a liquid condition to the action of hydrogen and the catalytic substance. For instance, if fine nickel powder obtained by reduction in a current of hydrogen is added to chemically pure oleic acid, then the latter heated over an oil bath, and a strong current of hydrogen is caused to pass through it for a sufficient length of time, the oleic acid may be completely converted into stearic acid. The quantity of the nickel thus added and the temperature are immaterial, and will only affect the duration of the process. Apart from the formation of small quantities of nickel soap, which may be easily decomposed by dilute mineral acids, the reaction passes off without any secondary reaction taking place. The same nickel may be used repeatedly. Instead of pure oleic acid commercial fatty acids may be treated in the same manner. The same method is applicable not only to free fatty acids, but also to their glycerides occurring in nature, that is to say, the fats and oils. Olive oil will yield a hard tallow-like mass; linseed oil and fish oil will give similar results.

"By the new method all kinds of unsaturated fatty acids and glycerides may be easily hydrogenized. It is not necessary to employ pure hydrogen for the purpose of the present invention; commercial gas mixtures containing hydrogen, such as water gas, may also be used."

Such, then, is the wording of the problem submitted to practica chemists for decision as to whether it was a workable process or not.

It must be remembered that in investigating the experimental conditions under which any chemical reaction takes place, failure, either partial or complete, may often be encountered, and it is therefore essential to possess a ready means of detecting, not only whether complete success has been obtained, but also to what extent, if any, the desired reaction has actually taken place. The case under discussion provides an apt example of what is meant by these words. As already mentioned, the manufacturer, as a practical person, is not concerned with providing an illustration of the meaning of the chemical word saturation, but requires a method for converting an oil into a workable solid fat.

It might, therefore, be argued that the hardness of the resulting product would be a sufficiently good criterion of success. It is, however, quite possible to cause an oil to absorb a certain amount of hydrogen without complete solidification, and hence if the reaction is to be followed, scientifically speaking, by the experimental chemist, some much more delicate or exact method must be employed for determining the course of the reaction, and the one actually selected is known as the iodine number or iodine value.

It has already been mentioned that oleic and other similar acids, capable of absorbing hydrogen, are called unsaturated, but hydrogen is not the only substance which can be absorbed by these acids, and among others may be mentioned iodine. Just as oleic acid absorbs two atoms of hydrogen to become stearic acid—

$$C_{18}H_{34}O_2 + H_2 = C_{18}H_{36}O_2$$

so it will absorb two atoms of iodine to become di-iodostearic acid—

$$C_{18}H_{34}O_2 + I_2 = C_{18}H_{34}O_2I_2.$$

If, therefore, after determining the iodine value or iodine absorbed by an oil, hydrogen is allowed to act on the latter in presence of reduced nickel and during the course of the reaction, samples of the product are withdrawn and their iodine values determined, it is found that the amount of iodine absorbed becomes less and less, because some of the unsaturated oil has been converted into the saturated hard fat. Hence we get at any one moment and under any specified set of conditions an exact and quantitative measurement of the amount of hydrogen absorbed.

The results of such a series of experiments are conveniently expressed in the form of a curve, for example as shown in Fig. 1, illustrating the behaviour of whale oil under successful conditions, i.e. employing a definite temperature, purified hydrogen, and a suitable proportion of nickel prepared in an approved manner. The time during which the experiment was continued and the volume of hydrogen absorbed are indicated. In this case during the first hour rapid absorption of hydrogen amounting to 175 c.cm. took place, which then suddenly decreased, and during the second, third, and fourth hours amounted to about 50 c.cm. per hour, whilst during the next three hours only a further 20 c.cm. of hydrogen were absorbed. It will be seen that the iodine value of this specimen of whale oil fell from 130·7 to 38, and the liquid oil became a solid fat melting at 38°.

Let us now glance at the patent from the point of view of the

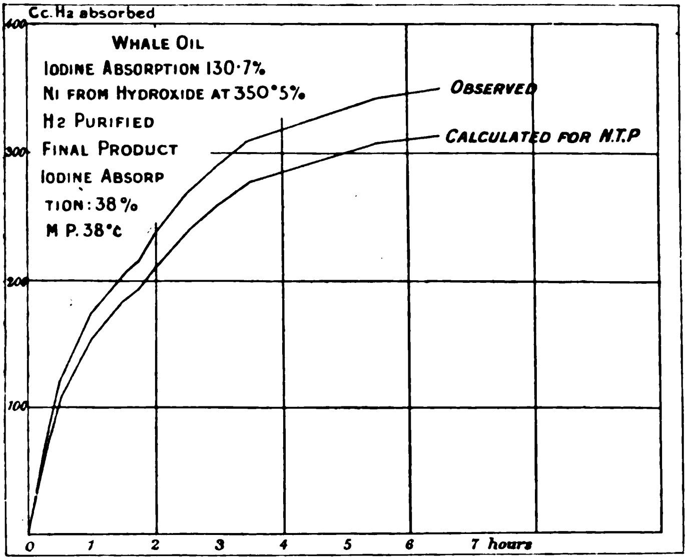


Fig. 1. Temp. 250°.

chemist who is going to investigate the question, and note some of the points which he would have to submit to experimental test.

First, as regards the nickel, the patent states "if fine nickel powder obtained by reduction in a current of hydrogen, etc." Reduction of what? Nickel oxide or nickel hydroxide? and further, does it make any difference as to how the oxide itself has been prepared, or whether the hydroxide has been precipitated from the sulphate or the nitrate of nickel?

Such questions may seem superfluous, and it might be asked: If there is only one nickel, and it is obtainable by all the methods mentioned, how can it possibly make any difference which method is selected for its preparation? Substances such as nickel, which are used as catalytic agents, i.e. bodies which are recovered unchanged at the end of a chemical reaction, though their presence is necessary for the reaction to take place, are, however, of an extremely sensitive nature, and are easily poisoned, or, in other words, their action is lessened or annihilated by the presence of infinitesimal quantities of certain other chemical individuals. For instance, if prepared from the sulphate, the nickel might contain traces of sulphur, which, as will be found later, is for this reaction a deadly poison.

These and many similar points, of so little or of no importance to the lay mind, are in reality the essentials of success. Such points could never be detected without much systematic and organized research, and it would be possible to point to many past failures due to the non-recognition of such apparent trivialities. Doubtless ammonia would have been manufactured synthetically from hydrogen and nitrogen many years previous to 1914, if it had been ascertained that catalysts are susceptible to the action of poisons.

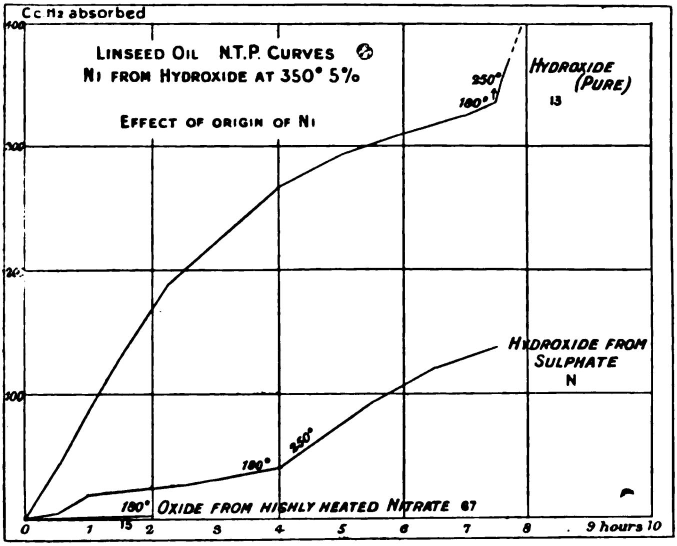


Fig. 2.

A very definite answer to the question regarding the preparation of the nickel is provided by Fig. 2, showing that when the metal is prepared from the oxide, obtained by strongly heating the nitrate, it is practically without action on linseed oil; prepared from the sulphate through the hydroxide (probably containing traces of sulphur), it is rather more active; whereas, when obtained from the highly purified hydroxide, it is extremely active.

The following example, also taken from evidence handed into court in the case already referred to, may be cited in further exemplification of this point. The iodine value of an oil was decreased from 108·4 to 72·1 by treatment with hydrogen in presence of nickel obtained by igniting nickel carbonate at 400–450°, and reducing the resulting oxide with hydrogen at 400°. A specimen of the same oil had its iodine value lowered to 13·2 when treated in presence of nickel prepared from the carbonate by roasting at 300° for 5–6 minutes, and reducing the resulting oxide with hydrogen at 300° for 4–5 minutes.

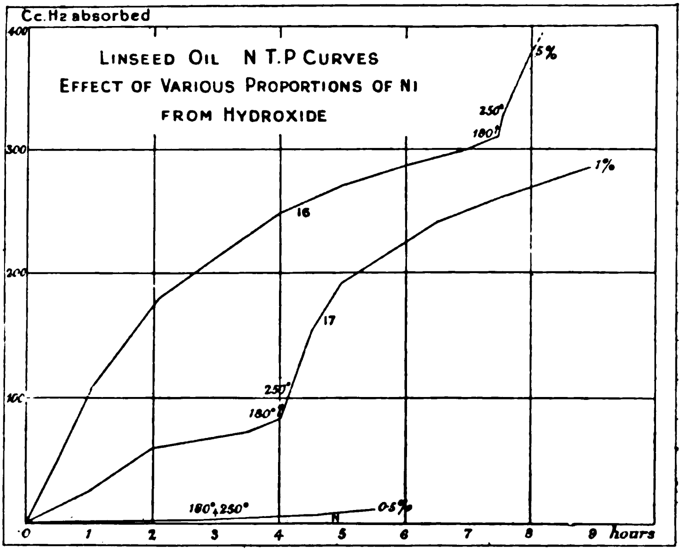


FIG. 3.

In other words, it is not only necessary to make a selection from the chemical methods for preparing the nickel, but it is also of importance to produce the metal in a certain physical condition, obtainable by reducing the oxide for a certain length of time at a definite temperature.

A further very important point is raised by the above curves—namely, the question of temperature, which is not the same for all oils, and which requires to be carefully selected after many trial experiments.

Another phrase from the patent specification which would at once attract the attention of the experimental chemist is the following:—" The quantity of the nickel thus added and the temperature are immaterial, and will only affect the duration of the process."

That may be so, but obviously there must be one set of conditions which is best suited to making the process an economic success, and that is, of course, what the manufacturer desires. There must, therefore, be a definite temperature, and a certain proportion of nickel which will give the best results, and these important factors can only be determined by making numerous experiments.

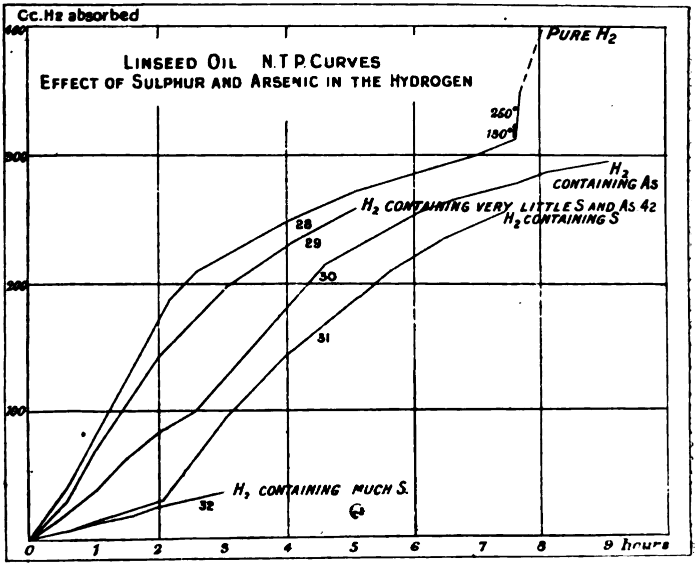


Fig. 4.

These two points are well illustrated by the curves in Fig. 3, which show the great importance of selecting a definite proportion of the catalyst, and also, in common with previous curves, the effect of temperature on the absorption of hydrogen. With 0·5 per cent of nickel there was practically no absorption of hydrogen, even on raising the temperature to 250°; with 1 per cent of nickel absorption was slow at 180°, increasing rapidly on raising the temperature to 250°, whereas with 5 per cent of nickel the absorption was at the end of seven and a-half hours considerably greater at 180° than it was at 250°, when using only 1 per cent of nickel.

Once more referring to the patent, these words are found :—"It is not necessary to employ pure hydrogen for the purpose of the present invention ; commercial gas mixtures containing hydrogen, such as water gas, may also be used." Again, that may be so, but these gas mixtures will not all be equally efficient, hence we encounter more points which have to be put to the test of experiment. The results of such experiments are sufficiently indicated in Figs. 4 and 5, which show the effects produced by the presence of arsenic and sulphur in the hydrogen, or by using commercial gases, such as water gas. By unpurified hydrogen is meant the gas such as would be obtained from a good specimen of zinc without further removal of impurities. Hydrogen containing arsenic was generated from an ordinary sample of zinc. Hydrogen containing much sulphur refers to the gas passed through a very dilute solution of sulphuretted hydrogen in water.

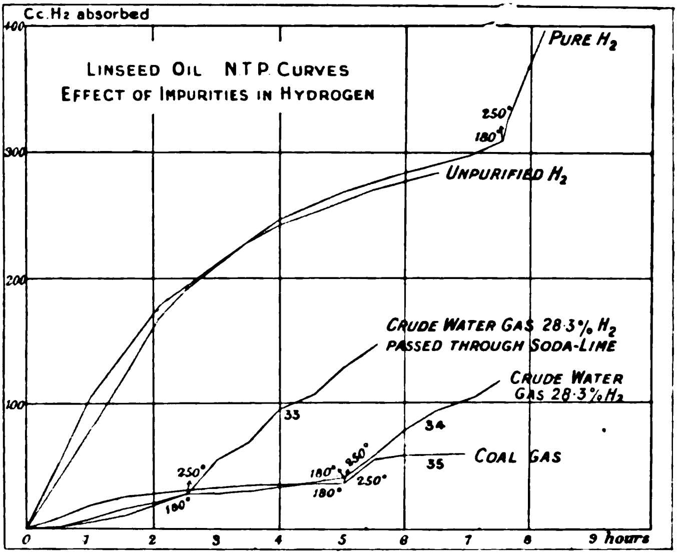


FIG. 5.

The effect of hydrogenation on a variety of acids and oils, indicated by the decrease in their iodine values and by the melting points of the products, is shown by the figures in the following table.

Many other points still remain, requiring investigation, such as the influence of hydrogen under diminished or increased pressure and the refining of the fatty body, but these may be dismissed on the

—	% Ni.	Temp.	Time in Hours.	Iodine Value		Melting Point	
				Of Original Substance.	Of Product.	Of Original Substance.	Of Product.
			ACIDS.				
Ricinoleic acid	3	180°	2	89·8	7·99	Liquid	53°
” ”	6	100°	4	91·8	3·69	”	72°
Erucic acid	10	180°	1	120·3	5·95	”	58·5°
” ”	3	180°	1	74·0	2·05	34°	79°
Linoleic acid	3	240-250°	2	182·1	30·00	Liquid	60·5°
” ”	3	180°	2	176·0	7·79	”	66°
” ”	6	100°	3	179·2	5·98	”	65°
Oleic acid	3	240-250°	½	82·9	4·08	”	63°
” ”	1	100°	1½	82·1	9·77	”	61°
” ”	6	100°	2	84·8	3·83	”	63°
			OILS.				
Whale oil	3	180°	1½	120·0	10·30	”	54°
” ”	3	240-250°	1¾	123·0	27·7	”	48·5°
Cod oil	3	240-250°	1½	157·0	13·1	”	56°
” ”	3	180°	3	159·5	11·1	”	57°
Linseed oil	3	240-250°	1½	181·2	9·52	”	66°
” ”	10	180°	1¼	176·8	4·75	”	62·5°
Rape oil	3	180°	1½	118·2	11·2	”	59°
” ”	3	100°	10½	119·1	24·3	”	56°
Cotton seed oil	3	175°	1¼	101·8	18·5	”	58°
Fish oil	2	190-200°	6	124·5	51·5	”	solid
Almond oil	2	175-185°	6	92·1	22·6	”	—

present occasion and the task of the experimental or laboratory chemist may be considered as practically finished, he having now proved definitely that oils can be hydrogenated on treatment with hydrogen in presence of reduced nickel under certain specified conditions.

But will the manufacturer be satisfied? Can he now go into his works and start the new process? By no means. His task is only going to commence, for the process passes out of the hands of the experimental chemist into the care of the industrial or technical chemist, who may himself be a chemical engineer, or, if not, he will have to work in conjunction with an engineer. Together they will have to study the mechanical devices to be employed for bringing oil and catalyst into contact with hydrogen; the advisability of applying continuous mechanical agitation during hydrogenation, and the many other points necessary to convert the operation, which has been proved workable with a few grams of oil, into one where tons of oil can be as easily changed into hardened fat.

The responsibility for constructing such large scale apparatus is no light one, for it must be remembered that the conditions of experiment established in the laboratory may not be found to be the most suitable for manufacturing operations.

Despite the large amount of work which has been done on the hardening of fats, the literature on the subject is very meagre,[*] and almost the only information which is forthcoming is in numerous patents[†] with which it is quite impossible to deal in detail on the present occasion. A brief description of the processes and forms of apparatus employed is to be found in a paper by Carleton Ellis (J.S.C.I., 1912, xxxi. 1,155 ; Journ. Ind. Eng. Chem., 1913, v. 95).

There is, however, one patent (Lessing, Eng. Pat. 18,988, 1912) not mentioned in Carleton Ellis's paper which appears to differ materially from others, more particularly in the novel method for bringing catalyst and oil into contact.

According to this process hydrogen containing 5–10 per cent of carbon monoxide, such as may easily be prepared from water gas, or the thermal decomposition of coal gas or hydrocarbons, is passed over reduced nickel, with formation of nickel carbonyl. The nickel need not be pure, but in the form of such complex mixtures as are obtained in the treatment and reduction of nickel ores. The mixture of hydrogen and any desired proportion of nickel carbonyl is then passed into the substance to be hydrogenated at a temperature of between 200° and 240°, when the nickel carbonyl is decomposed and elementary nickel, in a very pure and particularly active form, is produced. The proportion of nickel carbonyl required is very small, excellent results having been obtained with an amount equivalent to 0·1 part of nickel per 100 parts of oil. Fresh nickel carbonyl is always passing into the substance to be hydrogenated, and the nickel is believed to act in the nascent condition at the moment of decomposition of the nickel carbonyl. This seems to be borne out by the fact that if the same percentage of nickel be introduced into an oil as nickel carbonyl, the latter decomposed and then hydrogen passed in, there is practically no result from the commercial point of view. (Compare Carleton Ellis, p. 1,160 ; Shukoff's Ger. Pat. 241,823, 1910.)

The process of hydrogenation gives rise to fats of any desired degree of hardness, which are used at the present time primarily for soap-making. It is stated that the soaps produced are very hard and do not lather well, but this objection would not appear to be of a very serious nature, as the hardened fat could be mixed with a suitable quantity of another fat, which would give the desired quality to the resulting soap.

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[*] Since these lines were written, the following book has appeared: "The Hydrogenation of Oils," by Carleton Ellis. Constable & Co., London, 1914.

[†] *English Patents.*—1,515, 1903: 2,520 and 13,042, 1907; 9,112, 1908; 7,726 3,752, 29,612, and 30,282, 1910; 5,188, 13,519, 15,439, 25,326, and 15,440, 1911; 4,702, 5,773, 18,282, 18,310, 18,996, 18,998, 22,092, 23,377, 23,643, 24,084, 72, and 28,754, 1912; 10,204, and 16,283, 1913.

German Patents.—211,669, 1909; 221,890, 1910: 236,488, 1911; 260,885, and 256,500, 1913.

French Patents.—450,703, 1912; 458,445, 1913.

U.S. Patents.—1,004,034, and 1,004,035, 1911; 1,040,531, 1,040,532, and 1,043,580, 1912; 1,059,720, 1,058,738, and 1,060,673, 1913.

Hardened fish oil is odourless and gives a good soap, but unless the iodine value of the oil has been lowered during hydrogenation to 50 or less, then, on ironing materials washed with such soap, the odour of fish oil again becomes noticeable.

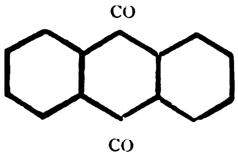
Hardened oils seem likely to be used largely in the future for lubricating purposes, and as foodstuffs. Even at the present time hardened cotton seed oil, a substance resembling lard, is used edibly, and a great number of physiological experiments carried out on the edible properties of hardened whale oil (Offerdahl, Ber. deutsch. Pharm. Ges., 1913, xxiii. 558) have proved it to be easily digested.

One point, however, will require very careful consideration in this respect, namely, that, as mentioned in the patent, an oil hardened by a process including nickel as catalyst always contains small amounts of nickel, as nickel soap, i.e. the nickel salts of the acids contained in the original oil. Offerdahl has shown that the average amount of nickel in hardened whale oil is from 0·5–2 milligrams per kilo., whilst the most found was 4 milligrams per kilo. Despite the facts that 99·8 per cent of nickel taken is eliminated from the system, and that Normann and Hügel (Halbmonatsschrift für Margarine-industrie, 1913, vi. No. 17) have shown that this amount of nickel is very much less than that contained in many foodstuffs prepared in nickel vessels, which have had no injurious effects on those who have eaten them, the matter is one which will require detailed attention, and hence into this industrial problem is drawn still another research worker, namely, the physiologist, and we may leave him to work out his experiments while passing on to consider another phase of research as applied to industrial problems.

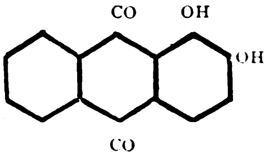
The investigation of the process of fat hardening provides an instance of a research in which the problem to be solved is recognized from the start; it is therefore systematically attacked and conquered. The question to be answered is simply—What are the conditions under which it is possible to change an oil into a solid fat, by the addition of the necessary number of atoms of hydrogen to convert the glycerides of unsaturated acids into the glycerides of saturated acids? There are, however, other types of problems which come under the notice of the research chemist, where, for example, though the ultimate aim is a perfectly definite one, the means of attaining the desired end are not foreseen, and it is only by the systematic scientific investigation of thousands of chemical derivatives in the laboratory that the end is finally achieved.

The domain of organic chemistry proper or the chemistry of the compounds of the element carbon may be called upon to provide an example. The organic research chemist is frequently alluded to with perhaps a tinge of scorn as a "compound maker," by which epithet is meant one who sets out to make new organic substances without apparently any particular object in view, except that of filling the pages of scientific journals with very long names and equally long formulæ, which have no particular interest for anyone. But our organic chemist can afford to bear the brunt of this criticism with equanimity; for though the labours of any one chemist may not seem to have furthered, to any appreciable extent, the aims of scientific industry, yet, when the results of the labours of several such research workers are put together and systematized or organized, certain well-defined facts appear, of which scientific technical chemists have not been slow to avail themselves.

The aim of the research chemist in the particular instance chosen was to produce from anthraquinone (I), a substance obtained by oxidizing the hydrocarbon anthracene contained in coal tar, the greatest possible variety of colouring matters; a perfectly definite object, but how was it to be attained?



(I)



(II)

Synthetic chemistry had proved as long ago as 1869 that the introduction of two hydroxyl groups into the anthraquinone molecule gave rise to a brilliant red dye stuff, named alizarine (II), or perhaps better known as Turkey red. For twenty years after this discovery comparatively few related colouring matters were produced, these few including flavopurpurine, anthrapurpurine, alizarine orange, and alizarine blue. These colours cannot, however, be used to dye cloth without the use of mordants, different shades being produced according to the mordant employed. Suddenly, about twenty-five years ago, the anthraquinone dye industry began to develop very rapidly, due largely to the discovery of derivatives capable of being used without mordants and distinguished by a rare solidity of colour of all shades of the spectrum—yellow, orange, red, violet, blue, and green. In the last twenty years literally thousands of these colours have been made, of which some have proved to be useless from the commercial point of view, because they are not fast to light or to the process of soaping, or because the brilliance of the original shade was not permanent. Nevertheless, the manufacture of a selection from among these thousands of colouring matters has proved to be one of the greatest commercial enterprises of modern times. (R. Schmidt, Bull. Soc., June 1914.) How has this result been achieved? Simply by systematic "compound making" on the part of hundreds of highly trained organic chemists.

As it had been shown that the introduction of the hydroxyl group into the molecule of anthraquinone gave rise to a colouring matter, organized science at once became inquisitive, and was anxious to know if the hydroxyl group was the only grouping which was capable of producing this result. If not, what other groups had similar effects? Was a variety of colours produced by the introduction of different groups? Did the number and position of these groups in the molecule give rise to marked differences of shade in the resulting dye stuffs?

Answers have been supplied to all these questions in a manner which fills a chemical soul with nothing but admiration. Such work has cost thousands upon thousands of pounds, provided largely by manufacturers who, while not seeing any possibility of immediate return for their money, still appreciated to the fullest possible extent that pure scientific research work, carried out in the laboratory, is the soul of industrial prosperity.

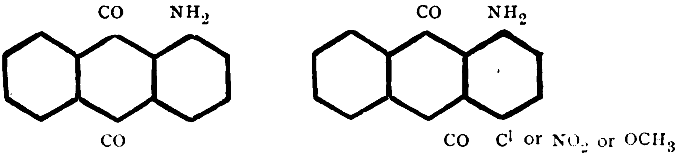
In the first place the methods by which the following groups could be introduced into the anthraquinone molecule were fully investigated:—

$$Cl,\ Br,\ NO_2,\ OH,\ SH,\ NH_2,\ NHCH_3,\ NHC_6H_5;$$

and, secondly, the influence on the shade or colour of the substance resulting from the introduction of one or more of these groups into different positions in the anthraquinone molecule. As a result it has been definitely proved that the colour of a derivative of anthraquinone depends on two factors:—

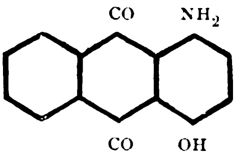
1. The nature of the substituting groups.
2. The number and position of these groups.

The influence of the substituting group is shown by the fact that on introducing in the para position to the amino group, in aminoanthraquinone, a chlorine atom, a nitro or a methoxy group, little or no effect is produced, so that the following substances have all an orange or brick-red colour:—

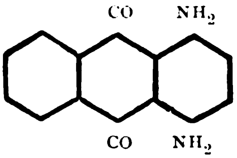


Orange to Brick-Red.

But when a hydroxyl group enters the molecule, para to the amino group, a bluish red colour is produced, and an amino group gives a violet colour.

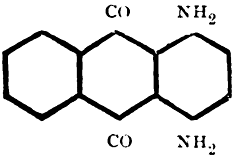


Bluish Red

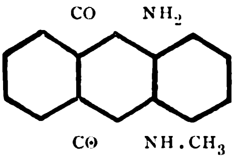

CO
NH$_2$
CO
NH$_2$

Violet

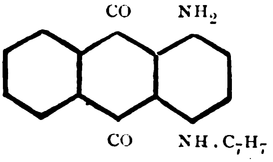
The point is further illustrated by the following series :—


CO
NH$_2$
CO
NH$_2$

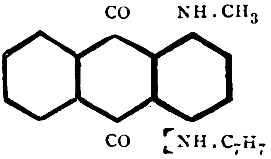
Violet


CO
NH$_2$
CO
NH.CH$_3$

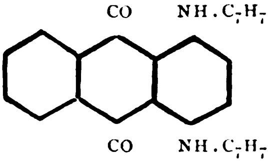
Blue Violet


CO
NH$_2$
CO
NH.C$_7$H$_7$

Blue

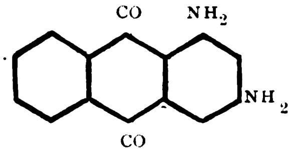

CO
NH.CH$_3$
CO
NH.C$_7$H$_7$

Greenish Blue

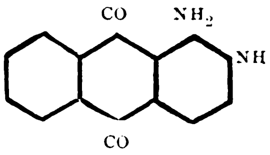

CO
NH.C$_7$H$_7$
CO
NH.C$_7$H$_7$

Green

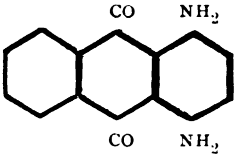
The influence of the number and position of the substituting groups is again shown by a comparison of the formulæ and colours of the following substances :—


CO
NH2
NH2
CO

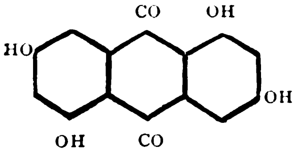
Yellow Red



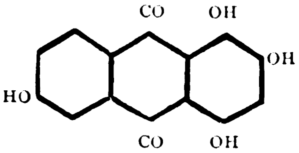
Bluish Red


CO
NH2
CO
NH2

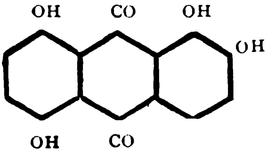
Intense Violet.


CO
OH
HO
OH
OH
CO

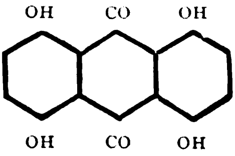
Yellow.


CO
OH
OH
HO
CO
OH

Orange.


OH
CO
OH
OH
OH
CO

Orange Red.

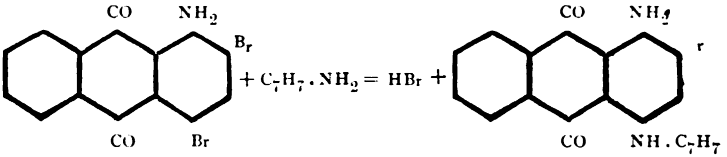

OH
CO
OH
OH
CO
OH

Bluish Red.

The information gathered from these investigations has been systematized to such an extent that, knowing the constitution of any anthraquinone derivative, it is possible to predict with certainty the effect which the introduction of one or several groups into the molecule, in any of the available positions, will have on the colour of the resulting substance; or, stated in another way, if it is required to produce a dye of a particular shade, it is no longer necessary to make numerous experiments, for one versed in the science could write down at once the formula of the dye with the desired colour, and if it had not previously been made, he would know exactly the series of reactions by which it could be manufactured.

It sometimes happens that accident or luck, as it is erroneously called by some people, plays a very important part in research work, and the manufacturer of anthraquinone derivatives is again able to provide an example of what importance a chance observation may be, if only it is made by a trained chemist, who allows no fact to escape him unexplained, and who has the necessary scientific ability to follow up and master such occurrences.

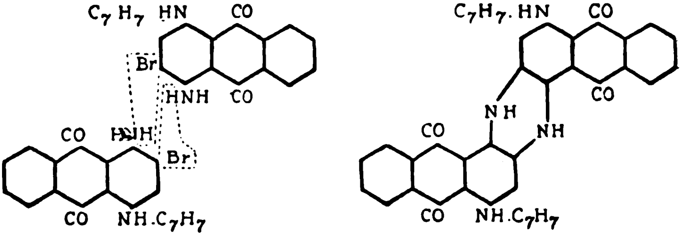
In the preparation of a colouring matter called "sky blue alizarine" (III), it was customary to heat dibromaminoanthraquinone with para-toluidine, when the following reaction took place :—



(III)

On carrying out this replacement in a new apparatus, but otherwise under exactly the same conditions as previously, none of the sky blue alizarine was obtained, but an entirely different derivative.

How was the explanation of this fact to be sought ? Surely it could not possibly have anything to do with the new apparatus. Investigation showed, however, that such really was the case, for whilst the old apparatus was made entirely of iron, the new one had a copper lid, and it was soon proved that the presence of the merest traces of copper exerted a very great influence on the course of the above reaction, resulting in the formation of a derivative of indanthrene.



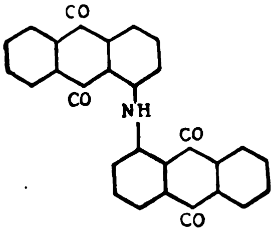
It may seem very interesting, and perhaps wonderful, that the course of the reaction was thus established, and that a method was thereby devised for the synthesis of indanthrene derivatives.

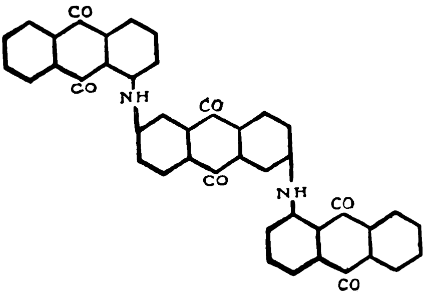
But one can almost hear the question, What was the good of this work? If it was desired to make sky blue alizarine and the reaction did not give the desired result, what was the object of further investigation?

The reply is obvious, when the course of the chemical reaction involved in the production of the indanthrene derivative is followed a little more closely. The chemical fact, by virtue of which an indanthrene derivative is formed, is the interaction of an amino group in one substituted anthraquinone molecule and the bromine atom in a second molecule, with consequent linking together of the two molecules.

Then comes the important question, Is this reaction a general one? Investigation soon proved that minute traces of copper influence in a surprising and general way the interaction of halogen derivatives of anthraquinone with primary aromatic amines, and there has resulted another new series of anthraquinone dye stuffs, of which many, such as orange algol, bordeaux algol and indanthrene red, are of great commercial importance.

The following examples serve as types of the constitution of such derivatives:—





Now it may well be asked, what would have been the result of the accidental occurrence connected with the installing of this new apparatus for sky blue alizarine if skilled research assistance had not been at hand? Would it still have been called luck? Will you try to place yourselves in the position of the foreman in charge of the operation and then reply to these questions?

Instead of receiving nothing but severe censure for carelessness, that foreman, whoever he was, learnt to appreciate the value of the skilled chemist in the works; he would not be slow to let those under him know his opinion on the subject, and so gradually throughout the whole factory the leaven of scientific enquiry would spread, even to the most recently admitted workman.

Time will not permit me to review the many economic advantages which have accrued as the result of the application of science to the solution of industrial problems; but I may mention the catalytic process for making sulphuric acid, which substance may be described as the basis of all chemical industry; further, the synthetic production of nitric acid and ammonia from atmospheric nitrogen; the investigation of the rare earths and the resulting improvements in lighting due to the use of metallic filament lamps; the synthetic formation of precious stones, such as the ruby; the production of dye stuffs of all conceivable shades, including synthetic indigo, on which it is stated nearly £1,000,000 was expended before final success was obtained; the hardening of fats; the production of explosives, synthetic remedies and many of the natural perfumes contained in flowers and plants.

Very little of the work connected with the technical side of these problems has been done in this country, not because we have not any intelligent manufacturers, chemists, or workmen, but because the nation has not yet learnt to appreciate the value of scientific enquiry. English money is quickly forthcoming when a clear and immediate 5–10 per cent profit is discernible, but it has not been forthcoming for investment in research, where a return is certain, though it is impossible to say what length of time will be necessary to ensure victory.

Nor will money alone bring success, which will only be attained when the individual members of our Governments, of our Houses of Parliament, and of the community at large, have learnt to appreciate one fact—namely, that

“It is pure and organized scientific research work, carried out in the laboratory, which is the soul of industrial prosperity.”

[A. C.]

WEEKLY EVENING MEETING,

Friday, February 12, 1915.

Donald W. C. Hood, C.V.O. M.D. F.R.C.P.,
Vice-President, in the Chair.

William S. Bruce, LL.D., F.R.S.E., Director of the Scottish
Oceanographical Laboratory, Edinburgh.

Recent Advances in Oceanography.

Dr. Bruce pointed out that the science of oceanography began with those tentative expeditions that led to the dispatch of the "Challenger," and that the lamentable death of Sir John Murray, followed closely by that of Sir George Nares, together with the tragedy of the European war, marked the end of the first era in the History of Oceanography.

Defining oceanography as the detailed study of the oceans, both biological and physical, he proceeded to recall some of the earliest investigations that had been made in deep seas, Commodore Phipps sounding in 780 fathoms in 1773, Sir John Ross in even greater depths, and Sir James Ross sounding in 2425 fathoms. Both Rosses also succeeded in securing animals from great depths. The early work of Edward Forbes in 1840, Gwyn Jeffreys, Wyville Thomson, Carpenter, and Sars was briefly referred to, and more especially the work of the "Lightning" and "Porcupine" in 1869, and of the "Challenger" in 1874. Dr. Bruce made special reference to the Prince of Monaco's researches, and to the part that Mr. J. Y. Buchanan, Dr. Jules Richard, and others had taken in them.

Having considered the work of other oceanographical expeditions, Dr. Bruce referred to the establishment of Marine Biological Laboratories and Fishery Bureaux in Europe and Britain. Scotland led the way in the development of Marine Biological Stations by the establishment of two stations in 1884, namely, those of St. Andrews and Granton, and next year those of Tarbert and Millport. England followed, dedicating the Laboratory at Plymouth in 1887. Sir John Murray was the leading spirit in all those movements.

Having given a general account of the Marine Biological Stations, which Dr. Bruce showed were established on the principle of doing local marine research, he spoke eulogistically of the magnificent Oceanographical Institute established by the Prince of Monaco at Monaco and Paris for world Oceanography, and classed it as the most important Institute of its kind in the world. He also briefly referred to the Oceanographical Institutes of Berlin and Tokyo. In Britain the only two Institutions that could be compared with these were in Edinburgh, namely, Villa Medusa, where the late Sir John Murray concentrated his best efforts, and the Scottish Oceanographical Laboratory, with which several oceanographical expeditions have been associated. Dr. Bruce pointed out that, as soon as more propitious times returned, it was intended to establish in Edinburgh an Oceanographical Institute of Imperial importance. The Forth and Clyde area had been the cradle of oceanographical research since the dispatch of the "Challenger." The importance of this area for an Oceanographical Institute was emphasized by the stupendous naval base developing at Rosyth, and the practical certainty of the cutting of a great Forth and Clyde Ship Canal which would be the most direct east and west highway between Northern Europe and America, including the Panama Canal, and which offered a magnificent access both by the Forth and Clyde. He had been told that in naval affairs science was at a discount at present, and that only those who could fight were required But modern fighting more than ever depended on the application of the most intricate scientific research, physical and biological. It was our physicist and mathematician who guided the head and hand, not only of the gunmaker, but also the shipbuilder in building the very ships and turrets upon which the guns were borne, while the explosives used were the outcome of the work of the chemist in his laboratory. Our knowledge of the drift of mines was due to much careful work by men of science, while the bacteriologist had reduced mortality from wounds and disease to a minimum, and thus increased the power of the very fighters themselves. It was, therefore, very appropriate to establish such an Oceanographical Institute in the Forth and Clyde area.

Referring to ocean meteorology, the importance of the work of Mr. R. C. Mossman was dwelt upon by the lecturer, showing how climatic conditions in one part of the world were related to those of another, owing to certain "centres of action" of high and low pressure. Dr. Bruce also reviewed magnetic work at sea, from Columbus' discovery of the line of "no variation," Scoresby's magnetic voyage, and, finally, to the work of the magnificently equipped yacht "Carnegie."

Having thus summarized the general features of oceanography, he dwelt specially upon Polar oceanographical work. The researches of James Murray, Bartlett, and Mackinlay on the ill-fated "Karluk" he considered stood out as one of the best pieces of oceanographical work that had been done in the Arctic seas, and told a tale which, but for the tragedy of the war, would have thrilled the world with its story of devotion and heroism in the quest for adding to the sum of human knowledge. The observations eliminated the possibility of land to the north of Alaska, and included the extension of a deep Polar basin to within sixty miles of the shores of Alaska. He referred to the hydrographical observations of the "Scotia" in the Labrador Current in 1913; the "Scotia" had been chartered by the Board of Trade, in order to carry on investigations with regard to the distribution of ice, after the wreck of the "Titanic."

Antarctic oceanography, Dr. Bruce said, could not be considered without special reference to the work of Ross, Hooker, Murray, and Buchanan. Everything that Ross did remains good to the present day; much has been added, but nothing taken away.

The Scottish Antarctic Expedition of 1892 led the way in modern Antarctic exploration. The work carried out by it was oceanographical in character. These were the first scientific observations taken in these seas since the time of Ross. This expedition resulted in a wave of enthusiam which ultimately led to the dispatch of the many international expeditions during the past twenty years. The "Belgica" was the first ship to pass a winter in the Antarctic regions, and made a most extensive and systematic bathymetrical survey, mapping out the continental shelf of the Antarctic continent along a distance of 500 miles, and determining its mean depth.

The voyages of Scott, Shackleton, and Amunsden did not emphasize oceanography, though they shone in brilliant achievements on shore. The Belgian, Scottish, French, and Australasian expeditions carried on a large amount of oceanographical work.

The "Scotia" had carried out the most extensive deep-sea researches of any Antarctic expedition. These researches still hold the record despite the fine oceanographical work of the Australians. The Australians carried out a fine bathymetrical survey between Australia and New Zealand towards and along the coast-line of Antarctica over a stretch of almost 2000 miles. Many valuable dredgings were taken by the Australians under the direction of Dr. Mawson and Captain Davis, including very rich trawlings on the continental shelf of Antarctica.

Referring especially to the work of the "Scotia," he said she was fitted out to sound, trawl, and take physical observations in the Antarctic Ocean to depths as great as 6000 fathoms if they were to be found. The whole equipment of the "Scotia" was based on the record of Ross, having in 1843 sounded in the Weddell Sea with 4000 fathoms of line, and without touching the bottom. It was found that Ross's sounding was incorrect, the "Scotia" touching bottom at 2660 fathoms and fetching up a sample of the "blue mud." This was mainly due to the primitive sounding equipment that Ross had compared with that used by the "Scotia" after sixty years of development. Altogether the "Scotia" took 75 deep soundings in the region described, besides nearly 500 in the neighbourhood of the South Orkneys in water less than 100 fathoms. Fifty of these deep soundings were taken whilst actually navigating in the ice; 43 were taken in water exceeding 2000 fathoms. The deepest sounding was 2900 fathoms, rather more than 3¼ miles. The work was not carried out without considerable difficulty, labour, and discomfort. Usually sounding platform, sounding machine and workers were covered with ice from the frozen water carried aboard by the incoming wire. Pack-ice floating in the water formed an additional difficulty, with the risk of it carrying away the sounding wire and valuable instruments attached to it.

The discovery of Coats Land, which was named in recognition of the support that Mr. James Coats, Jun., and Major Andrew Coats gave, added half a million square miles to the supposed area of the Antarctic continent, and defined the southern limits of Antarctic seas in those longitudes. The deep-sea zoological collections of the "Scotia" were absolutely unique. Dr. Bruce, finally, emphasized the importance of carrying out further hydrographical research in Antarctic seas. A circumpolar oceanographical expedition was the most important work yet to be carried out in the South Polar regions.

Dr. Bruce's discourse was illustrated by a specially fine set of lantern-slides, and by a large series of diagrams and deep-sea instruments lent by the Scottish Oceanographical Laboratory. Besides these, he also showed a number of animals that had been captured by the "Scotia" naturalists in Antarctic seas in all depths and latitudes; most of these animals were new to science, and many were taken from depths exceeding 2500 fathoms, or three miles, and have not been taken by any other Antarctic expedition. The price of one battleship, said Dr. Bruce, would permanently endow Antarctic exploration. Given the brains multiplied by sound physical constitution and experience, then money multiplied by time would solve every Antarctic problem at sea or ashore.

[W. S. B.]

WEEKLY EVENING MEETING,

Friday, February 19, 1915.

EDWARD POLLOCK, Esq., F.R.C.S., Vice-President,
in the Chair.

PROFESSOR HENRY E. ARMSTRONG, Ph.D. LL.D. D.Sc.
F.R.S. M.R.I.

The Visit of the British Association to Australia.

THE British Association for the Advancement of Science is an established Imperial Institution, now in its eighty-fifth year. Severely affected by wandering instincts in its maturity, it has journeyed thrice to Canada—to Montreal in 1884, to Toronto in 1897, to Winnipeg in 1904—ever growing bolder in its westward flights. Meanwhile, it began to travel equatorwards and in 1905 visited South Africa, stopping at Cape Town, Durban, Pietermaritzburg, Colenso and Ladysmith, Johannesburg, Bloemfontein, Kimberley, Bulawayo, finally sighting the Victoria Falls, where the President personally opened the bridge over the Zambesi; some of the party returned via the East Coast, so that they circumnavigated the African Continent. This last year the Association reached the Antipodes. Some of us journeyed across the Australian Continent, from Freemantle on the extreme south-west, by sea to Adelaide, thence by rail to Melbourne and afterwards to Sydney, so noted for its harbour, finally to Brisbane high up on the east coast; from there we steamed up the eastern margin by sea, skirting the Great Barrier Reef, with its wonderful Coral Island scenery, to Thursday Island in Torres Straits, calling first at Townsville and its appendage Magnetic Island, then at Cairns, on the Queensland coast, passing thence westwards to Port Darwin, the capital of the Northern territory, slightly west of the position which Adelaide occupies in the south: so we all but circumnavigated our great island possession. Small parties visited either Tasmania or New Zealand.

In Australia various excursions were made to points of interest inland away from the big towns—to forests and other regions of indescribable beauty near Melbourne; to the Mount Lofty district and the National Park near Adelaide; to the wonderful scenery, near Sydney, of the Blue Mountains, including the limestone caves for which the district is famous; to mining centres, such as Kalgoorlie, Iron Knob, Broken Hill, Ballarat, Bendigo and Mount Morgan; to sheep runs; to fruit and other farms; to irrigation areas; to sugar plantations; to the vast region over-run with the dread prickly pear—so that what we did not see collectively we might hope was scarce worth seeing; yet we must admit that what we saw was as nothing, so vast is the island, covering, as it does, an area only slightly less than that of the whole European Continent.

There has been great difference of opinion, in the past, within the Association, as to the advisability of our taking long journeys away from our islands. I venture to think that the objectors, if not selfish, have at least been narrow and insular in their outlook: moreover, there is the point of view represented by *Punch's* recent remark: "It's an ill wind that blows nobody any good; the War has at least saved us from any mention of the British Association proceedings in the newspapers." I will not reproduce here the arguments for and against but merely state my opinion that such visits are of infinite value to all parties, both to those who take part in them and to those whom we visit; it is to be regretted that our younger members have not shown more enterprise in availing themselves of the great opportunities that have been offered to them of learning geography in the only way in which it can be learnt—in the way that Mr. Squeers required his boys to learn to spell "Winders" by cleaning them.

Only seeing is believing. Even in these days of photography, when a new journey is undertaken, the things which are seen rarely correspond with the impressions formed in advance by reading; at least, this has been my experience recently in such countries as Java, Ceylon and India.

It is admitted that it takes all sorts to make a world; it is equally true that it takes all sorts to see the world, to see it in proper perspective. Too few travellers are sufficiently trained as observers to see properly; moreover, the point of view each traveller represents is usually very limited. Our Association should represent all points of view; it can travel as no other body can travel—with all-seeing eyes.

When we talk of Australia, what does it represent to us? A far distant land; in the past a veritable Tom Tiddler's Ground, where the most noble of metals is found, where sheep are pastured in millions and bear a veritable Golden Fleece; a land ever calling out for emigrants, yet never receiving them in the numbers it desires. But how many of us know what the conditions it offers really are?

In various ornate shop-windows in London and elsewhere, we see displays of grain, of rosy apples, of golden nuggets, of minerals galore, of opal and other precious stones, backed by most attractive photographs of fern-clad glens and giant trees, all spread out to tempt the unwary; but these do not represent Australia, at all events Australia as we saw it; some of its rewards may be indicated but its hardships are rarely portrayed. I fear, indeed, the pictures that are painted of the outlying regions of our Empire are not always fair; they certainly lack the necessary detail of hardship.

There are few of us who have made the pilgrimage who do not now feel, I believe, that we have visited a land of which we had no understanding previously and one of which we still lack full understanding, so numerous and complex are its problems, so different the conditions it offers from those with which we are acquainted at home; a land as yet unmasked as to its potentialities and its future, full of difficulties, though rich in promise; a land in no way made entirely clear, least of all, perhaps, to those who people it. Unfortunately, there are "such quantities of sand" and far too little water. It is like no other land in aspect and colour, owing to the spare and pendant character of much of the foliage and the peculiar grey or olive-green of the *Eucalyptus*, a genus spread everywhere, which dominates all but the so-called desert regions, where again, however, the vegetation is peculiar in form and hue. The grey evergreen vegetation of Australia, it may be noted, in general aspect is not unlike that of Mediterranean countries; moreover, all the Mediterranean plants flourish in Australia, even the clovers are gaining hold in the pastures.

One of the questions often asked of us was, "What do you think of the Wattle?" the national flower, used as the floral emblem of the Commonwealth. There are over 400 species and like the *Eucalypts*, of which at least 230 species are known, the Wattle or Acacia, known here as Mimosa, pervades the Continent, giving it everywhere in spring-time a golden glow such as no other land exhibits. We were too early to see the Wattles at their best in the Adelaide region and a little too late in Queensland but along river banks in the Healesville district near Melbourne and in valley bottoms in the Blue Mountains near Sydney, we saw enough of their golden glory to convince us of their beauty and to justify the repute in which the species is held. Unfortunately, as civilization proceeds, the characteristic vegetation must disappear and probably at the next visit of our Association much of the country dressed in sober grey or olive-green passed through during our visit will be clothed with emerald-green in winter and spring and golden yellow in summer, because wheat will have taken the place of forest and pasture. Not a little of Australia is still virgin land, untouched even by savage man and as it is so peculiar in character, travellers should lose no time in seeing its special features, as the opportunity will not last for ever. To judge from what we saw at Brisbane and at Cairns, in the neighbourhood of the Baron Falls, the tropical forests of Queensland are truly primeval, such as it is impossible to see elsewhere—far more striking than those of Ceylon or Java even.

Unfortunately, too many Australians, like Americans and Canadians in the past, are behaving as utter barbarians and with the selfishness and lack of forethought characteristic of civilized man are not only killing the trees everywhere by ring-barking them and burning the undergrowth, even the long grass, between the trees, in order to improve the pasturage; by firing the forests in the hill ranges they are also recklessly destroying large areas of most magnificent timber. To many of us the aspect of such land, the sight of an endless succession of charred forest giants destroyed by fire, was heart-rending; we felt not seldom that there was urgent need of a little shooting at sight to inhibit the practice. It is difficult to believe that some remunerative method of cutting up the timber on the spot at the time of clearing cannot be developed, whereby some part at least of the present awful waste could be prevented. It is strange that in a country at one time covered with trees complaint should already be made of the treeless character of the cultivated areas and that much is now being done to popularize tree-planting for shade and ornamental purposes on farms; it is even proposed to cultivate some of the species of *Eucalyptus* which yield valuable products.

Our expedition, some 300 strong, left England in sections late in June or early in July last. I need say nothing of our transport. The steamship companies who carried us doubtless did their duty but they in no way treated us as strangers or granted us any special privileges, though we came as a windfall to them at a slack season. I was one of a party that journeyed via the Cape of Good Hope. The one event of interest on the voyage happened on the afternoon of the first day, as we were about to leave the English coast—we were called by "wireless" to the help of a ship ashore on the Scilly Islands. After three or four hours' cautious steaming through fog, we suddenly came upon her high on the rocks—a striking spectacle—only to learn that the passengers and crew were already landed, a fact that might well have been notified to us some time previously; still, we acquired confidence in the skill of our captain and had an interesting demonstration of the value of the wireless system. Another was afforded us subsequently, when we were about half-way between the Cape and Australia and were able to make arrangements with Perth, 2100 miles away, in connexion with a severe accident to one of our party requiring early surgical treatment.

We had several delightful hours at Las Palmas, Grand Canary, a volcanic district. Owing to the delay off Scilly we were a day late in arriving at Cape Town and therefore could not avail ourselves of the special arrangements made for our reception on the previous Sunday. To our intense annoyance we were only allowed four hours ashore. Even in this short time, however, thanks to the efficiency and courtesy of our guides, those of us who had not been there before were able to gain some idea of the beauties of the Cape Peninsula and of the astounding wealth and individuality of its vegetation; we were just able also to pay personal homage to the memory of the great man who had made it his home. But we were an angry, almost mutinous, body as we left, realizing as we did, for

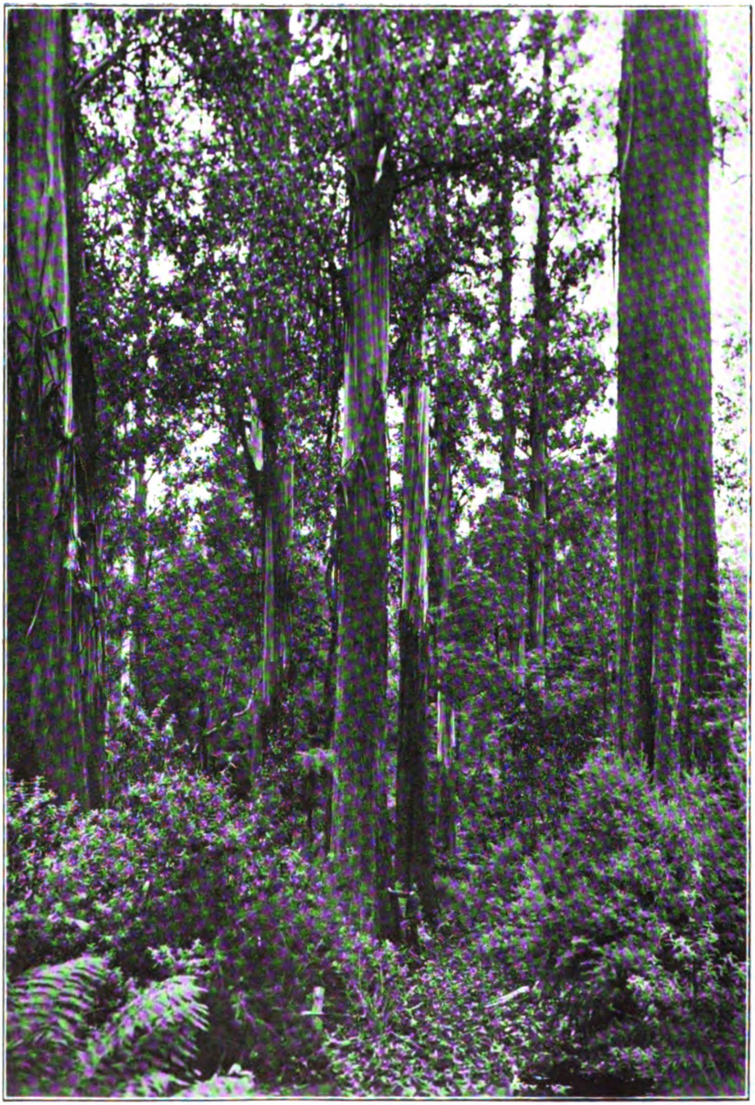


FIG. 1.—EUCALYPTUS FOREST; HEALESVILLE DISTRICT, NEAR MELBOURNE.

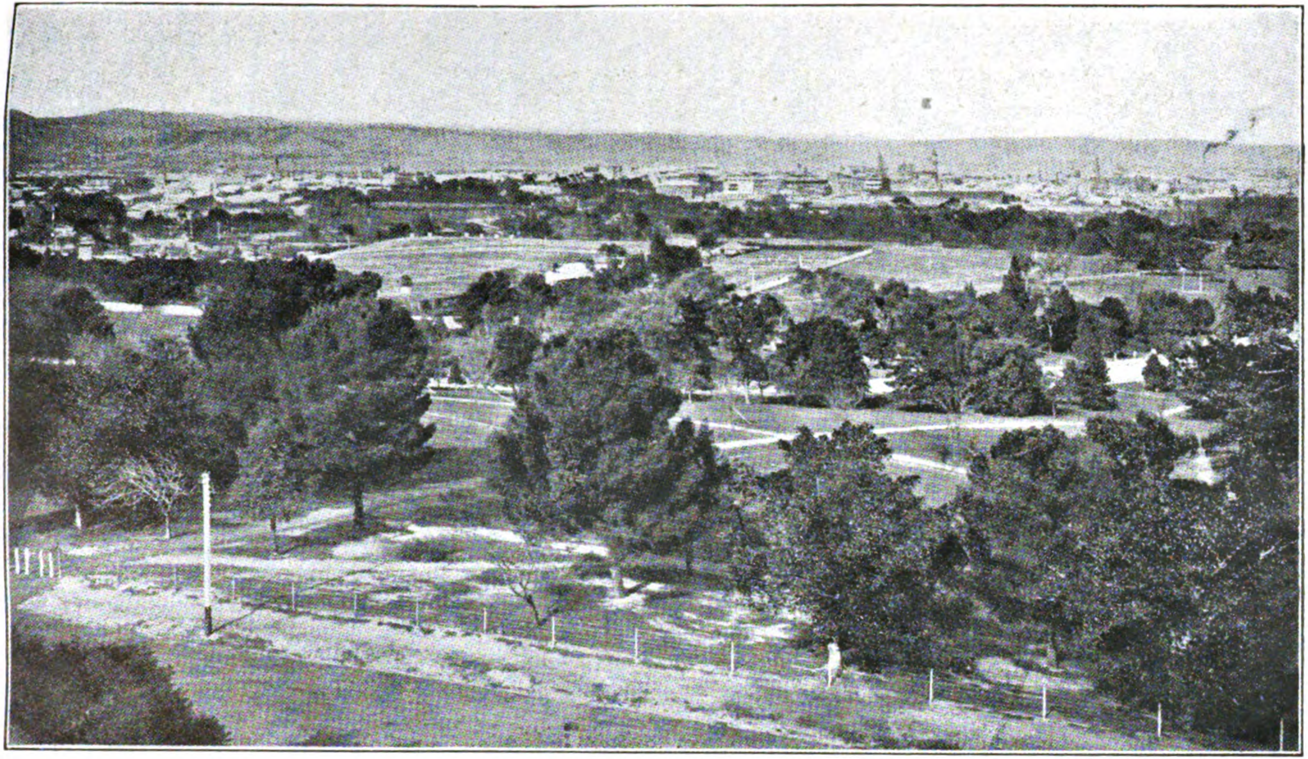


FIG. 2.—ADELAIDE, SHOWING BELT OF PARK LAND AND GARDEN ENCIRCLING CITY AS ORIGINALLY PLANNED.

the first time, that no one had thought out a plan of campaign for our voyage and that we were sacrificing golden opportunities. We felt this still more keenly later on, when we were denied the chance of passing close enough to see the islands of St. Paul and Amsterdam, near the 38th parallel of latitude, in the Indian Ocean. St. Paul is one of the most remarkable volcanic islands in the world, on account of the way in which one side of the cone is breached, letting in the sea to form a crater lake. This time we were too soon and we passed considerably to the south of the islands during the night; had we spent an additional four hours at the Cape we could have been abreast of them during daylight, in which case our captain was to have taken us within a mile of St. Paul. We were timed to arrive in Australia only the day before proceedings commenced, without any allowance for contingencies; had we had a couple of spare days at our disposal or had our ship been less of a tramp, with a spare ounce or two of power, all would have been well—we should not have journeyed thousands of miles only to miss an opportunity such as can never be regained. In the end, it was found that a retardation to enable us to see St. Paul could easily have been allowed, as on our arrival, on July 28, we were unable to land at once at Freemantle, West Australia, the Port Medical Officer being absent at Perth at the garden party arranged in our honour. This time Mr. Marconi either failed us or no notice was taken of the signals advising our arrival; but we could forgive him this one breakdown, as he had stood by us so well during the voyage. The splendid isolation of the past is no more known at sea; though not a single vessel was sighted by us during the long passage from the Cape to Australia, we were always in sympathetic vibration with one or more and even learnt of the assassination of the Austrian Archduke while on our way, an event which some of us at once thought to be of serious import.

Though unvisited by man after leaving the Cape, we were always accompanied by a number of giant albatross, 9 to 12 feet at least across the wings, whose wonderful powers of flight and beauty on the wing were a source of constant enjoyment to us. And at night we had the Southern Cross to admire and a starry firmament remarkable in comparison with that of our northern hemisphere, on account of the prominence of stars of high magnitude, the beauty of the nebulæ and the remarkable density of the Milky Way.

The port of Freemantle is but a short motor ride from Perth, which is most beautifully situated on hills bordering the Swan River, so named because it is the home of the black swan. Here we were in a strange land, full of wonders. Ordinary people will tell of the agricultural development and possibilities, of the timber and the vast quantities of wheat that are being brought down from the interior. And a truly wonderful development is to be recorded. In 1890 the amount of wheat produced was about half a million bushels but in 1913 this had risen to 13·5 millions, the average yield being

12·2 bushels per acre; it is expected that in 10 to 15 years the 50 million bushel mark will be reached.

Most of us, however, would rather speak of the natural vegetation, which is totally unlike anything known to us here in appearance—extraordinary in its variety as well as in the character of the foliage, which has obviously been evolved to withstand long periods of drought. At Perth, though it was only early spring, we found already in blossom a number of heaths (Epacrids) and many remarkable *Proteaceæ—Banksias* and *Hakeas.* Especially striking was the profusion of species of *Drosera* we met with here and elsewhere. Some very beautiful trailing *Leguminosæ* (*Kennedya*, *Swainsona*) were also seen.

A very advanced party spent fourteen days in West Australia; others only a week; a few of us only a day—but in that day we saw a vast deal, thanks to the zeal of our local guide, who took us by motor right out into the bush on the Darling Hills, to a magnificent view point, then to a vineyard and finally back into town through the home district. During our excursion we visited a school for aboriginal and half-caste children. As we left this our President remarked: "At last I have seen Palæolithic man"—it is not possible to use terms more apposite as a description of the Australian aboriginal. The Association came into contact with natives at various other times and places. They are fully described by Prof. Baldwin Spencer, Special Commissioner for Aboriginals, in the invaluable handbook presented to the members of our body by the Federal Government of Australia; his article is richly illustrated and of remarkable interest. It is probable, Professor Spencer says, "that with the exception of one or two isolated groups, they represent the most backward race extant and in many respects reveal to us the conditions under which the early ancesters of the present human races existed." Since the present race has inhabited the continent, they have never had to contend with any higher race nor with dangerous animals, hence the low development of their intelligence, in all probability.

The natives are very carefully protected and to destroy them is a penal offence; they now play but a small part in the country and mostly live by themselves in protected areas; in the west and also in Queensland, I believe, they help on the sheep and cattle runs. In 1911 there were about 20,000 in the Northern Territory, about 4000 in South Australia, perhaps 2000 in New South Wales and only a very few hundred in Victoria.

As there is no railway connecting Perth in Western Australia with Adelaide, South Australia, a distance of about 1500 miles, we all journeyed by steamship across the great Australian Bight, a region which enjoys but an indifferent reputation among seamen; our party was exceptionally fortunate, as the weather was calm throughout the five days' passage. As we passed southwards down the coast, near to Albany, though we were several miles from the shore, we could see giant trees growing at the edge of the high cliffs—part of the great Jarrah and Karri forest from which the material for paving our London streets is obtained. Wood to the value of about a million sterling was exported from this region in 1910. The forest covers a wide belt—the timber-belt—at the south-west corner of the continent, where the rainfall is considerable; as this timber-belt is cleared it becomes available for settlement and is a land of rich promise, well suited for growing oats, potatoes, root crops and especially apples of high quality, of which a large quantity is now exported to Europe. The conditions to be faced by the first settlers are not easy. The cleared forest-land is said to be five or six times the value of that within the wheat-belt, which lies behind the timber-belt, within the region in which there is a rainfall of only 10 to 13 inches; this is already well served with railway facilities and with another necessity peculiar to Australia—the rabbit fence. Beyond the wheat-belt comes the pasture-belt, which extends well outside the 10-inch rainfall limit into the desert area. The term desert, however, must not be taken too literally. One of our party, a German, the authority on deserts, who visited the region as well as

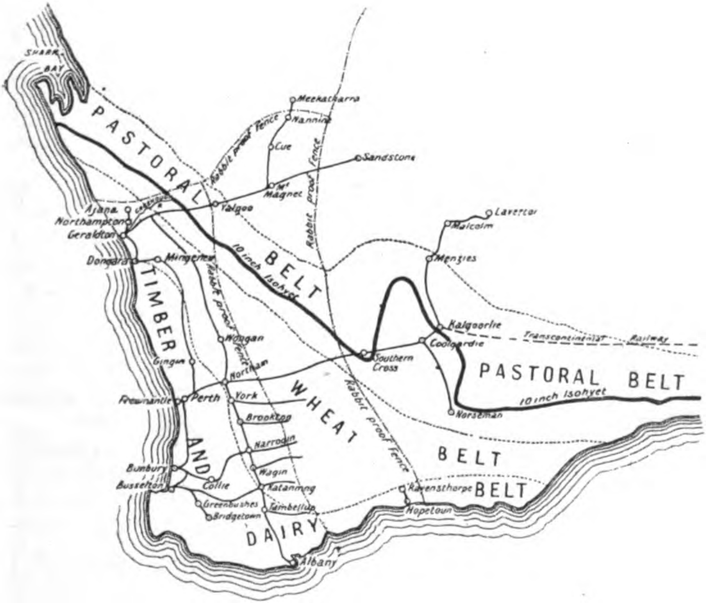

SHARK BAY
PASTORAL
BELT
10 inch Isohyet
Rabbit proof Fence
Meekatharra
Nannine
Cue
Sandstone
Mt Magnet
Yalgoo
Ajana
Northampton
Geraldton
Dongara
Mingenew
TIMBER
AND
Wongan
Gingin
Northam
Fremantle
Perth
York
Brookton
Narrogin
Wagin
Katanning
Bunbury
Busselton
Collie
Greenbushes
Bridgetown
Tambellup
DAIRY
Albany
WHEAT
BELT
Laverton
Malcolm
Menzies
Kalgoorlie
Coolgardie
Southern Cross
Transcontinental Railway
PASTORAL BELT
10 inch Isohyet
Norseman
Ravensthorpe
BELT
Hopetoun

Fig. 3.—South-West Australia.

other desert regions in Australia, told us that there is no true desert on the continent. Kalgoorlie, the most recently established and one of the most remarkable of Australian mining towns, lies just inside the margin of the desert, about 400 miles west of Perth. The region in which it is situated was uninhabited waste until 1887, the year in which the Southern Cross goldfield was discovered; the Coolgardie field was opened in 1892 and the Kalgoorlie field a year later. The prosperity of West Australia dates from this period. But Coolgardie is already a deserted town, while Kalgoorlie flourishes; it has very broad streets and some good houses and about thirty thousand inhabitants, nearly as many as have all the other towns in the district put together. At first gold was picked up on the surface but it is now won from deep mines. Up to the end of 1913 the value of the gold produced in West Australia was about 115 millions sterling; nearly one-third of the total mineral production of all Australia, in fact, is to be credited to the western State. The average value of the gold produced per man employed above and below ground in 1913 was £403·58.

In early days, the water used was all distilled from the salt water collected in local lakes or reservoirs and the old air condensers are everywhere visible. Since 1903, however, water has been brought from a great reservoir, constructed near Perth, in a pipe line 33 inches in diameter, 376 miles long, rising 1460 feet. The water is raised at pumping stations about 40 miles apart, at each of which it is elevated 140 feet; it takes eleven days to get up. When the service was completed to Coolgardie, the town for which it was originally intended, the goldfield was already worked out but fortunately by that time the Kalgoorlie field had been opened up; local opinion does not seem to give this a long life. Visitors to Kalgoorlie tell me that nothing impressed them more than the arrangements made to supply water to the district; more than 50,000 people and the mines, in fact, depend for their very existence on the pipe. At starting from the town in the train in the afternoon the pipe is alongside the track; after a long night, it is still to be seen in the morning—and practically the whole time the train has been passing through desert on which grows a short dismal-looking vegetation, mainly blue bush and salt bush and a few stunted "gum" trees. In early days great difficulties were encountered owing to the rusting of the iron main.

The northern area of Western Australia, in so far as it is settled, is devoted to sheep and cattle; future settlement seems to be almost entirely a question of water supply.

On arrival off Adelaide, on Sunday, August 2, our advance party was greeted by the news of the declaration of war by Germany against France and Russia and our own participation was formally declared three days later. We saw prizes in the harbour before we left. The main body arrived on the following Friday and Saturday. At an emergency meeting of members of the Council, on the Saturday afternoon, at which we considered what was desirable in the altered circumstances, it was resolved that "a communication be sent to the Federal Council expressing the desire of the Members of the British Association to recognize in every way the special difficulties which might attend some of the arrangements made for the Meeting, owing to the unforeseen situation in Europe and their willingness to acquiesce in the withdrawal of any item from the programme which might seem inappropriate under the circumstances."

A reply was at once received from Professors Masson, David and Stirling, as official representatives of the Federal Council and the local executives, informing the Meeting that "while appreciating the thoughtfulness exhibited by the resolution, they felt strongly that the scientific and other business of the Meeting should go on as it would have done under happier circumstances but that some of the social functions might have to be modified in such a way as to meet the general feeling and, moreover, that if any difficulties of transport or other matters should interfere with or modify or delay any of the excursions arranged, they were glad to know that the Members of Council present realized the possibility of such difficulties and would make the needful allowances."

After discussion, we decided to treat our German guests as though nothing had happened, as they could not be held responsible. The agreement was loyally observed throughout the Meeting and neither side, I believe, had reason to complain of lack of cordiality, except in the case of one individual. I know that our attitude towards them has produced a deep impression upon some of our Teutonic companions and in course of time it may be of service, perhaps, in justifying the compliment paid to us by one of our foreign members since his return, in the quaint remark, "I am glad to see one people ever alike and ever with cold brain."

Though a reception was held by the State Governor in Adelaide and several lectures were delivered by members of our party, except that Presidential addresses were read in two sections, the Association did not formally begin its regular sectional work until we reached Melbourne, where the President read the first part of his address; the second part was delivered at Sydney, where the regular proceedings terminated, though two sectional addresses and three evening lectures were delivered subsequently in Brisbane.

The organization for the mobilization of our party of three hundred was remarkably complete. We were severally provided with a diary and memorandum book containing a list of the party, a number being assigned to each of us to be used on baggage; and on the occasion of each new journey we received an information list giving full particulars as to place in train, name of host, etc., so that we were able to fall in on leaving and arriving at the various centres almost without asking a question. Perhaps the most agreeable feature of our visit was the fact that the majority were entertained privately, which gave us the opportunity we so much desired of coming into intimate contact with residents ; the hospitality afforded us by the chief clubs is also to be mentioned with particular gratitude, as in these circles especially we were often brought into direct contact with the informed members of the business and professional community in the various cities. A mass of most valuable literature was placed at our disposal officially.

To convey us from Adelaide to Melbourne, a distance of 482 miles, three trains were provided, on which there was "sleeping" accommodation for all. We left Melbourne for Sydney, a distance of 583 miles, in one train, then changed at the State boundary, where the gauge is broken, into three in which we passed the night in comfort. Two trains sufficed to convey the diminished party which travelled from Sydney to Brisbane, a distance of 725 miles; in this journey also we changed trains at the State boundary.

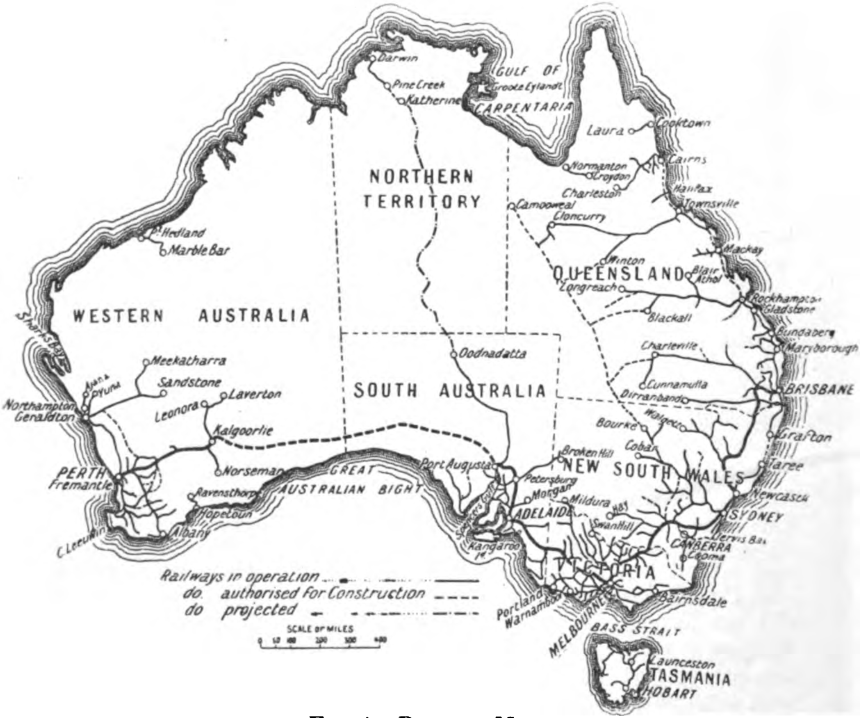

Darwin
Pine Creek
Katherine
GULF OF
Groote Eylandt
CARPENTARIA
Laura
Cooktown
Cairns
Normanton
Croydon
Charleston
Halifax
Townsville
Camooweal
Cloncurry
NORTHERN
TERRITORY
Mackay
Winton
QUEENSLAND
Blair Athol
Longreach
Rockhampton
Gladstone
Blackall
Bundaberg
Maryborough
Charleville
Cunnamulla
Dirranbandi
BRISBANE
Walgett
Bourke
Cobar
Grafton
Broken Hill
NEW SOUTH WALES
Taree
Newcastle
SYDNEY
Jervis Bay
CANBERRA
Cooma
Mildura
Swan Hill
VICTORIA
Bairnsdale
MELBOURNE
Portland
Warrnambool
BASS STRAIT
Launceston
TASMANIA
HOBART
Oodnadatta
SOUTH AUSTRALIA
Port Augusta
Petersburg
Morgan
ADELAIDE
Spencer's Gulf
Kangaroo I.
GREAT
AUSTRALIAN BIGHT
P. Hedland
Marble Bar
WESTERN AUSTRALIA
Shark's Bay
Meekatharra
Sandstone
Laverton
Leonora
Kalgoorlie
Northampton
Geraldton
Pyura
PERTH
Fremantle
Norseman
Ravensthorpe
Hopetoun
Albany
C. Leeuwin
Railways in operation
do. authorised for Construction
do projected
SCALE OF MILES
0 50 100 200 300 400

FIG. 4.—RAILWAY MAP.

These breaks of gauge are the one blot on the railway system of

Australia, which is in other respects admirable. The cost of converting all lines on the continent to a 4 ft $8\frac{1}{2}$ in. gauge has been estimated at about 37 millions sterling. Up to the 30th June, 1912, only 944 miles out of a total distance of 17,842 miles of railway open for general traffic were under private control; up to the present time, 160 millions sterling have been expended on the Government lines.

The outbreak of war necessarily affected our proceedings most seriously, as public attention was centred upon that topic; unfortunately also, the Parliamentary elections were close at hand. Nevertheless, in addition to our home party, about 4700 local members joined the Association, so that the meeting was the largest on record.

The public lectures (of which at least a dozen were given) were well attended and evoked much interest. The proceedings in not a few of the sections were also well supported and often gave rise to serious and sound discussions on topics of special local importance. The press, on the whole, treated us very liberally. So that, even regarded from a technical point of view, the meeting was a complete success and far more was done than is usually done at our meetings to bring the general public into touch with the Association. Foundations were laid which we may hope will be built upon in future; thus, the second part of our President's Address, delivered in Sydney, was a most weighty pronouncement of direct public importance such as has never been delivered hitherto by a president of the Association, as attention was called to the problems of heredity in terms which could not be misunderstood even by a general audience. It is deplorable that no notice has been taken of the address by the public press at home.

A deputation of astronomers was received by the Prime Minister of the Commonwealth with reference to the establishment of a solar observatory at Camberra, the future capital of the Commonwealth. They were assured of his sympathy, though no promise could be given under the conditions then prevailing.

We were unable to give more than a cursory glance at the life and manners in the course of our rapid passage across the Continent. It is not going too far to say that, one and all, we felt at home at once: the welcome was so warm and the surroundings so absolutely British. Some of us, I think, had the feeling that we were back in Victorian England and that the background was in some way distinctly simpler and more serious than that we had left at home. The Australian, though confident, is generally free from bounce; he believes, as he has a right to believe, in the future of his country. But perhaps he has been, if he be not still, a little lacking in outlook; we have to remember, however, that though an Imperial people, we are all of us too much afflicted with the vice of parochialism for an

Imperial people. The Australian Government is now in a peculiar position—practically in the hands of men who represent the interests of only one special section of the community—the labour section; bearing in mind the complexity and difficulty of her problems, this cannot be to the advantage of the country in the long run and it is to be hoped that a broader policy will prevail eventually. We found everywhere a deep and growing sense of patriotism; Australia seems, indeed, to be now fully aware that it is vital that we should hold together and row together.

She is bent, however, on making the experiment—a vast experiment—of remaining a white man's country, notwithstanding the great scarcity of labour and the great relief an influx of cheap coloured labour would bring. At present only about two per cent of the population is non-British in origin.

This is the crux of her situation. From the point of view of our President's Address, the action she takes in forming a race is a matter of absolutely vital consequence and her action should be guided, if possible, by the dictates of science. She will need a great influx of ability to form a strong race. The problem is one that concerns all countries that are formed by colonizing.

Will she prove that the white man can deal successfully with tropical conditions? Much will depend on the attitude, on the reasonableness, of the labour party; more, perhaps, on the scientific study of the problem. After discussing the subject with natives in India recently, I have been led to think that coloured people are almost as much affected as we are by extreme conditions of heat and no more inclined for work during hot times; the advantage lies with them mainly because they are grown accustomed to the conditions and have learnt how to accommodate themselves to them better than we have: they work very early in the morning and in the evening, even at night and can be quiet in the hot daytime: it rests with us to imitate such native methods more fully than we have done in the past; but we shall never be so easily satisfied, as we cannot escape the penalty of our more highly developed individuality and we have yet to learn if it be possible to run our race under tropical conditions. As a matter of fact, it has been proved in Queensland that whites can work there effectively and they have done the work of twice the number of coloured labourers—but at a much greater cost; unwillingness to allow cheaper labour to compete is at the bottom of the question.

The Australian towns are much like our towns and not yet affected by the skyscraper, so fashionable in America; but they have about them much of the hideous litter of telegraph and telephone posts which often disfigure the streets of American and Canadian towns, though pills do not spoil the landscape. In Melbourne, particularly, the streets are surprisingly wide, a relic of the time when great droves of sheep were passed across the country.

Australia has a population a little over 4½ millions; the density of the population is only 1·57 per square mile, as against 30·93 in the United States of America and 117·13 in Europe.

The serious feature is the accumulation of a large section of the population in a few large towns, nearly half being in the five chief cities of the five States. In view of the great dearth of labour on the land, the large number of residents in a town such as Sydney can only be accounted for on the supposition that the conditions back in the country are so strenuous that many cannot stand them, yet these townsfolk must be living largely on the country, directly or indirectly.

The distribution of industries is indicated in the following table:—

ESTIMATED VALUE OF COMMONWEALTH PRODUCTION, 1911.

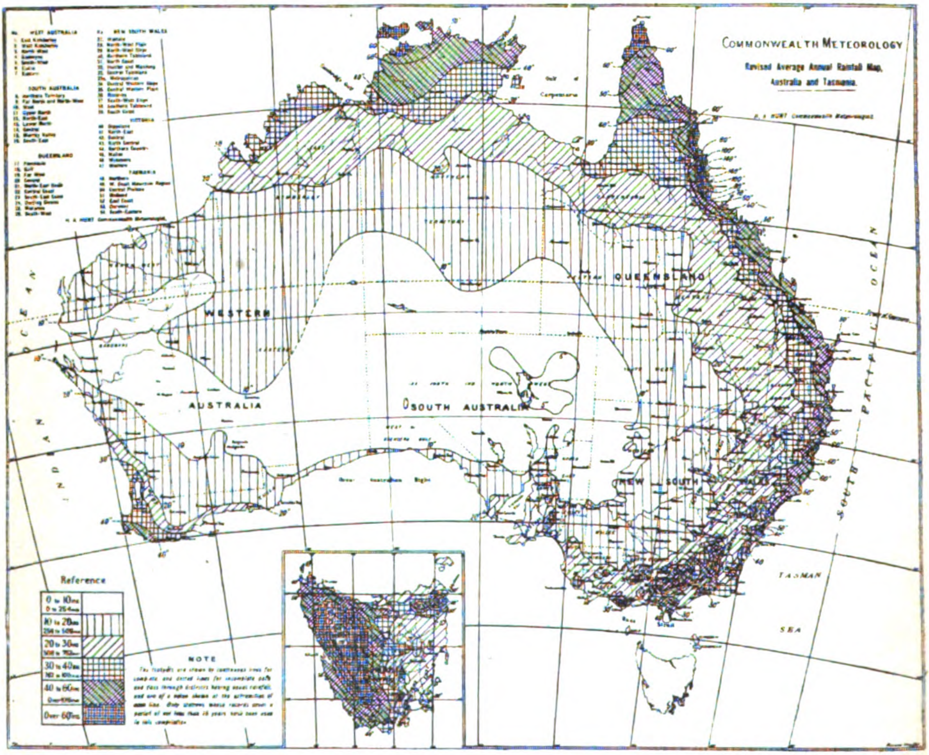
Agriculture	£38,774,000
Pastoral Industry	50,725,000
Dairying and Poultry Farming	19,107,000
Forest and Fisheries	5,728,000
Mining	23,480,000
Manufacturing	50,931,000
Total	£188,745,000

What is included under "Manufacturing" I cannot say but the item is a surprisingly large one, taking into account the high price of labour and the statement constantly made that capital is not flowing into manufacturing enterprises, on account of the difficulties connected with labour.

I have already remarked that Australia offers peculiar problems. The climatic conditions are altogether special—these dominate everything. Except near the mountains, it has a low rainfall; as it is not a mountainous continent—the highest peak is Mount Kosciusko, 7308 ft. in altitude, in the south-eastern part of New South Wales, 220 miles south-west of Sydney—the regions in which the rainfall is at all considerable are few and far between. Over a large inland area the amount is less than 10 inches per annum and the area within which the rainfall does not exceed 20 inches is of astounding proportions. The mountain regions all lie near to, if not within, the coastal region. This will be obvious when relief maps of the continent are inspected (Figs. 7 and 8). The western half is mainly a plateau region from 1000 to 1500 ft. high; the eastern half has high land at its southern and eastern rims, whilst the inland portion is a great basin.

On the whole Australia is a hot country, as the winters are not at all cold even in the extreme southern region, palms growing out of doors in Adelaide; but the heat is a dry heat in most parts and therefore it is bearable and very different from that experienced in countries like Java or Ceylon, for example.

Agriculture is necessarily greatly affected by the climatic conditions. On account of the scarcity of rain special measures for conserving moisture in the soil are now adopted. Thus in the Adelaide region, in which much wheat is grown, a system of dry farming prevails which involves either placing the land under wheat one year and under bare fallow the next or the wheat year is followed by one in which weeds are allowed to grow up together with the large quantity of wheat spilt during harvesting by means of the


Commonwealth Meteorology
Revised Average Annual Rainfall Map, Australia and Tasmania.
WESTERN AUSTRALIA
QUEENSLAND
SOUTH AUSTRALIA
NEW SOUTH WALES
INDIAN OCEAN
SOUTH PACIFIC OCEAN
TASMAN SEA
Reference
0 to 10 ins
10 to 20 ins
20 to 30 ins
30 to 40 ins
40 to 60 ins
Over 60 ins
NOTE

McCarron Bird & Co. Printers Melbourne

FIG. 6.—RAINFALL MAP OF AUSTRALIA.

stripper, which merely takes off the ears; the weeds are fed off and then follows a year of bare fallow. The land is cultivated sufficiently often during the year of bare fallow to prevent any growth upon it and the upper layer of soil is kept open so as to minimize the loss by evaporation; in this way a fair proportion of the rain which falls during the year is conserved in the subsoil.

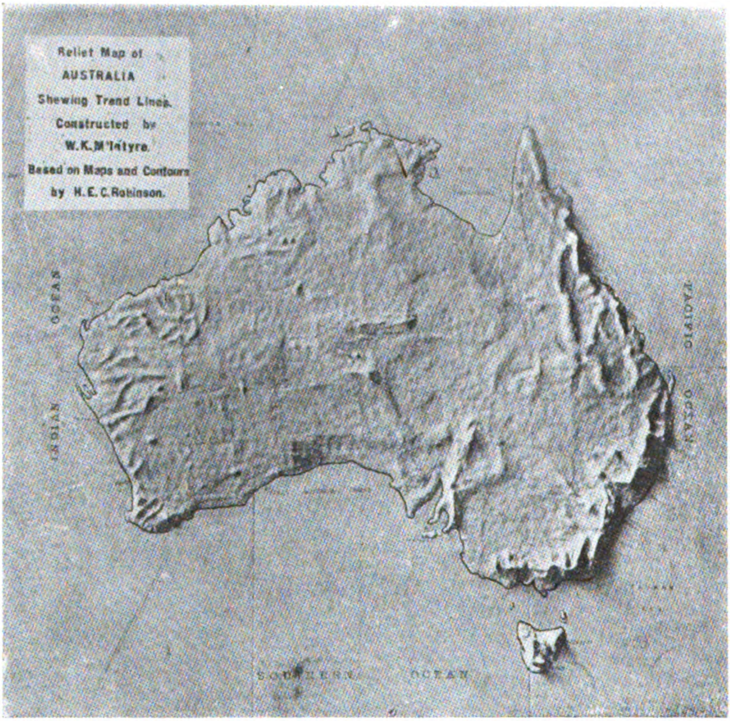

Relief Map of
AUSTRALIA
Shewing Trend Lines.
Constructed by
W.K.M'Intyre
Based on Maps and Contours
by H.E.C.Robinson.
INDIAN OCEAN
PACIFIC OCEAN
SOUTHERN OCEAN

FIG. 7.

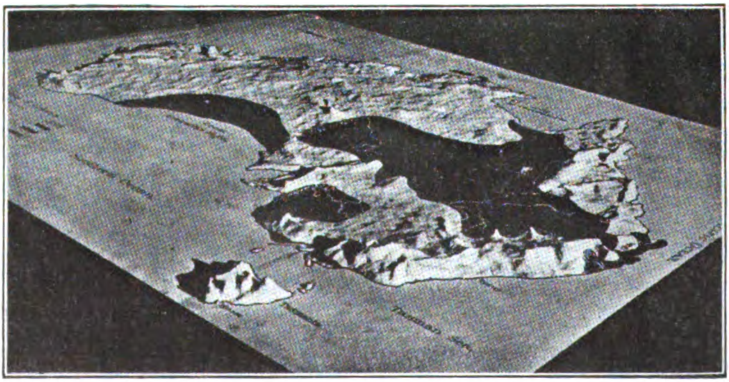


FIG. 8.

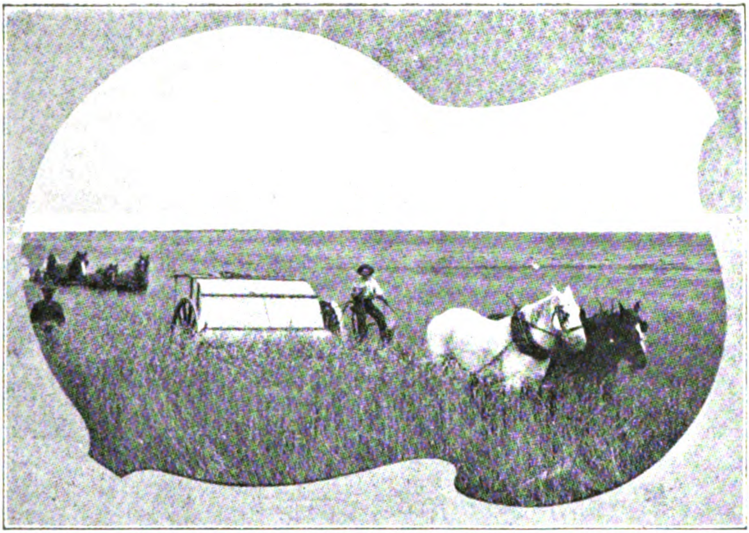


FIG. 9.—WHEAT-STRIPPING.

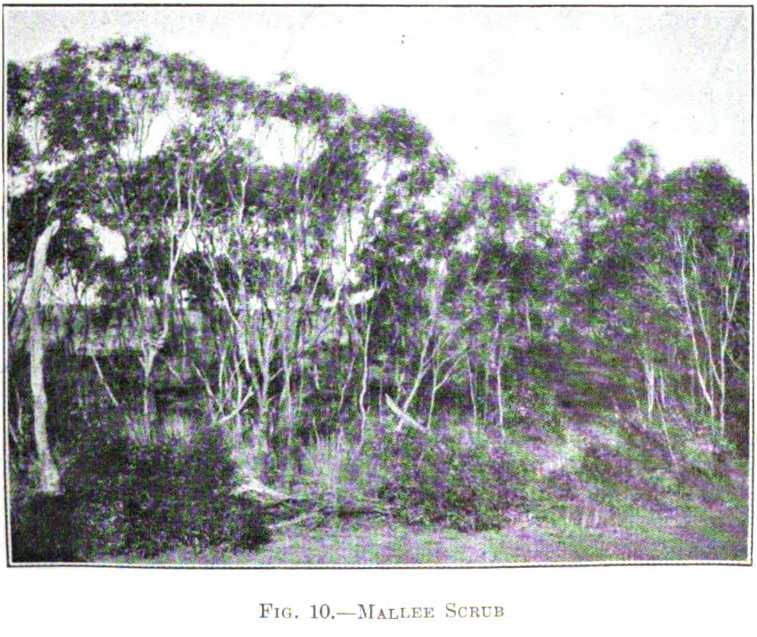


FIG. 10.—MALLEE SCRUB

One result of the bare fallow system is that the wheat country in South Australia and in other regions where similar soils prevail has a very beautiful aspect, owing to the contrast the rich red colour of the bare soil affords with the green of the adjacent fields on which wheat is growing. The effect is very like that to be seen in South Devon. The soils referred to are the so-called *Laterites*, containing a large proportion of iron oxide, which prevail over a large area in Australia; such lateritic soils are formed apparently by chemical concentration at the surface of iron oxide from the underlying weathered rock. The laterites are poor soils agriculturally and do not respond to any artificial manures whatever except phosphates, of which they contain a very small proportion. Altogether surprising results have been obtained in various parts of Australia by the application of quite small amounts of a phosphatic manure. Land to the east of Adelaide, in the region known as the 90 mile desert, the so called Mallee country, which was formerly deemed useless, is now being brought under cultivation, as it is found to yield fair crops of wheat after treatment with superphosphate. It is impossible to raise a variety of crops in rotation under the conditions referred to and this is true of the wheat lands generally, I believe; but under irrigation such lands yield astonishing crops—I saw a field at Roseworthy, near Adelaide, on which Egyptian clover was grown luxuriantly and was told that as much as 36 tons per acre had been cut.

To obtain a supply of water within the dry areas, artesian wells have been sunk at great expense all over the continent. The basins which within such wells are operative are indicated in the map Fig. 11. The character of these basins may be illustrated by a section across a line drawn from the Gulf of Carpentaria southwards to the Great Australian Bight (Fig. 12).

The question of the origin of artesian waters has given rise to much discussion and is a problem of great importance. The head has fallen one-third in the course of the last five years. There appears to be little doubt that the water is largely derived from rainfall but it is held by Professor Gregory, in particular, that some may be of plutonic origin—that is to say, that changes are going on in the deep-seated rocks which lead to the separation of chemically combined water; the injection of this deep-seated water, which is probably superheated, causes a gas pressure in the water of cisternage which forces it to the surface when it is reached by boring. The intake beds are estimated to cover an area of 60,000 square miles in Queensland and 10,000 in New South Wales; the total length of bores sunk in the latter State is 158 miles.

The character of the outflows will be obvious from the slides now shown. But there are various difficulties connected with the artesian water supply. In certain districts the water rapidly corrodes the iron or steel pipes with which the bore is lined and they become useless after a few months or a year or so. The corrosion does not always take place throughout the whole length of the pipe but more or less locally; and it has happened in several cases that a water which did not corrode the pipe in a well of considerable depth became corrosive when the bore has been carried to a greater depth. There can be no doubt that corrosion is due to the presence of carbonic acid, together with oxygen, in the water but we shall have to discover why pipes behave so differently. It is surprising, considering the great importance of the problem, that no systematic inquiry has ever been promoted by the iron trade. In not a few cases artesian water contains such a proportion of salts that it cannot be used by animals; and even if it can be so used, in some cases, when applied to the soil, owing to its alkalinity, it causes the soil to cake, so that this becomes waterlogged when flooded.

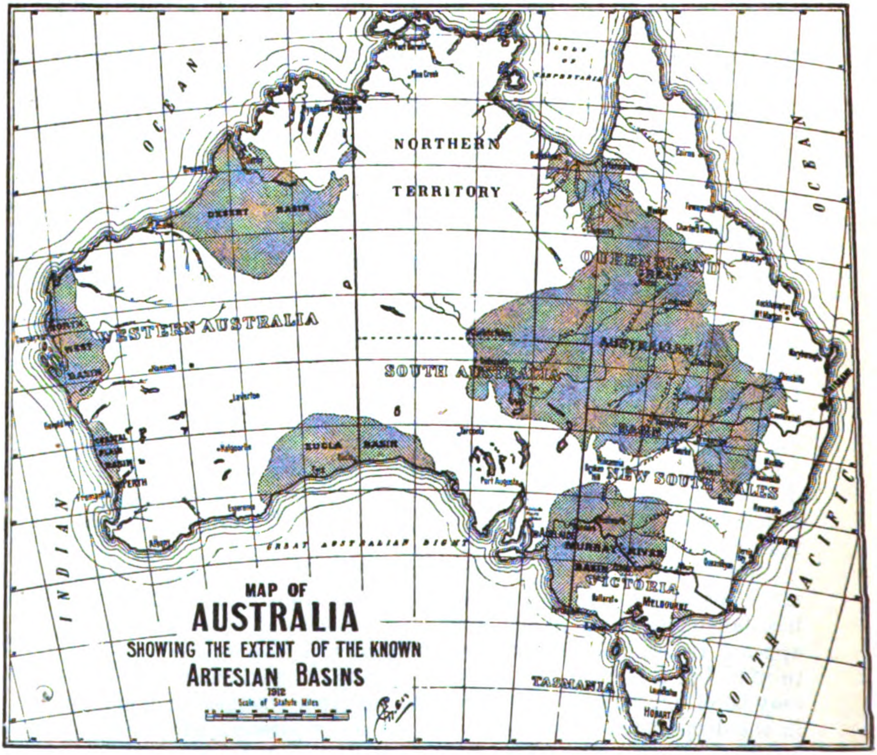

MAP OF
AUSTRALIA
SHOWING THE EXTENT OF THE KNOWN
ARTESIAN BASINS
1912
Scale of Statute Miles
INDIAN OCEAN
OCEAN
SOUTH PACIFIC
OCEAN
GREAT AUSTRALIAN BIGHT
NORTHERN
TERRITORY
DESERT BASIN
WESTERN AUSTRALIA
NORTH WEST BASIN
COASTAL PLAIN BASIN
PERTH
SOUTH AUSTRALIA
EUCLA BASIN
Port Augusta
QUEENSLAND
GREAT
AUSTRALIAN
BASIN
NEW SOUTH WALES
SYDNEY
MURRAY RIVER BASIN
VICTORIA
MELBOURNE
TASMANIA
HOBART

FIG. 11.

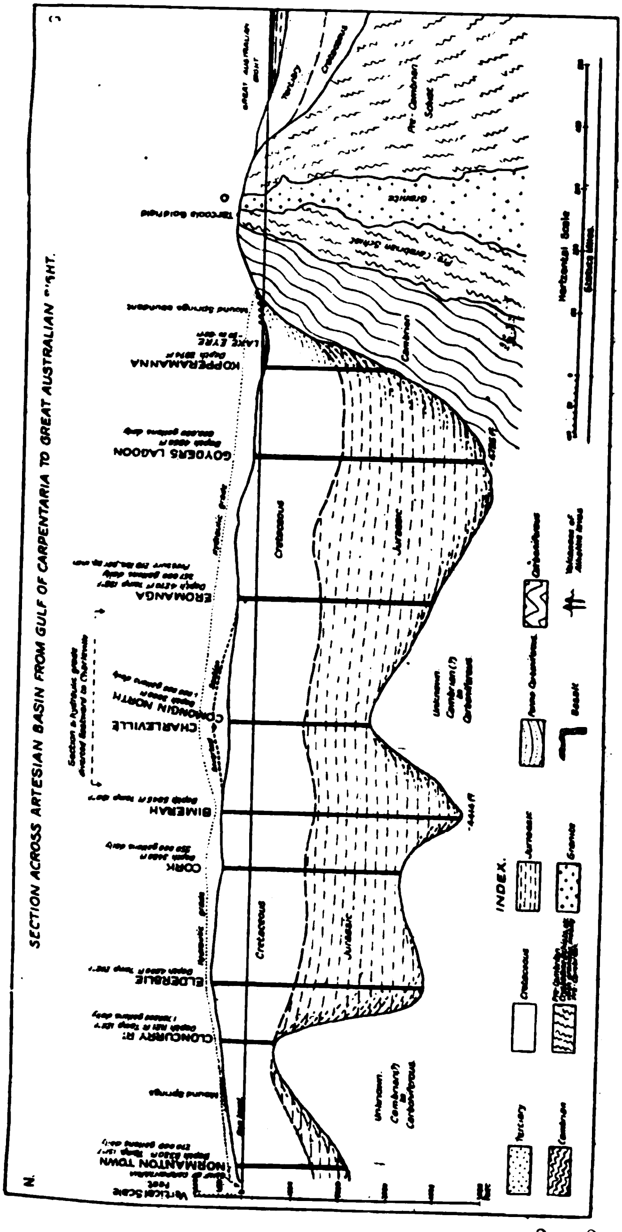

SECTION ACROSS ARTESIAN BASIN FROM GULF OF CARPENTARIA TO GREAT AUSTRALIAN BIGHT.
N.
Vertical Scale
NORMANTON TOWN
Mound Springs
CLONCURRY R.
ELDERSLIE
CORK
BIMERAH
CHARLEVILLE
CONONGIN NORTH
Section is hydraulic grade diverted eastward to Charleville
EROMANGA
GOYDERS LAGOON
KOPPERAMANNA
LAKE EYRE
Mound Springs
Tarcoola Goldfield
GREAT AUSTRALIAN BIGHT
Cretaceous
Jurassic
Unknown Cambrian(?) to Carboniferous
Cambrian
Pre-Cambrian Schist
Granite
Tertiary
Cretaceous
Hydraulic grade
INDEX.
Tertiary
Cambrian
Cretaceous
Pre-Cambrian
Jurassic
Granite
Permo-Carboniferous
Basalt
Carboniferous
Volcanoes of Mesozoic times
Horizontal Scale
Statute Miles

FIG. 12.

It is impossible in the time at my disposal to deal with sheep and cattle farming. Our visit was made in the winter and during a period of drought. The pasture land bore a strange aspect when we saw it, as there was practically nothing growing on much of it and we had difficulty in understanding how the poor beasts could keep body and soul together but they clearly did. We were told that, in some districts, the sheep had learnt to grub up the wild carrot. Land in the interior may carry only a sheep to ten acres, owing to the small amount of pasture it affords but at times it has great fattening power. The characteristic of the vegetation is that it is sparse and stemmy, only near the sea is there what a grazier calls a sole of grass. Large areas have been ruined by the introduction of weeds—stink weed (an *Inula*), from Central Europe, tulip grass and sour sod (an *Oxalis*), from the Cape. The Globe Artichoke, which follows all the stream courses in S. Australia, provides passable fodder in droughts. In the very dry interior, the grazing is chiefly salt bush. A feature of the plains within the dry area is the marvellous rapidity with which recovery is made from a drought. During a dry period, the black soil plains especially may become reduced to dust with not a vestige of herbage for miles, though shrubs and trees, many species of which are valuable as fodder, may remain alive. Within a week after rain has fallen all this is changed and the country becomes covered with a green mantle which in a few weeks furnishes luxuriant pasture.

The great sheep runs of the past within the areas of moderate rainfall are now being cut up into farms on which mixed farming prevails, each carrying only a small number of sheep.

We had the opportunity in Queensland of seeing sugar grown and manufactured. The industry has flourished in the past with the aid of cheap coloured labour but it is difficult to believe that its prosperity can be maintained now that such labour is not available. Judging from what I have seen of the industry in Java and in the southern region of the United States, I am definitely of opinion that successful competition with such regions and the West Indies is scarcely to be contemplated and that it is undesirable to attempt to produce sugar if the land can be used otherwise with advantage. The public money spent in subsidizing the Australian industry may probably be better used in other ways—for instance, in promoting the clearing of land for occupation by new settlers.

One other agricultural product of Australia should be mentioned—its wine. Here we are acquainted only with a cheap variety used by invalids and Australian wine does not rank among experts. Our visits to vineyards in West and South Australia convinced some of us, however, that wines of high quality, both red and white, are being produced which compare favourably with those met with in the course of travel in Europe; it was a revelation to us that it was possible to obtain at moderate cost good wine of British origin on British soil.

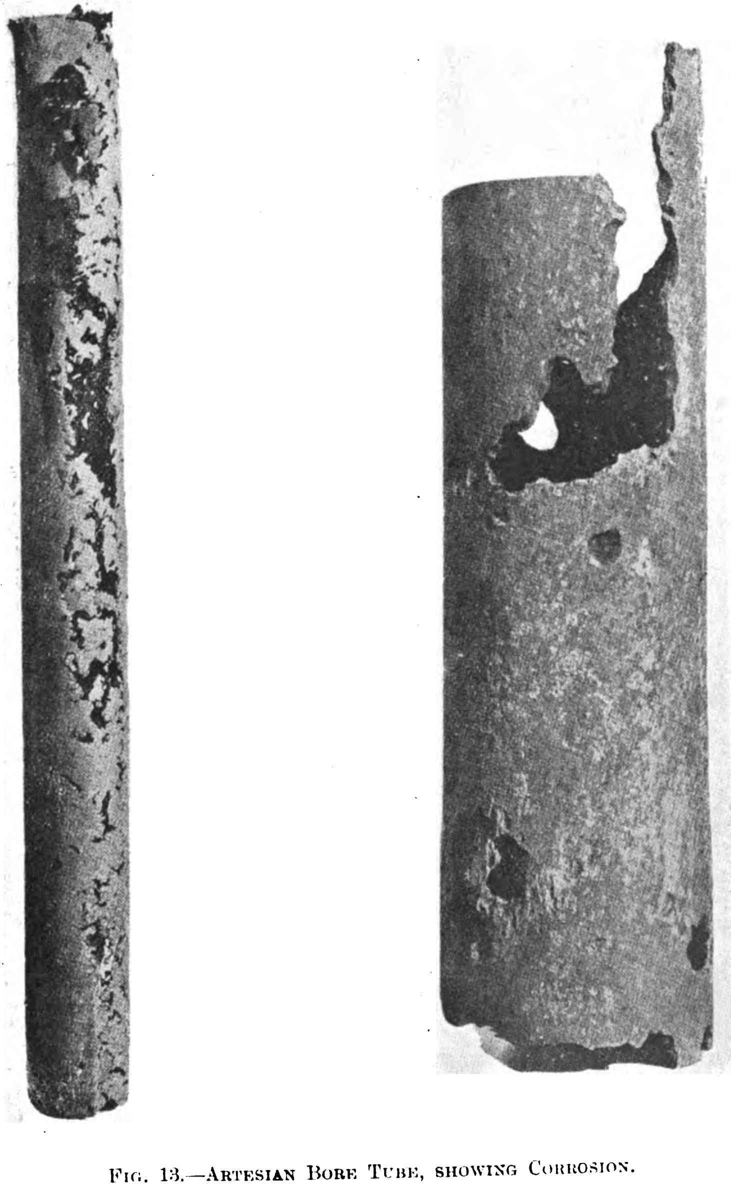


Fig. 13.—Artesian Bore Tube, showing Corrosion.

Though fortunate in many ways, Australia is a country sore afflicted by foreign weeds; in fact, we constantly come across familiar wild plants. Cape weed greeted us in West Australia; and a yellow *Oxalis*, also from the Cape of Good Hope, a well-known Mediterranean pest, occupied a prominent place in the landscape near Adelaide, growing as it did in great sheets in the almond orchards, which are a feature of the district; the brilliant green foliage and yellow flower of the weed formed a carpet in striking contrast with the pink flower of the trees growing out of it. In Queensland, we were struck by the extraordinary development attained to by *Lantana* (*L. camara*), chiefly the yellow form, which seemed to take the place the blackberry does here, growing luxuriantly as a big bush everywhere at the sides of clearings. I afterwards came across it in Java, Ceylon and India. Here we know it only as a half-hardy plant, which flowers but scarcely grows, in the open during summer. But the great pest of Eastern Australia is the prickly pear. It is said this was first planted out at a station in Queensland forty-five years ago and that forty-three years ago a man was discharged for not watering it; now there are about 2,500,000 acres of pear-infested country in New South Wales and the cost of eradicating it was put, two years ago, at ten or twelve millions sterling. It is stated that in Queensland 30,000,000 acres are affected and that the spread is 1,000,000 acres per year. It is carried further afield by birds and stock, which eat the seeds; as every joint, or portion of one, which drops off, forms a new plant, it must be killed entirely if it is to be got rid of. It flourishes most luxuriantly under the partial shade of the stunted forest trees which form the scrub over the dry lands. The Queensland Government have established a special experimental station to study methods of destroying the pear at Dulacca, on the

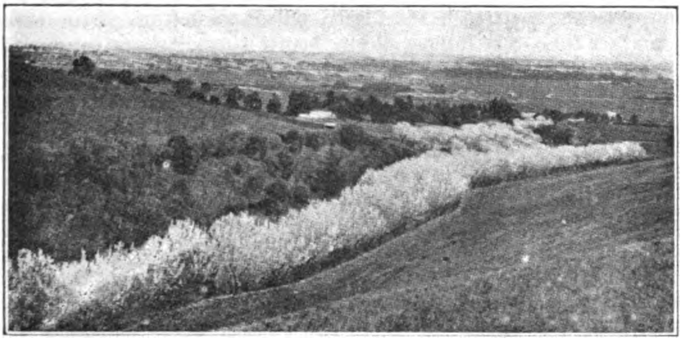


FIG. 14.—ALMOND GROVE IN BLOSSOM.

margin of the Darling Downs, about 260 miles west of Brisbane, under the chairmanship of Dr. Steele, the Professor of Chemistry at Brisbane University. I had the good fortune to visit the station with Professor Steele and Dr. Jean White, a lady botanist who is in charge of the experiments that are being undertaken. An American, Mr. O. C. Roberts, has also been making experiments on a large scale in the district with the aid of a company which he has established and has obtained a concession of 200,000 acres, subject to conditions as to clearing the ground within a certain period. After many trials, Mr. Roberts has developed a treatment destroying the plant which only an American would dare to put into action. He boils off the vapour of arsenious chloride, in quantities up to four tons at a time, across country when a favourable wind prevails. The vapour does not seriously affect the Eucalyptus but wherever it is absorbed by the prickly pear, the plant is killed after a time; the objection to the method is the highly poisonous nature of the vapour and the fact that it is impossible to bring it everywhere into contact with the plant. A great variety of experiments have been carried out at the experimental station and the results show that the use of a particular arsenic compound in the form of spray is likely to be effective. Apparently, when the compound once enters the plant, it spreads from joint to joint and ultimately reaches the root, killing the whole growth; the dried remains can then be burnt *in situ*. It is proposed to extract potash from the ashes but in view of the scarcity of potash manures this is a policy to be deprecated. The prickly pear has also over-run parts of India; I saw it growing thickly among the trees covering the hill slopes as I passed up the Eastern Ghats.

While speaking of imported weeds, I may point out that, perhaps, the most serious pest in Australia is the wild rabbit—as proof, it is sufficient to point to the long line of rabbit fencing shown on the map of Western Australia; there are 10,000 miles of such fencing in Queensland.

Epiphytes also play a great part as pests. The Eucalypts are infested by a *Loranthus*, a kind of misletoe, which is their deadly enemy.

The opportunities awaiting science, in agriculture and education especially, are very numerous; but like our own, the Australian public are in no wise alive to the service it can render to the State. The point of view is mainly literary and their scholastic opinion, like ours, has yet to be persuaded into right lines—to be made practical and in harmony with the times.

To conclude—in days gone by, not so very long ago, it was customary for our gilded youth to make the grand tour of Europe as a part of their general education; now travelling is no longer confined to the gilt class but is open to all who have sufficient gold. Unfortunately, it is no longer a recognized duty, even of the wealthy, as a part of their education—but it should be. I would urge all young people to travel, widely and imperially; but while visiting the outlying regions of our empire, to study other lands as well. It should be a necessary qualification, in our House of Commons, that a man had learnt to know something of the world before entering there and that he should continue to study it while a Member.

To me the visit to Australia was of absorbing interest, not merely in itself but because I had previously learnt to know Canada and the United States, regions which, in some respects, are far less interesting naturally; and especially because I was able subsequently to study, however superficially, Java, as well as our own possessions, Ceylon and India, so that I could contrast the ancient, unfathomable civilization of the East with our own more recent, superficial development.

The comparison, I fear, cannot be made with entirely satisfactory results. In Australia the labourers' wages may be fixed by law at as high a sum as ten shillings per day; in Java they probably never exceed sixpence.

While staying at an outlying sugar plantation in Java, one evening, after dark, we sat on the verandah lit by electric light from the adjoining factory and listened to the performance of the village orchestra or *Gamelang*. Besides a drummer, who beat time, there were half-a-dozen performers each of whom had a rude instrument, consisting either of a wooden trough across which a number of strips of metal were fixed or simply of a wooden box, perhaps one that had carried groceries from Europe, across the open top of which a couple of rough plates of metal were slung—the performer had one or a pair of hammers with which he hit the metal plates. Each instrument was tuned. Each player played his part, so the melody was complex. Our hostess had provided for a deft rehearsal previously, by one of the women from the village, of their method of drawing patterns in hot molten wax, preparatory to dyeing it, on the cotton slip used as chief garment—the *Sarong*—and we had seen admirable specimens of the finished work. All the household, together with a number of outsiders, squatted on their haunches around the performers, listening with rapt attention to the music; the while the factory watchman wandered up and down with drawn sword. Clad in colour as all were, with the moon shining on the distant timbered slopes of a volcanic mountain, the weird scene was one of strange charm and beauty—the music indescribable but entirely befitting the surroundings. When we heard that such performances were the staple form of enjoyment in the compound, my friend (a Swede) and I could not help giving expression to the feeling that the æsthetic plane on which these poor folk stood was certainly not below that we were accustomed to rate so high. And yet we are seeking to compass the ruin of their system. The Dutch are now introducing Western methods of education into Java and Western teachers, without understanding, are perverting Eastern ideals; we are following the same mistaken course in India.

These are the things that attract the attention of the traveller and give cause for reflection; more, therefore, should travel and reflect.

We of the British Association now have cause to glory in the fact that we are a chartered body of wanderers, with full right to an Imperial title. In the future it will be our duty, I believe, to take ourselves still more seriously and assume the office of incitors and co-ordinators of the scientific activities of all regions of our Empire; they will need correlating and developing if we are to hold our own and deserve the title of a civilized cultured people. The War must imbue us with a deeper sense of responsibility. We *must* now follow the advice given us by Huxley long, long ago, to Organize and Organize and Organize. In some way the inertia of our ancient Universities *must* be overcome, so that they are made efficient as educational agencies; in the future, we can have no use for any but observant, reasoning beings, gifted with eyes and practical instincts, as our schoolmasters. We shall need *real men* in large numbers for the service of Australia and of our Empire generally and these can only be trained by men; I have seen everywhere evidence that such are lacking and that our public service is most incomplete, not only in point of numbers. To repeat words used in addressing the Educational Science Section of our Association at Melbourne: "If in Science, to-day, we have something unknown to former civilisations, what is its influence to be on the future of the world, in particular on the future of the white people? If we are not to suffer the rise and fall which all previous civilizations have passed through —rather let me say, if the period of our fall is to be retarded beyond the period our forerunners enjoyed—it will be solely because we wield and use the powers Science has put into our hands: not so much those of abstract science but the broad wisdom which the proper cultivation of Science should confer; hence it is that I desire to urge the absolute importance of giving, through Science, a place to the cultivation of wisdom in the State: therefore in education."

This message we carried to Australia; this message we brought back with us; the task before us is to give reality to it in the future.

[During the lecture nine dozen slides, mostly photographs of scenery, were exhibited. The lecturer's thanks for these are specially due to Miss Thomas, B.Sc., Prof. Pope, Dr. Seward and Dr. Sidgwick.]

[H. E. A.]

WEEKLY EVENING MEETING,

Friday, February 26, 1915.

J. H. BALFOUR BROWNE, ESQ., K.C. D.L. J.P. LL.D.,
Vice-President, in the Chair.

THE REV. A. L. CORTIE, S.J. F.R.A.S.

Total Solar Eclipse of 1914.

THE path of the shadow of the moon during the total solar eclipse of August 21, 1914, entered Europe by the Scandinavian peninsula, crossing from Mosjöen, on the west coast of Norway, to Hernösand on the east coast of Sweden. Thence it passed over the Aland Islands in the Baltic Sea, and penetrated Russia near the city of Riga. Pursuing its course in that Empire it traversed the cities of Minsk, Kiev, and Theodosia in the Crimea, and, crossing the Black Sea, reached Asia Minor near the city of Trebizond. The expedition from the Stonyhurst College Observatory, dispatched under the auspices of the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies, was stationed at Hernösand. An expedition for observing a total solar eclipse has a three-fold character, for it is an adventure, an experience, and an experiment. The uncertainty of the weather conditions that will be experienced on the eventful day, which may render the work of months of preparation abortive, always make an expedition an adventure. From past experience prognostications founded on average meterological results for any given station are generally useless. With regard to the present eclipse, one distinguished Russian astronomer declared that he would wait until all other observers had selected the stations which would presumably enjoy fine weather conditions, and then would take a sporting chance at whatever station was left. Of the seven expeditions that have been dispatched from Stonyhurst to observe total eclipses, two only have enjoyed good weather conditions. Father Perry observed the eclipse of 1870, at Cadiz, in thin clouds: at Carriacou in the West Indies, in 1886, between drenching showers of rain; while that of 1887, when he went to Moscow, was altogether blotted out by thick clouds. In 1889 he went to the Salut Islands, French Guiana, but though the weather was fine, his devotion to duty cost him his life. The story of how he was supported from a sick bed to his instruments by British sailors, and then was almost carried back to H.M.S. "Comus" to die, is a pathetic page in the history of eclipse expeditions. Under my own direction the eclipse of 1905 at Vinaroz, in Spain, was observed through thin clouds; that of 1911 at Vavau, in the Tonga Islands, through thick clouds. But at Hernösand we observed in a perfect translucent blue sky, though had the eclipse taken place on any other day, during our three weeks' stay, except on August 9, and the day of the eclipse, August 21, we should have been baulked by clouds. But though exceptionally favoured by weather conditions, we experienced our share of other adventures.

It had been proposed that Father O'Connor and myself should join Professor Fowler's party intended for Kiev. But the reiterated refusal of the Russian Government through the British Foreign Office to allow the Jesuit Fathers to enter Russia necessitated the division of the original expedition. It was arranged that we should form a separate expedition to Hernösand, a station which had been excluded from the original programme of British official expeditions, on account of its inferior chances of good weather conditions. The permission which was ultimately granted for our entrance into Russia, through the intervention of Dr. Backlund, the Imperial Astronomer at Pulkova, was fortunately not communicated to us. Had we accompanied Professor Fowler, with him we should have had the mortification of abandoning our expedition at Riga, on account of the impossibility of proceeding to Kiev, owing to the incidence of the War and the consequent mobilization of troops. Even had we succeeded in penetrating to Kiev we should have shared the ill-fortune of Professor Campbell and the party from the Lick Observatory, and been prevented from observing by clouds.

Our return journey from Hernösand during the war was also excitingly adventurous. Between Hernösand and Stockholm we traversed two Swedish mine-fields, under the escort of armed vessels. Arriving at Stockholm on August 26, we had to change our intended homeward route via Gothenburg and Hull, and started via Christiania, and thence by the truly magnificent mountain railway to Bergen. Thence on August 29 we took a Norwegian steamer to cross the North Sea to Newcastle. Had it not been for timely warnings given us in the course of the succeeding day by British cruisers, we should in all probability have run into a mine-field laid by the Germans some little distance from the mouth of the Tyne. Accordingly we changed our course for Peterhead, and came down the coast inside the mine-field. Finally, we were escorted into Newcastle on the early morning of August 31 by three torpedo-boats. The British Minister at Stockholm, Sir Esmé Howard, very kindly took charge of our instruments, and subsequently transmitted them to the Vice-Consul at Bergen, whence they were safely shipped to Hull.

One experience of those who go on eclipse expeditions is invariable, and that is of the kindliness and helpfulness of all with whom observers are brought into contact. Professor B. Hasselburg, of the Academy of Sciences at Stockholm, did everything in his power to secure the success of the British expedition. He recommended a suitable site for our instruments at Hernösand, and obtained the expediting of our cases by the Custom-house officials after a merely formal inspection. At Hernösand the Rector of the Technical School, Herr Tham, placed his school, containing physical and chemical laboratories, mechanics, and carpenters' shops, and a dark-room, entirely at our disposal, and that of an expedition from St. Ignatius' College, Valkenburg, Holland, consisting of Father Wulf and Father Rodés. The Technical School had an efficient electrical installation, and Herr Helenius, the town electrician, laid down leads gratuitously, so that we were enabled to employ the current at our instruments. We finally chose to erect these in a field at the back of the Technical School, at an altitude of about 300 feet above sea-level. The co-ordinates of the station were: latitude 62° 38′ 8·8″ N., and longitude 17° 57′ 32·25 E. of Greenwich. The computed duration of totality was 129 seconds. It really was 125 seconds. The station was 3·9 miles from the central line of totality.

Our experiences in the erecting and adjusting of the instruments are described in some detail in the Preliminary Report of the expedition, communicated to the Royal Astronomical Society by the Joint Permanent Committee.* It will be only necessary, therefore, to summarize some of the more important details. The members of the expedition, in addition to myself, were Father O'Connor, of the Stonyhurst College Observatory; Mr. G. J. Gibbs, of Preston; and Mr. E. T. Whitelow, of Birkdale, Southport. Our instrumental equipment included a 16-inch cœlostat belonging to the Royal Astronomical Society, and an 8-inch "Grubb" cœlostat, kindly lent by the Royal Irish Academy. The 16-inch cœlostat supplied light to three coronagraphs and a horizontal telescope for projecting an enlarged image of the sun on to a circularly graduated screen of ground glass. The long focus coronagraph had an O.G. of 4 inches aperture, and focal length 20 feet. This also was kindly lent by the Royal Irish Academy. The image of the sun for best focus was 2⅛ inches in diameter. For obtaining images of the filmy extensions of the coronal streamers we employed two cameras, one with a 4-inch O.G., focal length 34 inches, the other with O.G. 3·5 inches and focal length 12 inches. Our programme of exposures included six exposures with the long-focus lens of 2, 4, 10, 25, 7, 3 seconds respectively. All these, except the first, were successfully made by Father O'Connor. With the 34-inch coronagraph four exposures of 10, 50, 15, 5 seconds were secured, and with the 12-inch camera one long exposure of 95 seconds. These two instruments were under the charge of Mr. Whitelow. The plates employed were bathed by Mr. Crowther, of Carlisle, in a solution to render them impervious to solarization. In addition to these two cameras Mr. Whitelow had mounted on a wedge-head, cut to the latitude of Hernösand, a Zeiss lens of 14-inch focus, with which he successfully photographed on "Hydro" plates the moon projected on the corona, 30 seconds, and 1 minute after totality. The function of the 3-inch Cooke telescope mounted on top of the long camera was to enable Father O'Connor to give signals 10 minutes, 5 minutes, and 10 seconds before totality, by observation on the graduated glass screen of the angles subtended by the cusps of the advancing moon at the centre of the sun's projected image. An ingenious eclipse-clock constructed by Mr. Gibbs, a large hand completing the circuit of a two-foot dial once in 129 seconds, was also set up. It was illuminated by an electric lamp.

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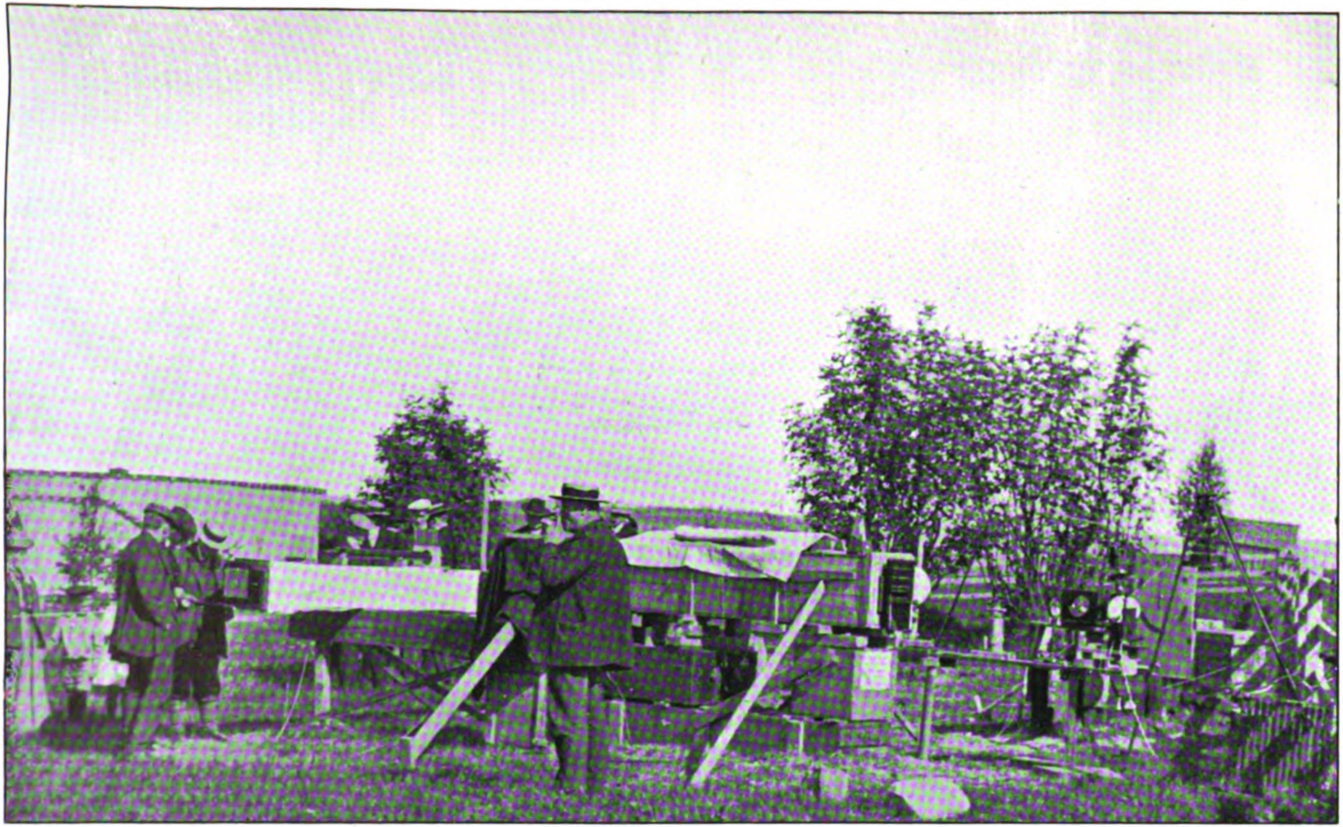
* Monthly Notices, R.A.S. lxxv. No. 3, January, 1915.

The 8-inch cœlostat was employed to supply light to a 5-inch Alvan Clarke lens, focal length 7 feet, which projected an image of the sun on to the slit plate of the spectrograph. This spectrograph was designed in conjunction with Professor Fowler, and was constructed under his supervision at the Imperial College of Science, South Kensington. It was probably the most powerful slit spectrograph so far used in eclipse expeditions, and gave a very brilliant spectrum covering 4 inches lineally between λ 6700 and λ 4800. It was of the Littrow type, and the constants of the instrument were as follow :—

Length of slit	$\frac{7}{8}$ inch.
Width of slit	0·0017 inch.
Aperture of O.G.	6 inches.
Focal length of O.G.	98 inches.
Edge of prism	7 inches.
Refracting angle of prism	40°.
Deviation of prism at H_β	28·5°.
Diameter of plane mirror	6·5 inches.
Purity of the spectrum	6400.

This instrument was worked by Father Cortie and Mr. Gibbs.

A novel feature in eclipse operations was the photographing of a comparison spectrum given by an iron-arc on the same plate as the spectrum of the corona. A current of 12 amperes at a pressure of 110 volts was employed. The image of the arc was formed on the slit by means of a single lens, 4·9 inches aperture, and a diagonal prism. The diagonal was mounted in front of the lens so that it could be pushed forwards and backwards in a slotted groove. To fill the O.G. with light from the arc, the lens was adjusted so that the solid angle of the beam of light from the arc was equal to that subtended by the O.G. from the slit. A zinc shield with a horizontal slot $\frac{1}{10}$ inch wide was employed to cover the slit plate, during the exposure of the corona, when it was desired to expose for the spectrum of the arc. The slot lay across the dark image of the moon. The coronal image was placed on the slit plate so that the slit was almost tangential to the sun's limb on the E. side. The exposure on the spectrum of the corona had been in progress about 40 seconds, when the slotted shield was placed in position, the diagonal was pushed forward, and the arc struck. After an exposure of 4 seconds on the



THE ECLIPSE INSTRUMENTS. The morning of the eclipse, August 21, 1914.

arc the diagonal was pushed back, the slotted shield removed, and the exposure was continued on the corona, none of the optical parts for the formation of the image having been disturbed in the meanwhile. All the operations necessary for photographing the arc spectrum took about 20 seconds.

Drills commenced on August 17, and we were assisted by seven students of the Technical School, under the direction of Herr Askling, the master of mechanical science. Four of these students were trained to make a composite drawing of the corona, on graduated disks, each confining his attention to one quadrant. They practised on a drawing of the corona of 1901. The result was a very good picture of the chief details of the corona as visible to the naked eye. The photographs, however, taken with the coronagraphs of shorter focal length show a greater extension than was visible to the eye.

On the evening of August 20 the wind shifted to the S.E., and brought with it a perfectly clear sky, though clouds appeared about an hour after the eclipse was over. A considerable number of spectators assembled in the field, many of whom watched the progress of the partial phase through dark glasses. About 20 minutes before totality came the well-known eclipse wind, quite a chilly breeze from the S.E. The total drop of temperature was 7·4° F. At 10 minutes before totality, as indicated by the cusps on the ground-glass screen, Father O'Connor blew three blasts on a whistle. The observers and their assistants stood to their stations, the clocks were wound up, and the lamps were lit. At this time the illumination of the distant mountains to the north was weirdly beautiful. At 5 minutes before totality, at the signal of two blasts on the whistle, the slides were drawn, and silence was called. At 10 seconds nominally, but in reality at 12 seconds before totality, a single blast was the warning for exposures on the "flash" spectrum. At the signal "Go!" when totality was reached, Father O'Connor pulled a cord, which released the mechanism of the eclipse clock. The experience of the sight of a magnificent corona in a perfectly pure sky was most impressive and enthralling. On the west of the eclipsed sun was a long fish-tail streamer, while on the east were three bright winged streamers, a very bright one N.E., a longer though less bright one S.E., and a fainter bayonet-like streamer placed almost equatorially. In the N.W. blazed the planet Mercury, while Venus was shining brightly in the E. sky.

The general form of the corona of 1914 was rather of the "minimum" than of any other type. The open spaces at the poles, full of beautiful rays, and the long fish-tail streamer on the W. side, are indicative of this type. But on the E. side it approximates to the "intermediate" type, the spreading of the streamers being a characteristic feature. It is interesting from the fact that, occurring at a time when the sun-spot curve was rising after a protracted minimum, it suggests the genesis of the "maximum" type, as the superposition of bright streamers on the two long wings in the equatorial regions, which probably are a sub-permanent feature of all coronæ. The N.E. streamer is projected over the background of the continuous corona, and consists of a sheaf or bundle of very bright rays, which can be traced as far as 26′ from the sun's limb. The filamentous nature of the other streamers is also marked. The lower corona is represented by a bright continuous ring all round the sun, between 1′ and 2′ of arc in depth. This is seen on both the large scale and small scale photographs. A considerable arc of the chromosphere is shown on the W. limb, on the last-exposed of the larger scale photographs, as the slide was only just closed before the end of totality. The southern boundary of the S.E. streamer extends farther from the sun than any other feature, fully two diameters. It has a marked double curvature. On the W. side of the corona at position angle 356°, in close proximity to a small bright prominence, is a remarkably distinct filament, which can be traced as far as 7′ from the sun. It curves over towards the equator. Immediately below the filament are the bright edges of two shorter streamers, which are superposed upon the main mass of the coronal wing. These three bright rays are as characteristic of the N.W. limb as the bright sheaf of rays of the N.E. limb. We shall discuss their probable import in a subsequent part of this discourse.

The spectrum of the corona next claims our attention. Although extremely weak on the plate it is quite measurable. The corona was a bright one, yet the well-known green radiation of the coronal light about wave-length 5303 is inconspicuous, though just discernible on the plate. This result agrees with that obtained by other observers. Secondly, the intenser radiations are in the red end of the spectrum. Other observers of the eclipse have called attention to a well-defined bright radiation about wave-length 6374. This line is one of the strongest on our negative, but it is not an isolated radiation. It occurs as a member of one of a series of three bands or flutings at this end of the spectrum. The wave-lengths of these terminal lines are as follows:—

1. $\left\{\begin{matrix}6643\cdot 9\\6630\cdot 5\end{matrix}\right\}$ Three lines. 2. $\left\{\begin{matrix}6530\cdot 9\\6502\cdot 8\end{matrix}\right\}$ Six lines.

3. $\left\{\begin{matrix}6384\cdot 3\\6363\cdot 0\end{matrix}\right\}$ Seven lines.

Two other bands occur at wave-lengths—

4. $\left\{\begin{matrix}5118\cdot 1\\5114\cdot 3\end{matrix}\right\}$ Two lines. 5. $\left\{\begin{matrix}5315\cdot 3\\5302\cdot 7\end{matrix}\right\}$ Four lines.

All the lines in these bands are sharp, though the images are very faint. There are also other lines visible which have not as yet had definite wave-lengths assigned to them. The general character of the coronal spectrum on the photograph is undoubtedly that of a series of bands or flutings. From a study of the position of the slit relatively to the corona, it appears that the spectrum is not that of the upper chromosphere, nor that of the lower corona, but of the roots of the streamers on the E. side of the sun. The spectrum is unique, and suggests, as many another physical experiment, a fresh problem for further investigation.

We have dwelt upon our own results and operations, as these are naturally more familiar to us than those of the other British expeditions. Unfortunately two of the other three expeditions were entirely spoilt by adverse circumstances. It was a hard fate which compelled Professor Fowler to be stranded at Riga, and to be unable to proceed to his destination on account of the war. It was perhaps a harder fate which, frustrating all the careful preparations and adjustments of instruments at a chosen site at Theodosia in the Crimea, befell Professor Newall. For he had the mortification of being robbed of a view of the eclipsed sun by purely local clouds. Two miles from his station the Italian expedition under Professor Riccó enjoyed a cloudless sky. We sympathize with these astronomers in their naturally keen disappointment. At the same time we can rejoice in the success of the Greenwich observers, Mr. Jones and Mr. Davidson, at Minsk. With the Thompson coronagraph they obtained seven large scale photographs of the corona full of fine detail. More interesting still was the employment of two photographic cameras, with green colour screens, to obtain information as to the presence and distribution of coronium. In front of the object-glass of each of these a prism was placed. Its function was to disperse the continuous light passing through the monochromatic filters. Had coronium been present it would have shown itself as a bright ring or portions of a ring round the eclipsed sun. The resulting photographs show that it was entirely absent. This observation, which corroborates and strengthens the value of those made by other observers in this eclipse, is of great importance. The corona was bright, and yet there was no green coronium radiation as we should have naturally expected. As we have already noted, the faintest possible trace of this radiation appears in a band or fluting on our own photograph of the coronal spectrum.

The Greenwich observers have also been successful in another direction. With the Hills quartz spectroscope, specially improved for this occasion in various adjuncts, the spectrum of the "flash" has been photographed down to wave-length 3118 in the ultra-violet. This is considerably farther than had been photographed before, as far as accurate measures of the wave-lengths of the lines are concerned. At least 120 lines of a shorter wave-length than those which were photographed by Mitchell in 1905 have been measured.

The extended spectrum of the flash in the ultra-violet secured by the Greenwich observers, and the novel and unique spectrum of the corona in the red photographed by the Stonyhurst observers, are outstanding features of the spectroscopic records obtained at this eclipse. In addition, both parties of observers employed a comparison spectrum for the first time in eclipse observations, the Greenwich observers using the spark spectrum, and the Stonyhurst observers the arc spectrum of iron.

The co-ordination of the various phenomena observed on the sun's surface, or in the surrounding envelopes, is of primary importance in the advance of knowledge of solar physics. Sun-spots are undoubtedly the chief foci of activity in the manifestations of energy displayed by faculæ, flocculi, and eruptive prominences. The relation, therefore, which may exist between sun-spots, or areas of sun-spot activity, and the streamers of the sun's corona is a subject worthy of investigation.

The concordance between the general aspect of the sun's corona and the curve of sun-spot activity has been well established. At times of maximum sun-spot activity the corona extends all round the sun in irregular masses. At minimum there are two long wings disposed equatorially, generally in the form of a wind-vane, with open spaces at the poles filled with delicate filmy rays. At periods intermediate between maximum and minimum there are a number of bright streamers, which are superposed upon the equatorial wings, which have now extended both northwards and southwards. It has not unfrequently been asserted,* since sun-spots are confined within zones some 30° north and south of the equator, while prominences, which are more active at sun-spot maxima, are found in all latitudes, and even at the poles, that the solar corona reflects rather the variations of the curve of prominence activity than those of the sun-spot curve. Hence the conclusion has been drawn that the streamers of the corona emanate from regions of prominence activity. But there seems to be very little, if any, direct evidence of this from eclipse photographs. Whatever direct evidence we possess of the connexion between prominences and the solar corona points to the formation of a system of beautiful interlacing arches in the lower portions of the corona, immediately over the prominences. This is shown in a striking manner, for instance, in the photographs obtained by Professor Schaeberle in the eclipse of 1893, and in my own photographs of that of 1905.

If the streamers are supposed to emanate from regions of sun-spot activity, it is not difficult to explain the general aspect of the corona. As a preliminary, it must be borne in mind that we are viewing the streamers, which really exist in space in three dimensions, projected around a seemingly flat disk in two dimensions. The sun-spot zones have a limiting latitude of about 30° north and south of the solar equator, and at times of minimum have a mean latitude which is much lower. This explains the two main wings which are

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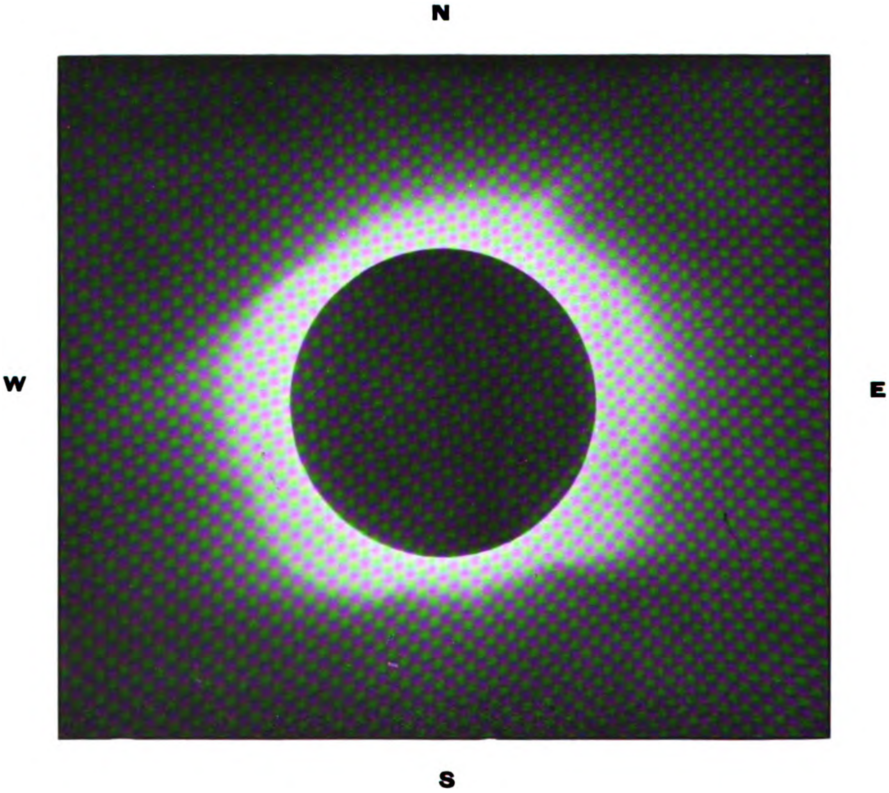
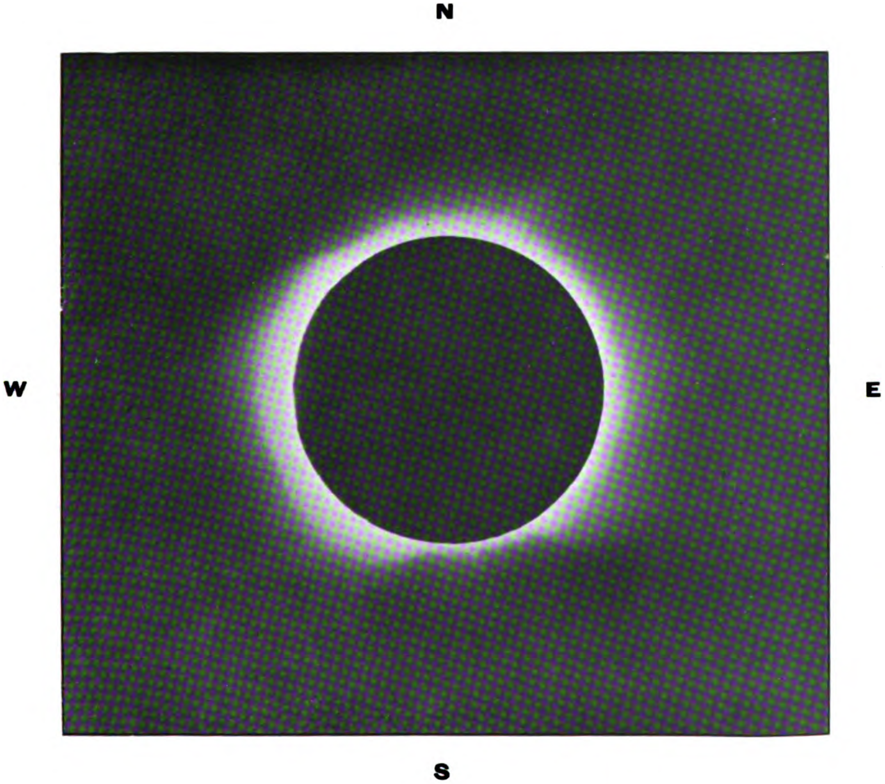
* e.g. Proceedings of the R.I., **xx.** p. 352.



THE SUN'S CORONA, August 21, 1914.
Exposure 25 seconds, 20-foot coronagraph. Reduced $\frac{7}{10}$.



THE SUN'S CORONA, August 21, 1914.
Exposure 3 seconds, 20-foot coronagraph. Reduced $\frac{7}{10}$.

disposed equatorially at such periods. At times of "intermediate" activity, when the sun-spot curve is rising towards a maximum, or falling towards a minimum, we should expect the wings to extend to higher latitudes and lower latitudes. This, too, is verified; the long equatorial wings of the minimum period assuming the form of four petals of a single dahlia. Sometimes sheaves of bright streamers are in addition projected across this winged background. Such were the coronæ of 1886, 1896, and 1898. A curve of limiting latitudes of the greater sun-spot disturbances reflects in its variations very closely the positions of the main wings of the corona.

At periods of maximum solar activity large and active spot centres are found in all latitudes, within the limits 20° N. and S. The streamers proceeding from such regions of activity would be projected in all possible directions around the sun, when viewed in two dimensions, from the earth, and would be sufficient to give the appearance of such coronæ as those of 1882, 1893, and 1905. The areas of sun-spot activity at such periods are also the areas of bright faculæ, of extensive overlying flocculi, and of the more violent outbursts of metallic and active prominences. It seems to be probable that these disturbances, focused on sun-spots, are more responsible for the irregular form of the corona at maximum than the prominences alone. In this view of the matter the wings are covered over by the projected images of streamers radiating in all directions.

In a research communicated to the Royal Astronomical Society * in the year 1913, evidence was adduced in the cases of four eclipses, namely, those of 1893, 1898, 1905, and 1908, of the existence in the sun's corona of bundles of rays, diverging apparently from narrow belts of areas of long-continued sun-spot disturbance. Moreover, the areas of disturbance had been connected with series of magnetic storms on earth, thus suggesting that the mode of propagation of the sun's influence which is operative in magnetic storms is represented by the stream lines of the solar corona. In these cases the areas of sun-spot disturbance had been near the limb of the sun.

The eclipse of 1914 took place when the sun-spot curve was rising, the mean daily area of spots in 1914 being about twenty times what it had been in 1913, which was the year of absolute minimum. The largest spot of the year was on the disk, being of considerable size, in north heliographic latitude 18° 18′ and longitude 67° 18′. But it was situated about 68° from the west limb. It has already been noted that superposed on the coronal wings N.W. were two bright streamers, and above them a very bright filament at position angle 356°, and over the E. wing a sheaf of bright rays constituting the N.E. streamer. Is it possible that these striking features of the solar corona, which are mainly responsible for differentiating it from the minimum type, were connected with the area of disturbance of

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* Monthly Notices, R.A.S., vol. lxxiii. Nos. 6, 7.

which the spot was the centre? We have a single large spot on the sun surrounded by a larger area of faculæ, and doubtless a still larger area of flocculi, and an incohative stage of the transition from a "minimum" to the "intermediate" type.

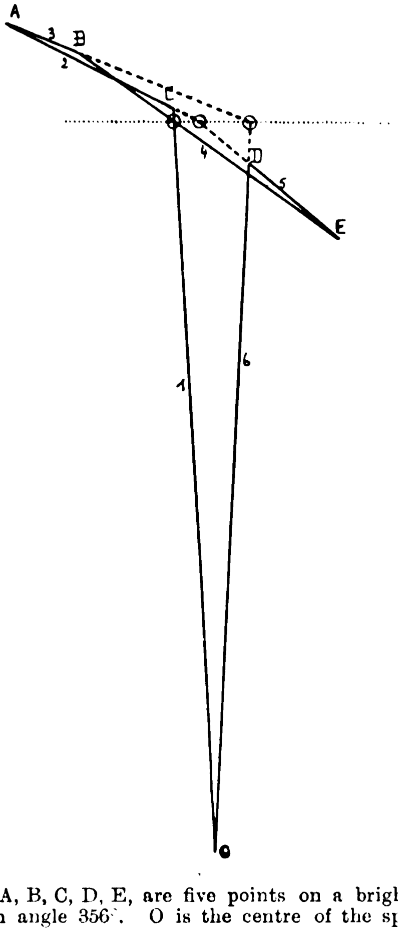

FIG. 1.—A, B, C, D, E, are five points on a bright ray at position angle 356°. O is the centre of the spot. The small circles which are collinear are the intersections of the opposite sides of the hexagon.

Mons. F. Henroteau, a Belgian astronomer from the Royal Observatory at Uccle, Brussels, a refugee at Stonyhurst, suggested the application of Pascal's theorem in pure geometry, to ascertain whether there was any connexion between the streamers named and the spot. His constructions were made from measures taken on an enlarged positive of the corona, six inches to the moon's diameter. In fig. 1, A, B, C, D, E, are five points accurately measured on the single bright ray at position angle 356°, and O is the centre of the large spot. The lines O C, C A, A B, B E, E D, D O form a hexagon. The intersections of the directions of the opposite pairs of sides O C, B E; C A, E D; A B, D O are in a straight line. Hence a conic passes through the five points measured on the ray and the sun-spot. Applying the geometrical process of reciprocation and making a graphical solution, it is found that the projected conic is a very elongated ellipse. Treating by the same method the bright edges of the N.W. streamers, it is found that the projected conics certainly come from the area immediately surrounding the spot, and probably from the spot itself. The geometrical solution in these cases is not so rigid,

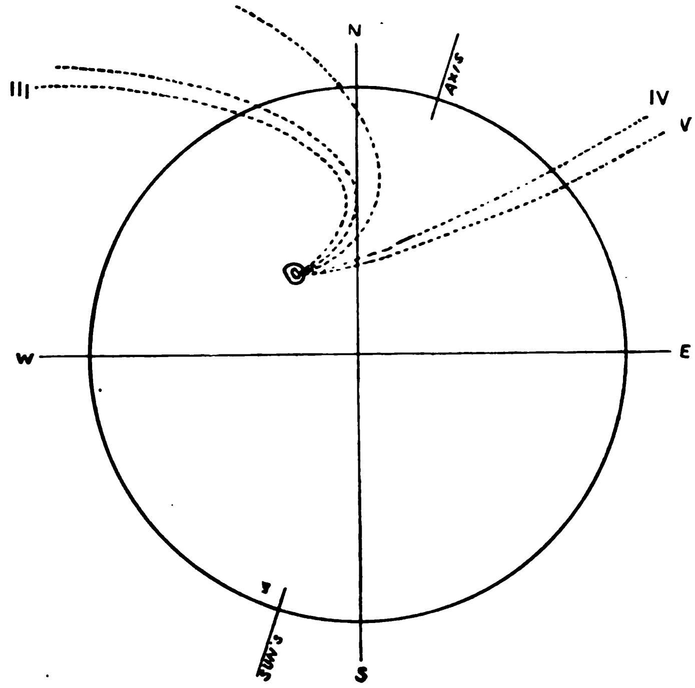

Fig. 2.—Curves I, II, III, on the West. Curves IV, V, on the East. O is the spot.

first, because the edges of the streamers are not so well-defined as in the first case, and secondly, because some of the angles of the hexagons are very acute, so that the point O, representing the spot, could be shifted, within limits, without altering sensibly the collinearity of the points of intersection of the opposite sides of the hexagon. The two projected conics are hyperbolæ.

We now turn to the luminous sheaf of rays in the N.E. quadrant, of which two bright ribs were selected for measurement. In these cases the solution is again not so rigid, even as in the last two cases, and the position of O could be considerably varied, without sensibly affecting the collinearity of the points of intersection of the opposite sides of the hexagon. The reason is that these rays point almost directly, with a very slight curvature, at the spot, which at first sight seems paradoxical. All we can affirm is, that the six points in each case lie on conics which probably pass through the region of the spot. The projections being nearly straight lines would indicate that the conics of projection from the region of the spot are seen nearly edgewise.

In the N.W. quadrant, therefore, curve I (fig. 2) is an elongated ellipse, which certainly passes through the immediate region of the spot. Curves II and III are hyperbolæ which most probably pass through the immediate region of the spot. They almost certainly pass through such a region as could be represented by the accompanying faculæ and flocculi. All we can affirm about curves IV and V is the probability that they pass through the region of disturbance. The total result of this investigation is that the area of disturbance marked by the sun-spot as a centre was the seat of emanation of streams of matter, represented certainly by the projected conic of curve I, very probably by those of curves II and III, and probably by those of curves IV and V. From this result, and those established in former investigations for the eclipses of 1893, 1898, 1905, and 1908, it would follow that the mode of propagation of matter from a centre of disturbance on the sun, which may be supposed to be operative in the production of terrestrial magnetic storms, is in the form of a set of diverging rays, and not in that of a single cathode-like discharge.

[A. L. C.]

GENERAL MONTHLY MEETING,

Monday, March 1, 1915.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Ernest George Coker, Esq., M.A. D.Sc.
Arthur L. Armitage Forbes, Esq., M.D. F.R.C.S.
Mrs. L. M. de Laszlo.
Charles James Singleton, Esq.
Frank Coston Taylor, Esq.

were elected Members of the Royal Institution.

The Managers reported, That they had received a sum of £5000 from the Rt. Hon. Charles A. Parsons.

Read, the following letter addressed by the Hon. Sir Charles Parsons to Sir James Crichton-Browne, the Treasurer:—

February 14, 1915.

DEAR SIR JAMES,

It gives me great pleasure to enclose cheque £5000 in favour of the Royal Institution, which may be of some help at the present time.

Yours very sincerely,
(Signed) CHARLES A. PARSONS.

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to express to the Hon. Sir Charles A. Parsons, K.C.B., J.P., LL.D., D.Sc., F.R.S., who has unconditionally placed at their disposal, for the purposes of the Institution, the sum of £5000, their most grateful appreciation of his munificence and discernment. They accept the gift as a timely and noble recognition of the good public work the Institution has done in the past, and is still doing, in the acquisition and diffusion of scientific knowledge, and as an incitement to maintain and extend its usefulness in the unique position which it has for more than a century occupied.

RESOLVED, That the Resolution of the Managers be adopted by the Members.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute: Pusa: Report for 1913-14. 8vo. 1914.

Agricultural Journal, Vol. X. Part 1. 8vo. 1915.

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Alchemical Society—Journal, Vol. III. Parts 15, 16. 8vo. 1915.

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Proceedings, Vol. XXVI. 1913–14. 8vo. 1914.
Armstrong, Charles Wicksteed (*The Author*)—The Mystery of Existence in the Light of an Optimistic Philosophy. 8vo. 1909.
Astronomical Society, Royal—Monthly Notices, Vol. LXXV. Part 3. 8vo. 1915.
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Rules and List of Members, 1915. 8vo.
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Canada, Department of Mines—Geological Survey: Museum Bulletin, No. 8. 8vo. 1914.
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Carr, H. Wildon, D.Litt. M.R.I. (*The Author*)—The Philosophy of Change: a Study of the Fundamental Principle of the Philosophy of Bergson. 8vo. 1914.
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Chile, Central Meteorological Institute—Valors, Horars de los Elementos Meteorologic en Santiago, 1912: Publicacione No 7. 4to. 1914.
Medidas de agua caida en 1912: Publicacione No. 8. 4to. 1914.
Ondas, Certizianas registradas en San Carlos de Ancud, 1913: Publicacione No. 9. 8vo. 1914.
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Clarke, Ernest, M.D. B.S. F.R.C.S. (*The Author*)—A Brief Review of the Work of Donders, and the Progress made during the last 50 years in Problems in the Accommodation and Refraction of the Eye. 8vo. 1914.
Editors—
Agricultural Economist for Feb. 1915. 8vo.
Amateur Photographer for Feb. 1915. 8vo.
Athenæum for Feb. 1915. 4to.
Bookseller, Feb. 1915. 8vo.
Cartoon, The. Feb. 1915. 8vo.
Chemical News for Feb. 1915. 4to.
Chemist and Druggist for Feb. 1915. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 5, 1915.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. D.C.L. LL.D. F.R.S., in the Chair.

PROFESSOR E. B. POULTON, M.A. LL.D. D.Sc. F.R.S. M.R.I.

Mimicry and Butterflies.

SUPERFICIAL resemblances between distantly related butterflies had been pointed out by Boisduval in 1836, long before Bates read his classical paper on mimicry before the Linnean Society, on November 21, 1861. Dealing with South American resemblances, and especially with the examples he had observed in the valley of the Amazons, Bates suggested the well-known hypothesis of the advantageous likeness of a rare and palatable "mimic" for some conspicuous and abundant "model" inhabiting the same locality. In a few years Bates' conclusions were supported by A. R. Wallace, writing upon the butterflies of the Oriental region (1866), and by Roland Trimen, upon those of Africa (1870).

It is important to remember that Bates, in his original memoir, spoke also of other likenesses between butterflies which he did not attempt to explain by his hypothesis of mimicry, viz. the local resemblances between conspicuous species belonging to different groups. Such resemblances between the models themselves also strongly attracted the attention of Fritz Müller, a great German naturalist resident in Brazil – so strongly indeed as to prevent him from accepting Bates' hypothesis as an explanation of wide application among butterflies. The great majority of the superficial likenesses between his Brazilian butterflies belonging to different groups were clearly not to be interpreted by the mimicry of Bates, for of this majority *all* were abundant, conspicuous, slow-flying, and presumably protected by taste, smell, or some special quality which rendered them unpleasant or unwholesome as food. Bates had suggested that such a likeness between models was due to some influence, presumably physical or chemical, connected with locality; but this interpretation did not satisfy Fritz Müller. For many years he puzzled over the subject, testing various ideas, as, for instance, the possible operation of sexual selection, throwing out the suggestion in his correspondence with Charles Darwin, that butterflies in the choice of their mates were influenced by the appearance of other butterflies in the same locality. Concerning this bold hypothesis, Darwin wrote, January 23, 1872, to Prof. Meldola :

"You will also see in this letter a strange speculation, which I should not dare to publish, about the appreciation of certain colours being developed in those species which frequently behold other forms similarly ornamented. I do not feel at all sure that this view is as incredible as it may at first appear. Similar ideas have passed through my mind when considering the dull colours of all the organisms which inhabit dull-coloured regions, such as Patagonia and the Galapagos Islands."

It was, however, impossible to accept sexual selection as a valid explanation of the facts, because in a large proportion of mimicking species the resemblance was confined to the female. On the other hand, there were hardly any species in which the male but not the female was a mimic. At the same time many facts disproved Bates' suggestion that physical or chemical forces were responsible for the likeness between models. Finally, in 1878,* sixteen years after the publication of Bates' memoir, Fritz Müller hit upon an explanation with which he was satisfied. The idea evidently came to him quite suddenly, for he begins his first little paper on the subject: "It is remarkable how one sometimes puzzles for years over questions the solution of which is so simple, that one can hardly understand how there could have been even a momentary difficulty over them."

Fritz Müller's interpretation of what has since been known as "Müllerian Mimicry," depends on the saving of life during the education of insect enemies. When two warning patterns are different, they must be learned and remembered separately, and the two species that bear them must separately endure the necessary waste of life or mutilation. When two patterns are similar the two species share the waste between them, instead of each suffering it independently. Thus "Müllerian Mimicry" is not mimicry at all in Bates' sense. It is rather "common warning colours," or "synaposematic resemblance." The mimicry of Bates has been defined, on the same system of terminology, as "false warning colours," or "pseudaposematic resemblance."

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* Zool. Anzeiger (Carus), i. (1878), pp. 54, 55.

That these two kinds of mimicry exist is admitted by nearly all naturalists who have paid attention to the subject, but there is much difference of opinion as to their relative extent and as to the position of particular examples in the category of Bates or the category of Müller. On these subjects there has been much controversy, Dixey and Poulton upholding the relative importance of Müllerian and Guy Marshall supporting the predominance of Batesian Mimicry.

In this controversy attention has been devoted to two lines of evidence. (1) Diaposematic Resemblance, or "Reciprocal Mimicry," originally suggested, but not worked out in detail, by Fritz Müller, and chiefly developed in later years by Dixey. Controversy has here turned upon the correct interpretation of the patterns believed to have originated in reciprocal mimicry. (2) Secondary Mimicry, or the mimicry of a mimic, rather than of the primary model.

In the meantime, increase in our knowledge has been progressing at a rapid rate, especially in Africa, where English naturalists, following the example of Roland Trimen, have been working at these problems for many years. We owe nearly all our recent knowledge to nine naturalists working in various parts of the continent:—G. D. Hale Carpenter, on the islands and shores of the north-west Victoria Nyanza; W. A. Lamborn, in the Lagos and Ibadan districts of Southern Nigeria; G. F. Leigh, in the Durban district of Natal; G. A. K. Marshall, in Natal and in S. Rhodesia; the late A. D. Millar, in the Durban district; S. A. Neave, in N. Rhodesia, Katanga, British C. Africa, German and British E. Africa, and Uganda; Rev. K. St. Aubyn Rogers, in British E. Africa; C. F. M. Swynnerton, in S.E. Rhodesia; C. A. Wiggins, in the Entebbe district of Uganda. Nearly the whole of the results bearing on the theory of mimicry, discovered by these naturalists, will be found in the Transactions and Proceedings of the Entomological Society of London.

While this work has been going on in the field, three museums at home have been engaged upon the material supplied—the British Museum of Natural History, the Hope Department of the Oxford University Museum, and the Tring Zoological Museum.

It is only possible in a single lecture, and that rigidly limited to one hour, to direct attention to some of the most striking results that have been obtained.

Roland Trimen originally showed, in 1870, that a swallow-tailed butterfly, *Papilio dardanus* (*merope*), represented in Madagascar by a form (*meriones*) with a female like itself, possessed on the mainland of Africa females entirely unlike the male, and mimicking three different distasteful Danaine butterflies. These different forms and others have now been bred from known female parents by G. F. Leigh, W. A. Lamborn, G. D. H. Carpenter, and C. F. M. Swynnerton.

G. A. K. Marshall, in 1902, suggested that two East African Nymphaline butterflies, with male and female alike (*Hypolimnas wahlbergi* and *H. mima*), mimicking respectively two Danaine butterflies with very different patterns (*Amauris niavius dominicanus* and *A. echeria*), were only two forms of a single species. Both have now been bred from the same female parent by A. D. Millar (recently confirmed in Natal by E. E. Platt), while the corresponding forms of the West have been bred again and again in a very extensive series of experiments by W. A. Lamborn. In these breedings, as well as in the experiments on *Papilio dardanus*, a Mendelian relationship between the mimetic forms was indicated by the results obtained.

Dr. Karl Jordan, of the Tring Zoological Museum, extended the above results into a wide generalization as to the relationship between models and mimics. By accurate and detailed anatomical investigations he proved that it is common, in all tropical countries that are rich in butterfly life, for two or more distinct species with different patterns to be resembled by the different forms of a single mimicking species. The most striking example in which he proved this relationship to exist, belonged to the African Nymphaline genus *Pseudacræa*. A number of *Pseudacræas*, supposed to be species, were found on the West Coast, mimicking large *Acræas* of the genus *Planema*; other different *Pseudacræa* mimics of *Planemas* occurred in Uganda; another in British East Africa; another in Natal. Dr. Jordan discovered that all these mimetic forms were anatomically inseparable, thus reaching the conclusion that all the *Pseudacræas* of this group were the mimetic forms of a single species described by Linnæus from the West coast as *P. eurytus*. The startling nature of this result becomes manifest when we remember that one of these forms in Uganda has a male very different from its female, while two other forms in the same locality have the sexes alike. It was, therefore, of the utmost importance to test the inference from anatomy by the method of breeding. The late A. D. Millar had successfully bred the Natal species, but this being of a single form, although with some of the females unlike their males, did not serve as a test. Finally, however, in 1912 and 1913, G. D. H. Carpenter succeeded in breeding several families from known parents. This work, carried on in Bugalla, one of the Sesse Islands in the N.W. of the Victoria Nyanza, conclusively proved that Dr. Jordan was entirely correct in his conclusions.

Another line of investigation which has been extensively pursued in recent years is the geographical distribution of both models and mimics, together with changes that are found in various parts of the range. A good example is furnished by these same *Pseudacræas* in Uganda. The commonest form in the Entebbe district, as shown by the prolonged researches of C. A. Wiggins, has been known as *P. hobleyi*, with its male mimicking the male of *Planema macarista*, as well as both sexes of *P. poggei*, its female, originally described as *Pseudacræa tirikensis*, mimicking the female of *Planema macarista*. C. A. Wiggins' splendid material at Oxford shows that a very small proportion of the females of *Pseudacræa hobleyi* resemble their males and mimic the same models as these latter. Now, as we pass eastward round the northern shores of the Victoria Nyanza, *Planema macarista*, far commoner than *P. poggei* at Entebbe, becomes rarer and apparently ceases on the western side of the Nile, issuing from the lake at the Rippon Falls, near Jinga. *P. poggei*, on the other hand, continues much farther eastward, to Mount Elgon and beyond it to the north-eastern corner of the lake, and probably farther into British East Africa. In correspondence with this distribution S. A. Neave found that the male-like female of *Pseudacræa hobleyi* becomes the predominant form east of the Nile. On the other hand, the female form *tirikensis*, resembling the female of *Planema macarista*, is predominant where, as at Entebbe, its model is common.

Another interesting geographical change is witnessed when we compare C. A. Wiggins' experience at Entebbe with G. D. H. Carpenter's in Damba and Bugalla Islands in the lake. At Entebbe the above-mentioned *Pseudacræa hobleyi*, with sexes nearly always unlike, is accompanied by two other mimetic forms with sexes alike, all three, as we know from Carpenter's researches, belonging to the same species. These are *terra*, mimicking *Planema tellus*, and *obscura*, mimicking *P. paragea*. At Entebbe the total number of these forms together is shown by Wiggins' investigations to be about one-third of their *Planema* models. On the two islands the proportions are very different, the models being greatly outnumbered by their mimics. On Bugalla, indeed, where the chief captures were made, the *Planemas* were not much over one-third of the *Pseudacræas*. We do not know the cause of this difference: it may be due to difference in the abundance of the food-plants or in some other condition. Corresponding with this reversal in the proportions, the mimetic forms, which are on the whole constant and sharply separated on the mainland, tend to vary and to run into each other on the two islands. The mimicry keeps true where the models are relatively abundant, but often tends to become blurred where they are relatively scarce. It must also be borne in mind that even on the mainland the *Pseudacræas* are far commoner than we should expect them to be, on the hypothesis of Bates.

Passing from Entebbe to the westward, other interesting changes are seen in both models and mimics. It has been explained that in the Entebbe district, the male of one chief model, *Planema macarista*, is mimicked by the male of *Pseudacræa hobleyi*. Both are dark butterflies, with an orange bar crossing the forewing and a white bar the hind. In a forest in western Uganda, S. A. Neave found the male of another species of *Planema* with both bars orange. In the same forest the male *Pseudacræa* had both bars orange, as had also two other mimics, which at Entebbe resembled the male *macarista*—viz. the female *Acræa alciope* and the male *Acræa althoffi*. Two examples of this last male were collected by S. A. Neave, and they are unlike any form of the species that has been seen hitherto ; but as regards the other two mimics similar forms are known to occur, although in very small proportions, to the north-west of the Victoria Nyanza.

The geographical changes of the female *Acræa alciope* in relation to its models, may be considered in fuller detail. The female at Entebbe with orange and white bars, is so unlike its non-mimetic male that it was always regarded as a distinct species, until a few years ago the true relationship was made out by H. Eltringham. On the west coast the female is less unlike its male, and has for long been recognized as belonging to the same species. The fine collections made by W. A. Lamborn in the Lagos district shows that the female *alciope* here mimics two or three species of *Planema*, but especially the males of *P. alcinoe*. The orange bar of the forewing is narrower and duskier in tint than in Eastern Uganda, while the hind wing is also dusky orange, fringed with dark grey-brown, almost black. There is considerable variation, as shown by very large numbers of families bred by W. A. Lamborn. The non-mimetic male, on the other hand, is very constant and almost precisely like the equally constant male in Uganda.

The great tropical forest of Africa stretches from the west coast to the western borders of Uganda, and here, in the Semliki Valley, a small but intensely interesting collection was made by S. A. Neave. The western model of the female *alciope* continues right over the whole area, but instead of being one of the commonest *Planemas*, it becomes in Uganda relatively rare, and instead of having its influence strengthened by the presence of other species with a similar pattern, it is accompanied in Uganda by the far more abundant white-barred *Planemas*, *macarista* and *poggei*. This change appears to begin about at the Semliki Valley, and it is precisely here that many of the females of *alciope* exhibit faint traces, especially upon the under surface, of the white bar that becomes so conspicuous a feature in eastern Uganda. Not a single specimen with a strongly marked bar was collected by Neave in the Semliki Valley, and it is, therefore, impossible to regard these intermediate females as hybrids between the western and the Entebbe forms of female. The transitional geographical area where the white-barred *Planemas* begin to appear yields the transitional females—the steps by which these mimics transfer their allegiance from the males of the *Planema alcinoe* type to that of the male *P. macarista*.

It is impossible to regard the resemblance of an *Acræa* like *A. alciope* or *A. althoffi* for a *Planema*, also belonging to the specially protected *Acræinæ*, as anything but examples of Müllerian Mimicry, or Common Warning Colours; and this conclusion is even more obvious when the rarer species of *Planema* assume the patterns of other *Planemas* when they enter the areas where these predominate.

The final illustrations, taken from the genus *Charaxes*, introduce a novel aspect of butterfly mimicry; for both models and mimics are regarded by C. F. M. Swynnerton, who has observed and experimented with birds and their insect prey for many years, as among the most palatable of insects. Yet there can be no doubt about the fact that the large species of *Charaxes* are mimicked by the smaller ones, and that some of the larger species mimic each other. Mimicry in the African *Charaxes* has been chiefly studied by Dr. Karl Jordan, who has most kindly suggested many of the examples that have been used as illustrations. These resemblances have also been observed in the field by G. A. K. Marshall, C. F. M. Swynnerton, Rev. K. St. Aubyn Rogers, and S. A. Neave, who also recognized a particularly striking example in the collection made by C. A. Wiggins in the north-east corner of the Victoria Nyanza. Nevertheless, in spite of this general recognition, very little has been published or exhibited on the subject.

The genus *Charaxes* is confined to Africa and the adjacent islands, its place being taken in the Oriental and Australian regions by the closely allied genus *Eulepis*, in which, however, mimetic resemblances have not been recognized. The butterflies are strongly built and extremely powerful in flight, the thorax containing the wing-muscles being specially capacious. They are strongly attracted by fruit, exuding sap, and putrid substances, becoming after a time almost entirely engrossed in their food, although up to this point remarkably alert. C. F. M. Swynnerton informs me that before alighting to feed they will fly backwards and forwards about the spot, displaying the upper surface of the wings: and that when they have alighted they will sometimes open their wings with a snap when disturbed, although a more usual response is to move the hind wings in such a manner as to display conspicuously the "tails" and exhibit the "eye spot" which, in many species, is found on the under surface of the fore-wing where it is normally covered by the hind, when the wings are held upright over the back in the attitude of rest. The meaning of these movements, diverting attention from the vital parts to the borders of the wings, is rendered clear by the frequency with which injuries are found in these positions.

When a *Charaxes* is seized its great strength enables it to struggle violently, and the effect is almost certainly intensified, in the larger species, by the serrated front edge, or costa, of the fore wing. In addition to this "fighting weight," Swynnerton finds that their chitinous exo-skeleton is so tough that an insectivorous bird will often abandon a *Charaxes* after spending perhaps twenty minutes in the vain attempt to remove the wings. Repeated observations have convinced him that, with alertness and power of flight, fighting weight and toughness of integument, *Charaxes*, in spite of its palatability, gives to its smaller enemies such an unpleasant experience that they will tend to avoid a repetition of it, except under stress of hunger, and that on this account the mimetic resemblance is advantageous. It should be added that the behaviour of large and powerful insectivorous birds shows that *Charaxes* is undoubtedly palatable. Furthermore, Swynnerton finds that the thorax, after removal of its integument, is much appreciated by the smaller birds.

Mimicry in *Charaxes* is confined to the upper surface of the wings, and is principally, although by no means exclusively, manifested by the females. It would be inappropriate to draw up a list of species on the present occasion, but reference must be made to the wonderful example of *C. etheocles*, of which some females mimic the males, others the females, others again both males and females, of larger species. In spite of all this mimicry, *etheocles* is one of the most abundant species in Africa, and its non-mimetic male is itself mimicked by a still smaller, and probably weaker, species on Ruwenzori. These facts, as well as the mimetic approach between the larger species that are mimicked by the smaller, are evidence that the resemblance is an advantageous advertisement of protective qualities held in common, although in different degrees, by models and mimics—that it is Synaposematic or Müllerian Mimicry.

[E. B. P.]

WEEKLY EVENING MEETING,

Friday, March 12, 1915.

Sir James Crichton-Browne, J.P. M.D. LL.D.
D.Sc. F.R.S., in the Chair.

Sir Rickman John Godlee, Bart., K.C.V.O. M.S. F.R.C.S.

Back to Lister.

When Sir James Crichton-Browne, amongst whose many charms is a singular felicity of phraseology, invited me to undertake this task, he kindly supplied the title. It sounded rather startling, but as I could not think of a better, I humbly accepted it. It shows that I have no great discovery to announce, no new theory to propound, but only to take you back over old, well-trodden ground, to try to interest you in very technical matters, and to suggest that, in this particular case, reaction has overstepped the bounds of moderation, and that, as in many other fields, the most modern ideas are not always the best.

I will not trace the various steps by which Lister was led to his conclusions about the causes of suppuration and hospital diseases, nor draw a lurid picture of the deplorable mortality from these diseases before the introduction of the antiseptic system of treatment. I must, however, give you a short account of his first antiseptic method, which was founded on the discoveries of Pasteur, and explain in what way and for what reasons he afterwards modified it. If I succeed in making this clear it will be easy to understand how the relinquishment of much that he at first considered essential, but which later discoveries proved to be superfluous, led others to give it up still more—much more than he ever considered it prudent to do. It will then be maintained that what he feared has come true: that the results obtained to-day, good though they are, are not so good as they would be if we were to return, perhaps not altogether, but almost, to those simpler and safer methods which Lister employed at the end of his active career.

Let us begin by trying to place ourselves in Lister's position during the years preceding 1865, when the writings of Pasteur were first brought to his notice. It was that of every thoughtful surgeon in those days. Of all the dealings of Providence with men—remember I am speaking of sixty years ago—not the least mysterious appeared to be the ordinance that the life-giving air, heaven's blessed breeze, without which life cannot be maintained for more than a few minutes, and on the purity of which man's vigour depends, should carry in some unexplained way the seeds of death and disease, being one day the doctor's greatest friend and the next his deadliest foe.

Physicians were quite sure that the acute specific fevers, such as scarlet fever and measles, were carried on the wings of the wind, and few had any doubt that cholera was borne by the same vehicle. Surgeons were equally certain that erysipelas should be placed in the same class as the acute specific fevers, and that the suppuration of wounds depended upon the same agency. It seemed quite obvious to anyone who thought about the difference in the behaviour of simple and compound fractures, that is, fractures with unbroken skin and those which are complicated by the presence of a wound. Except for this complication the fractures might be identical, but in pre-antiseptic days, the presence of a wound was almost certain to lead to suppuration of a serious, if not a dangerous nature.

Old Glasgow students speak of Lister contemplating a simple fracture of the leg; the muscles torn and pulped, the limb swollen and shiny, black and blue, and pointing out to them that all this destruction of tissue and extravasation of blood would be surely and safely dealt with by the kindly influences of nature; but that the admission of the air through the smallest wound in the skin would completely change the prospect; the extravasated blood would soon stink, the injured tissues—bone and muscle—would die, and suppuration would take place, which might possibly infect the whole system. It did not enter into the mind of anyone, therefore, to doubt the morbific influence of the air. It was one of those things which appear so obvious that for a time they form the very foundations of belief, such as that the earth is flat and that the sun rises, matters which in more barbarous times laid sceptics open to the rigours of the Inquisition.

This was still the universal belief when Pasteur's discoveries were made known. Pasteur put the finishing touch to the work of many observers, who, during the first half of the last century, had been striving to find out what there was in the atmosphere which gave rise to fermentations of all sorts, and amongst others to that form of fermentation known as putrefaction. So long as fermentation and putrefaction were looked upon as chemical processes it was natural to suppose that one of the gaseous constituents of the air was the cause. But clear thinkers, like John Hunter, saw that this could not be the case.

There are two surgical conditions that prove this :—

1. If a rib be broken and a sharp fragment injure the lung, large quantities of air may pass from the lung into the pleural cavity, but if the lung be healthy, decomposition never occurs in the putrescible fluid that is always present in the pleura in small quantity.

2. If air passes into the cellular tissue of the body, as it sometimes does after the same accident, or some other injury of the air passages, large portions of the body may be distended by it to an

extent that appears alarming. But again, as the air is effectually filtered on the way, decomposition does not occur and the evil result is only a temporary mechanical one.

Investigators, therefore, began to think that the cause of fermentation must be something solid and possibly living; something so small that it eluded their highest magnifying glasses, and so they adopted different lines of attack.

Some calcined the air, some filtered it, some passed it through caustic fluids. There was an old French confectioner named Appert—Citoyen Appert, in the time of the Republic, who anticipated by many years the work of our modern fruit preservers. He succeeded in preserving all sorts of food in well-corked bottles by boiling them for various lengths of time according to the particular article he was dealing with; and his results were so nearly uniform and so remarkable, from an economic as well as from a scientific point of view, that they attracted the attention of the French Minister of the Interior in 1810, and also of the Académie Française.

Unscientific as these observations were, they gave an impetus to the work of chemists and biologists, who carried out an enormous number of really scientific investigations in consequence. These were repeated by Pasteur, who made countless others of his own, of marvellous ingenuity. The results of his labours in this particular field before 1865 may be given in a tabular form. He showed that:—

1. Putrefaction is a species of fermentation.
2. It is caused by the growth of micro-organisms and does not occur independently of them.
3. The micro-organisms that produce fermentation and putrefaction are conveyed by the air on the dust that floats in it.
4. These micro-organisms can be destroyed by heat and other agencies, or separated by filtration.
5. Certain recognizable organisms produce definite and distinct fermentative processes.
6. All of these organisms require oxygen. Some of them flourish only in the presence of free oxygen (aerobic), others only in its absence (anaerobic). The latter acquire their oxygen from the bodies which, by their growth, they are causing to ferment or putrefy.
7. Many natural animal and vegetable products have no tendency to ferment or putrefy even in the presence of oxygen, if collected with proper precautions and kept in sterilized vessels.
8. Spontaneous generation has never been observed to occur, and thus may be regarded as a chimera.

Now it will be observed that Pasteur's work presented Lister with two great fundamental facts:—

1. That putrefaction is caused by germs which can be destroyed by heat and chemicals and separated by filtration.
2. That germs are carried by the dust in the air.

It is true that both Pasteur and Lister did not fail to recognize that if the air carried the germs it must deposit them upon the surface of everything, and that therefore the surface of every solid and the whole of every liquid must be, or might be, infectious. It is also true that Lister bore this in mind, and acted on the assumption that it was true from the very first. But still it was the air to which he paid and directed most attention—more attention, as we shall see, than it deserved. He probably did not recognize, he certainly did not say, that his precautions with regard to other sources of infection were far more important than those which he took with regard to infection from the air.

The sceptics and cavillers, the believers in spontaneous generation, kept saying, "Show us your germs in the air." They did not doubt that organisms were found in putrefying substances, they could not do that; but they said that they might be accidental, the result of putrefaction, not the cause of it, and asked for proof that germs existed in the air. Pasteur had tried to meet this objection by filtering the air through gun-cotton, which he afterwards dissolved and submitted the solution to the microscope. There were certainly objects which he was satisfied were germs, but the doubters were still unconvinced.

A few years later, about 1869, John Tyndall, whose eloquent addresses on "Dust and Disease" were listened to with breathless attention in this hall, succeeded in showing to the naked eye of untrained amateurs, the existence of, and the amount of, floating dust in any given sample of air, by passing through it a concentrated beam of light. Next he showed that, if the air was left undisturbed, say in a glass flask, the dust settled, and there was nothing for the beam to illuminate. Then he produced the same result by filtering the air, or by raising to a great heat a piece of platinum wire passing through the flask which burnt up the dust. Finally, he proved, by a series of charmingly simple experiments, that what he called optically pure air was incapable of setting up decomposition in putrescible fluids, whereas optically impure air invariably caused them to decompose.

Most of these facts were known to Lister in 1865. All the evidence pointed in the same direction; and therefore, stated in the simplest way, the problem seemed to be to kill the germs which might have gained access to the wound before it came under treatment, and to prevent the air from carrying in others afterwards.

He first applied what he now called the antiseptic principle to compound fractures, injuries which, above all others, were liable to be followed by those hospital plagues—pyæmia, erysipelas, and hospital gangrene.

He had to choose between the three recognized methods of excluding the germs—filtration, calcining, and chemical antiseptics—and he naturally selected the last as the most convenient. The first antiseptic he tried was carbolic acid, and as the crude sample he first worked with was insoluble in water he used it undiluted.

His plan for treating compound fractures was, after cleansing the wound, to sponge the whole of the raw surface with this undiluted crude German creosote in order to destroy the germs introduced by dirt or other foreign material at the time of the accident, or that, as he supposed, had been carried to it by the air. The carbolic acid mixed with the blood, caused an antiseptic crust, which he fortified by covering it with a thin piece of block-tin, and this crust effectually prevented the access of unpurified air to the wound; for he left it undisturbed for days or weeks and painted the outside with more of the undiluted acid from time to time.

I wish to impress upon you that in the earliest dressings he used a very strong antiseptic and did his best absolutely to exclude the germs in the air, and it is important to note that his results were strikingly good, in spite of the fact that the undiluted acid did actually cause a certain amount of superficial sloughing—or death of the tissues—in the parts to which it was applied. As time went on a purer carbolic acid was obtained which dissolved in water, so he abandoned the caustic undiluted acid in favour of a saturated watery solution: 1 part to 20.

He soon extended the treatment, first to abscesses and then to ordinary operation wounds. The old antiseptic crust was soon given up, and various dressings containing carbolic acid or other antiseptics were employed instead. But for a long time he was so anxious about the air that he irrigated the wound with a solution of carbolic acid in water throughout an operation, and took the most elaborate precautions against allowing any air that had not been submitted to the influence of carbolic acid to reach the wound at the changing of the dressings. He used to say that merely taking out a drainage tube without antiseptic precautions involved a serious risk, because the air which rushed in to take its place might carry some speck of dust and a germ along with it.

In his most palmy days, when he was professor of clinical surgery at Edinburgh (1869–77), and Edinburgh was for the time the surgeon's Mecca, he introduced the spray in order to deal still more effectually with the air. In its most highly developed form the steam spray-producer threw a copious vapour, which was supposed to contain one part of carbolic acid in forty of water, that surrounded the whole region of the operation, and, if the room were small, might even fill the whole apartment with a pungent vapour, to the great discomfort of all concerned. He thought at the time that the momentary contact of the dust with the particles of carbolized water in the spray, or the carbolized atmosphere between the drops would be enough to destroy the germs; but in after years he owned that such a result was impossible.

At this period, though he was still using strong antiseptic lotions very freely, his results were yet more remarkable; and in the meantime, relying on the antiseptic principle, he was doing new operations which, without it, he would have considered to be altogether unjustifiable.

In spite of this, as you know, Lister's treatment, though hailed with acclamation abroad, met with a very ungenerous reception in this country. His compatriots could not understand the great principle that underlay it. They said that there was nothing new in it; if there was, it was bad. They said that their results were quite as good as his, or, if they were not, it was only because he paid so much personal attention to his cases. They gave the treatment silly names, showing that they had not grasped the meaning of it; at first it was the "carbolic treatment," now it was the "spray and gauze treatment." This last name was even adopted by Lister's disciples, and thus the spray came to be regarded as a fetish to such an extent that anything which cast a doubt upon its infallibility sorely tried the tender faith of the converted.

But increasing knowledge forced Lister to alter his own beliefs and greatly to modify his practice in the two particulars to which I have especially directed your attention. Thus he more and more diminished the strength of and the amount of his antiseptics; and, having become gradually convinced that infection from the air, far from being the greatest source of danger, was a negligible quantity, he gave up the use of the spray for good and all.

We must now inquire how this came about. Lister always looked upon an antiseptic as a necessary evil, because he saw that anything strong enough to kill germs must damage the living tissues. Therefore in quite early days he tried to do without antiseptics altogether, or at all events to admit nothing of the sort to the wound after the operation was finished. He also recognized from the first that healthy-living tissues had the mysterious power of preventing the growth of micro-organisms; and that this protective power was diminished by anything that lowered their vitality. He further pointed out that the vitality of the tissues varies in different individuals and in different parts of the body, as is illustrated by the well-known fact that healing takes place more readily in infancy than in old age, and in the face than in one of the limbs. The true meaning of this power was not then known. The explanation was given by the marvellous discoveries of Metchnikoff which Lister's own work prepared him to receive, when they were made known to the world.

Everyone now is familiar with the word "phagocytosis," but have you, ladies and gentlemen, really grasped its meaning. It is a sufficiently appalling thought that each one of us, who looks upon himself as a single individual, is only a huge joint stock company. We carry about with us millions and millions of other individual organisms—micro-organisms indeed—but each one endowed at least with life and definite individual functions. Some of them are apparently essential to the success of our bodies as a going concern. Many, even of these, are only waiting for some interference with the vitality of a part, or the whole, of our body, to afflict us with local or general disease, and finally to dissolve us into our elements.

Metchnikoff showed how these malignant organisms are kept at bay. Our very own body is made up of cells, each of which has, so to say, its individual existence and its special functions—almost its special intelligence—a fact which Lister was one of the first to demonstrate. Hosts of these cells—Metchnikoff's phagocytes—form, some of them, our main mobile army of defence. They are not like other defensive cells, such as those of the spleen and bone marrow, confined to their fortresses, though as a rule they keep their allotted positions. But they can move along strategic railways to any special point of attack, and when they have reached it, there ensues a battle royal with the invading army of pathogenic organisms, which they try to swallow, and, if possible, to destroy. If they are successful, health is restored. It may mean the aborting of a cold in the head, the subsidence of a pneumonia, or the rapid healing of a cut. If, on the other hand, they are overpowered by the invaders, the catarrh may extend to the smallest bronchi, the pneumonia may end fatally, or the wound may suppurate with disastrous results.

These illuminating facts turned the attention of surgeons into another direction; and seeing that everyone knew that the vitality of phagocytes must be impaired by any mechanical or chemical injury, the tendency was to reduce the strength of the antiseptics and to trust more and more to the phagocytes. Lister himself used weaker and weaker antiseptic lotions, but he never thought it wise to dispense with them altogether. Others, however, being chiefly obsessed with the notion that chemical antiseptics diminish the power of the phagocytes, maintained that their use should be altogether discontinued, and have adopted, or say they have adopted, a treatment which, though having the same object as Lister's, is supposed to be founded upon a different principle. How far this is actually the case it will presently be our business to inquire.

This was one of the reasons why it began to be said that Lister's teaching, like the Old Testament, was obsolete, and must be replaced by a newer and more perfect gospel.

We must now further consider how far the air is really a source of infection. In the early days it was said that the number of germs in the air was enormous. As a matter of fact, such a statement is still true, as is easily shown by exposing a plate of cultivating medium in any room, say in the most up-to-date modern operating theatre.

It was also taken for granted that the large majority of germs were pathogenic—that is, disease-producing. But very little was known, in 1865, of the varieties of micro-organisms. It is true that

Pasteur had isolated various yeasts or ferments, and had shown that some organisms were aerobic and some anaerobic, and that the words vibrio and bacterium were beginning to be used. But as the time went on two very important facts were made out. First, that only a small proportion of the flora of the air are pathogenic; and, secondly (and this was one of Lister's own discoveries), that great dilution of a septic fluid very much diminished its chance of infecting a putrescible medium. Thus typhoid pollution of the Nile in a few miles ceases to be a source of danger, and one or two septic organisms introduced into a vessel of blood serum will fail to grow. It is really masses of particulate dirt which are dangerous, because they contain colonies of organisms adequately protected from attack. And, if this applies to attempts at infection of vessels containing blood serum, still more does it apply to the dropping of isolated staphylococci and streptococci on to a wound where the greedy phagocytes are lying in wait to devour them. At every operation scores of germs fall upon a wound—hundreds if it be prolonged—but most of them are those of moulds and other innocuous vegetables which have no chance of growing there; and although there is a possibility of an occasional pathogenic organism being among the number, the risk of its developing is so small, that it is now generally considered to be negligible, and that if one or two should escape into the lymph channel, they will never elude the phagocytes in spleen or bone marrow or elsewhere.

I must now explain how the spray helped to prepare Lister for the acceptance of Metchnikoff's discoveries. I said it was unpleasant for all. There were some surgeons to whom it was positively poisonous. Among these was a friend of Lister's, Thomas Keith, the ovariotomist, whose field of operation was, of course, the peritoneal cavity. In pre-antiseptic days he had obtained results that had astonished the world, by dint of great dexterity, devoted personal attention, and scrupulous regard to cleanliness. His success tried the faith of some, but not of Lister, who was aware of the specially high vitality of the peritoneum, and of other anatomical and physiological peculiarities which diminish the chance of survival of germs, but which are too technical for discussion here.

Keith's success was so great that he hesitated to adopt rigorous antiseptic methods, and Lister at first dissuaded him from doing so, fearing that carbolic acid might dangerously interfere with the vitality of the peritoneum. Besides, at that time he did not fully trust the efficacy of the spray. Keith did, however, for a time use all Lister's methods, including the spray; but this seriously interfered with his health, so he abandoned it, and yet, when he gave it up, his results continued to be as good as when he was using it.

The germicidal powers of the peritoneum are great, but they are only in degree greater than those of muscle fat and tissues. And when it was found that other surgeons, some of them keen disciples, were absolutely forced to give up the spray, or give up surgery, and that, when they chose the former alternative, their results were no worse than before, Lister at last came to the conclusion that the spray was unnecessary, even if it had really made an antiseptic atmosphere ; but, as I said before, he now saw that this was impossible. The spray was really only a convenient, unconscious, automatic irrigator. It killed germs on the wound, not in the air, and as such had been very useful during the time when the lesson was being gradually learned that every surgical operation, every surgical dressing, is, in fact, a complicated bacteriological experiment.

The mitigation of the strength of the lotion and the abandonment of the spray seemed to some like lowering the standard. But it was not so. It was not so because the principle remained unshaken, namely, that as organisms are the cause of putrefaction they must be excluded from a wound, or if they had gained access to it they must be destroyed or prevented from growing. Two changes of method had indeed been introduced : weaker antiseptics were employed because greater respect was paid to the defensive powers of the body, and the spray was given up because opinion had altered in regard to the importance of aerial attacks. Aircraft came to be disregarded while it was seen that what may be compared to land and water attacks were far more dangerous. The germs on the skin of the patient, the dirt on the hands of the surgeon, the unpurified sponge, the dried clot on a badly cleaned instrument—these were the dreadnoughts and submarines; these were the sappers and miners, the howitzers and hand-grenades that really decided the fate of the campaign. Thus it became obvious that the precautions taken against such sources of danger, which, though they were adopted from the first, had been overshadowed by the attention devoted to the air-raids, were really of far more vital importance ; and much ingenuity was thenceforward devoted to devising means for purifying the skin by mechanical or chemical means, to inventing gloves which would not impair the sense of touch, and also to proving that there was a radical difference between aseptic and antiseptic surgery.

We now come to the question of what is meant by aseptic as opposed to antiseptic surgery. Aseptic is no new word. Lister employed it quite early in his writings, and, though it is plain to see why he called his system antiseptic, it is almost to be regretted that he did not call it aseptic. It would have prevented the confusing suggestion that, as Hunter and others had spoken of and used antiseptics, his system was nothing new, and perhaps it might have saved us from the still more confusing suggestion that there is some fundamental antagonism between aseptic and antiseptic treatment, though they are really the same.

Those who call themselves aseptic surgeons maintain that they do not employ chemical antiseptic agencies. The idea started amongst the gynæcologists who, as has been shown, were working

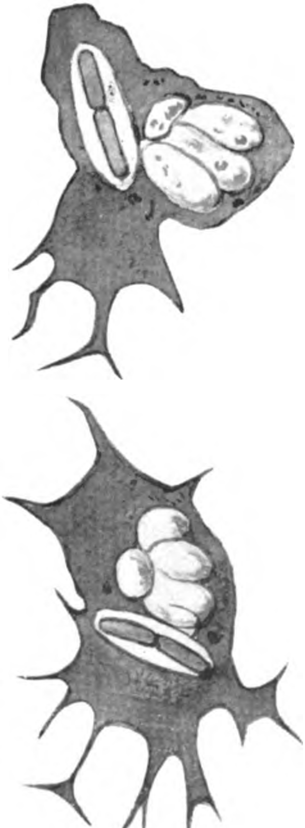

Fig. 1.—A Bacterium stained with Vesuvine in the Interior of a Leucocyte of a Frog. The two drawings represent the same cell taken after a certain interval of time (after Metchnikoff, "L'inflammation," Fig. 54).

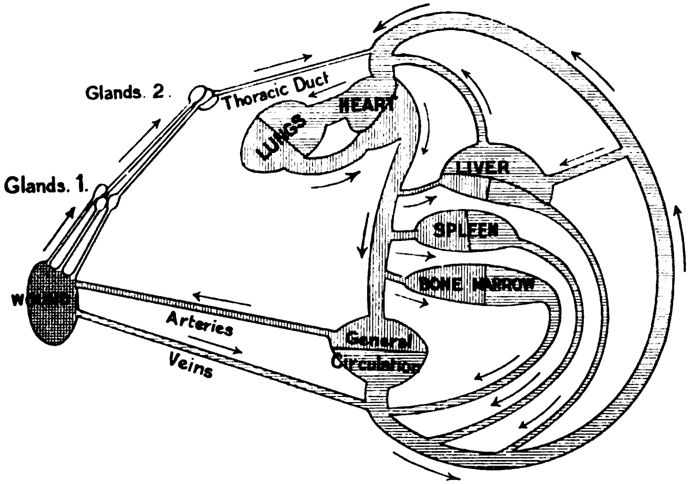
Glands. 2.
Glands. 1.
Thoracic Duct
HEART
LUNGS
LIVER
SPLEEN
BONE MARROW
WOUND
Arteries
Veins
General Circulation

Fig. 2.—Diagram to Illustrate the different Channels by which Micro-organisms enter the Circulation, and the positions in which phagocytes lie in wait for them, i.e. lymphatic glands, lungs, liver, spleen, bone marrow, and the general circulation.

under peculiarly favourable conditions as regards asepsis. They were supposed to use only mechanical means in striving after cleanliness—washings and scrubbings and so forth, which do, of course, remove the deadliest form of danger, particulate dirt. But even these surgeons, or some of them, used to employ freely the most potent of all antiseptics, heat. They boiled their instruments and boiled the water with which they washed out the peritoneal cavity, and some were in the habit of using a pretty powerful antiseptic, sulphurous acid, for purifying those most dangerous things, sponges.

Let us now watch an aseptic surgeon at work. Somewhere in the background there must be a very large sterilizer for superheating overalls, caps, veils, towels, dressings, and bandages; also a boiler for boiling instruments, and an unlimited supply of boiled (he calls it sterilized) water and normal salt solution.

He spends a long time in scrubbing his hands in soap and water, and probably in spirit of wine, which is an antiseptic; he then puts on his sterilized overall, cap, veil, and indiarubber gloves. Thus, converted from a dangerous into a harmless character, he ought never to touch any contaminated object. But it is to be feared that he sometimes forgets the meaning of his vestures: that he wrongly looks upon them as armour, and, inspired by this confidence, he touches the unclean thing, and then puts his fingers into the wound.

One may be forgiven a passing smile at the unreasoning way in which these details are followed out. Surgeons dress themselves up like white-robed priests to examine the external ear, or to explore those parts of the body that no process on earth can render even approximately aseptic.

But to return to our aseptic operation. The instruments, having been boiled, are commonly placed in a tray containing boiled water. Why I have never been able to discover. They would be equally safe if used dry. Probably it is only an imitation of Lister's plan of sterilizing his instruments in a tray of carbolic-acid lotion.

The patient's skin is almost always purified by a chemical antiseptic, sometimes alcohol, but now usually tincture of iodine. The part to be operated on is then surrounded by superheated dry towels, and the operation proceeds. Superheated swabs have replaced marine sponges. No antiseptic is applied to the wound. Plain boiled water or boiled saline solution is used for washing away clots; preferably saline solution, because it does not interfere with the living cells by osmosis as plain water does.

When the operation is over, a dressing of superheated gauze and wool is fixed by a superheated bandage or by plaster. This, of course, only acts as a mechanical filter of dust, but it is now looked upon with reverence, as if it had some other special virtue, and you may see it applied with extraordinary precautions to septic suppurating wounds and kept on for twenty-four hours, regardless of the fact that it becomes a septic dressing immediately after application; that it is, in fact, nothing but a beautifully whitened sepulchre full, if not of dead men's bones, at all events of all uncleanness.

Some aseptic surgeons apply their principle to compound fractures and rely solely on mechanical means for removing dirt from the recesses of the wounds; but most, I believe, continue to employ powerful antiseptics for this purpose, and in this class of injury follow Lister's instructions.

I am not so ignorant or bigoted to suggest that so-called aseptic surgery is a bad way of treating wounds. I know that first-rate results are constantly obtained by means of it. But I maintain, and here I am repeating what Lister has often said to me—

1. That it is a troublesome and difficult process, involving for successfully carrying it out an amount of paraphernalia and an amount of training that puts it almost beyond the reach of all except hospital surgeons.

2. That it is misleading to set it up in opposition to the antiseptic system, because most of its advocates use chemical antiseptics for one purpose or another, and all employ heat with great freedom.

3. That infection of the skin and consequent later suppuration is more likely to occur than if Lister's methods are followed.

I will now describe one of Lister's operations in his last years at King's College. He did not require a huge autoclave for sterilizing. He did not wear gloves, but he purified his own hands and the skin of the patient by a most potent antiseptic, which was called the strong mixture. It consisted of 1 to 20 carbolic acid in 1 to 500 corrosive sublimate. Lister, fortunately or unfortunately, had a very resisting skin; his hands, however, were usually rough, but he did not seem to mind. This was not the case with many of his followers, who could not, or would not, stand the discomfort strong lotions produced. This has undoubtedly been one cause of secession. The instruments and the sponges (he used marine sponges) had been long steeped in strong carbolic acid lotion, but during the operation they were wrung out of a very weak antiseptic solution. The towels placed round the field of operation were also carbolized. No irrigation was carried out. When the operation was complete, a dressing containing an antiseptic substance was applied.

His treatment of compound fractures was never modified after the first few years.

The differences you see are these:—

1. Lister trusted to a potent chemical antiseptic, instead of to mechanical cleansing and heat, for sterilizing the skin, instruments, and the objects likely to be brought into contact with the wound.

2. He applied a very weak antiseptic in small amount to the wound itself, instead of boiled water or normal saline solution.

3. Instead of plain gauze he used an antiseptic dressing, which has the great advantage of acting upon any organisms that after the

operation may reach the surface from the sweat and sebaceous glands of the patient.

These differences between the two systems, as thus stated, do not appear to be great, but they are important. "The little more, and how much it is! and the little less, and how far away!" For in one there is simplicity and safety, in the other complication and risk. Thus in the first place sterilizing by a chemical antiseptic involves no special apparatus. It can be carried out in the humblest cottage. Secondly, the use of an antiseptic during the operation does away with the necessity of the surgeon and his assistants, young dressers, or busy country doctors, or nurses being constantly on the *qui vive* lest their simply aseptic garments should touch some unpurified thing and then convey defilement to the wound. Thirdly, security is provided for against secondary infection from the skin; or secondary infection through dressing, if the discharges should soak through to the surface and thus supply a neutral track for the germs to travel along.

When I come to speak of results I must necessarily be vague. I have, however, formed the opinion, from much observation and long experience, that the standard as regards suppuration amongst aseptic surgeons is lower than Lister's used to be. If a case "goes wrong," as the common but mischievous expression is, it is looked upon as an unavoidable accident, not as a disgrace. In recent years the term "stitch abscess" has sprung into use. Lister said he never had stitch abscesses. Forgive me for dwelling on them for a moment. The name is applied to suppurations occurring about stitches, and they arise in this way. Our skin is full of small glands for supplying greasy material to the hairs. Organisms are present in these glands, and any weakening of the tissues gives them a chance of doing mischief, the commonest illustration of which is the ordinary boil that starts from the fretting of a cuff or a collar. A tight stitch, or a not very tight one, may do the same. But the risk is very much diminished by thorough purification of the skin, which is much more effectually obtained by following Lister's than by "aseptic" methods. It may be granted that stitch abscesses do not often lead to serious mischief, but they often cause troublesome suppuration in the wound itself and involve delay, and the removal of important deep stitches, which were intended to be permanent. This risk is also lessened by using an antiseptic as opposed to an aseptic dressing. I confess I have never been able to understand the objection to an antiseptic gauze dressing. If the wound heals by first intention, the antiseptic substance cannot possibly act upon the raw surface of the wound. Antiseptic gauze has all the advantages of sterilized gauze except that it is slightly more expensive and has none of its disadvantages; in fact, I am confident that it has only been discarded on purely theoretical grounds.

To what extent, then, should we return? I would not urge the use of marine sponges, because they are difficult to clean and expensive, and because cotton swabs are equally efficient. Nor would I recommend the giving up of indiarubber gloves, although I know that they have their dangers, and although I know that they may safely be dispensed with by an antiseptic surgeon. Boiling instruments, I think, should be continued. Otherwise I say that it is only fair to students to tell them, and show them, that by carrying out Lister's technique, it is more easy to obtain the very best results and less likely to fail.

I have only spoken of civil practice, but I would not have ventured to offer you a surgical address, and I could not have hoped to attract your attention if it had had no bearing upon the war which is now raging.

Lister's faithful followers had not only watched with regret what they considered to be a retrograde step in civil practice, but they feared that a war would bring out in relief its weak points.

Wars have occurred of late in distant parts of the world, but we paid little heed to the details. It required a war in our midst, with our own flesh and blood in the trenches exposed to the bullets of the foe, and to the pestilence that walketh in darkness, to bring the matter really home to Englishmen. And when it came, the reports from the front of almost universal sepsis made us fear that our prognostication had come true, and that the abandonment of antiseptics was, at least in part, accountable. It appears, however, that antiseptics are being very largely used, though with most disappointing results.

The fact must not be overlooked that military surgery is exceptional, and that this particular war is being fought in most exceptional circumstances—trenches dug and monster shells exploding in the highly-cultivated soil of a noted tetanus area like the valley of the Aisne. It is almost impossible for us at home to appreciate what is going on. Listen, therefore, to this graphic extract from a letter I received last week from Sir Anthony Bowlby:—"In this trench warfare you must remember that, if a man is hit, he often falls into filthy mud and water, which may be 3 ft. deep or more. Remember also that the trench is only 2½ ft. wide. *If it is night*, you can only grope about in the dark and can do no dressing of any kind, for you can't even get any clothes off in the dark, and in so cramped a space, and you must try to get the man away to a 'dressing station' half a mile distant, and thence to a field ambulance. *If it is daylight*, you can't get the man out of the trench at all, and he may have to be kept there for many hours, because he would certainly be killed if he was got out of the trench. And the water in the trenches is hopelessly polluted, and soaks his clothes and his wound. It is only too evident that large lacerated wounds, and especially bad bone smashes, are so contaminated that it can never be possible to render them aseptic."

This is a dreadful picture; but all fighting is not trench fighting, and we might have hoped not to be told that all wounds, except bullet wounds, are septic, seeing that a few, at least, come under treatment within a reasonable period, say twelve hours, after infliction.

I have been helping Sir Watson Cheyne to lead a crusade in favour of applying to these dirt-infected wounds Lister's original method of purification by means of undiluted carbolic acid. Not that I advocate it for civil practice, but because I think that the only chance of destroying the organisms of tetanus and gas gangrene, and the best way of dealing with streptococci, is to use the most potent and safest antiseptic which is at the disposal of the surgeon for the purpose.

The suggestion met with the cordial approval of those who, like myself, have employed this agent extensively. But it excited a certain amount of loud criticism from others, who were chiefly impressed by the fact that undiluted carbolic gas produces a certain amount of sloughing. This criticism is easily met, because, if employed with the discretion that may be expected of reasonable people, the slough is unimportant, superficial, and antiseptic—incomparably less suited for the growth of micro-organisms than the extensive and spreading sloughs which they themselves produce. Moreover, these antiseptic sloughs do no harm, but are quickly absorbed if the wound heals antiseptically.

That is not the weak point in the argument. There is, however, another, more difficult to answer, of which our opponents did not make so much. Unfortunately, it cannot be denied that the spores of the bacilli of tetanus resist the action of even undiluted carbolic acid for a very considerable time. Moreover, my colleague, Dr. Thiele, has shown that, if easily recognizable micro-organisms are injected into the subcutaneous tissue, they enter the lymph channels, and may pass into the circulation in the course of a few minutes, long before there is a chance, if the same thing happens in contused wounds, of getting at them with our antiseptics.

Are we, then, to abandon this line of treatment, to fold our hands in despair and say that it is useless to attempt to disinfect any wound in civil or military practice to which spore-bearing or other organisms may have gained access? By no means. All Lister's work cries out against such a conclusion. We must clear our minds of the delusion that carefully planned experiments on guinea-pigs in the laboratory are on all fours with unrehearsed experiments in factories, or on wounded soldiers in the battlefield. Lister did undoubtedly succeed in stopping sepsis in compound fractures and in banishing tetanus and hospital gangrene from his wards, and the experience of generations of surgeons working with a saturated watery solution of carbolic acid has confirmed his conclusions. Surely, then, it may be possible to find a practical way of applying the principle to military practice.

It will not, however, do to brush the argument aside in this way without examining it and answering it. I therefore submit for your consideration a suggestion as to what happens in a case successfully treated antiseptically, that is, one that heals without suppuration.

We do not know for certain in what form the anaerobic spore-bearing organisms are introduced into a wound. I, at all events, do not know of scientifically conducted observations upon the subject. But it is clear that they must be either in the form of bacteria, or spores, or both, probably embedded in decaying organic matter in the soil. If the antiseptic reaches them it destroys the bacteria, together with the other septic organisms. But possibly, or let us say probably, it has not sufficient time before becoming too diluted to kill the spores, though it may for a while inhibit their power of development. There, then, these spores lie dormant between a thin layer of carbolized slough and a mass of more or less carbolized blood clot. If sepsis is avoided, the phagocytes at last invade both the slough and the clot, and healing takes place without suppuration. What is the fate of the spores?

We must suppose that, when the carbolic acid has disappeared, as they are embedded in a warm, airless nidus, they germinate, but only to find themselves surrounded by active phagocytes which, if they form a suitable pabulum, destroy them. And so the matter ends so far as the wound is concerned.

This, I maintain, is a reasonable hypothesis and a sufficient answer, and fortunately it is rare indeed, if it ever happens, for tetanus to occur *after* a wound has healed without any suppuration at all; and, moreover, the abolition of tetanus in civil practice has coincided with the reduction in the number of suppurating wounds.

As a further support of the argument, let us now consider the case of the bacteria and spores of tetanus or gas gangrene lying together in an untreated wound, or in one to which less powerful antiseptics are applied, antiseptics not strong enough to destroy the pus-producing organisms. They find themselves, therefore, amongst extensive septic sloughs and decomposing blood-clots in which the phagocytes, if they can penetrate at all, have their time fully occupied with myriads of other organisms, which first weaken and then overpower them. Is it to be wondered at that lockjaw and gas gangrene affect a certain proportion of such cases? On the contrary, the marvel is that these complications are not more frequent.

For, after all, though tetanus and gas gangrene are ghastly things, and make a great impression because the laity knows about them, these diseases are not by any means the most deadly or the most frequent. Probably 40 per cent of the wounds at one part of the campaign were infected with tetanus, but, up to date, there have been reported only 207 cases, of which only 2 have occurred since January 20. And it must be remembered that there are effectual ways of dealing with both diseases—tetanus by the prophylactic administration of antitetanic serum, and gas gangrene by very free incisions.

No, the deadliest enemy is the ubiquitous Streptococcus, the foe that kills more than shells and bullets. But as it is certainly killed by undiluted carbolic acid or by a 5 per cent solution in water, there is at least a chance that, so far as Streptococci are concerned, wounds may be disinfected even in war, and if these and the other pyogenic organisms are destroyed, there is, as I have shown, great reason to suppose that sporing anaerobes like *Bacillus tetani* would have no chance of growing.*

But it may be said: "What about the organisms that have entered the lymph channel and the blood current? What is the good of trying to purify the wound if they have already given us the slip?"

Let us see precisely what Dr. Thiele says, and remember that his experiments are conducted by injecting cultivations of micro-organisms subcutaneously. He maintains:—

(1) That they travel quickly to the nearest lymphatic glands, where they are retarded, perhaps killed.

(2) If not, they make their way along the thoracic duct to the jugular vein and enter the blood stream, and by that channel are conveyed, a few at a time, not in sufficient numbers to be detected by the microscope, to the bone marrow, the spleen, and other parts where groups of cells of the phagocyte class are ready to deal with them.

(3) If the enemy overpowers all these means of resistance they may invade the blood in large numbers and cause general blood-poisoning.

(4) That some are quickly taken up by the blood without passing through the lymphatics.

I must again point out that, in spite of all these alarming facts, general septicæmia probably never occurs if the wound heals without suppuration. To take another simile from the war, the germs that escape into the circulation are like enemy aliens, prisoners of war, or the struggling Turks who crossed the Suez Canal. It is not they, but the main body on the fighting line—that is, the wound—who are engaged in manufacturing the deadly toxins. If they can be annihilated, there is not much fear of mischief from the enemy in our midst. It has never been suggested that germs which have entered the circulation from the wound go back with their ill-begotten progeny to make it suppurate. The argument, therefore, that it is useless to try to make wounds aseptic because some germs have already escaped into the circulation is no stronger than that founded on the resisting powers of spores to the action of antiseptics. Both are the arguments of the bacteriologist rather than of the practical surgeon, and, whilst being treated with all respect, they must not be estimated above their true value.

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* In using the word anaerobe I desire to own that I do not completely understand its meaning. It certainly has been used in more than one sense, and to-day bacteriologists are not agreed about the effect of oxygen on anaerobes, their need for it, the sources from which they obtain it, and other points. We cannot divide micro-organisms by a clear cut line into aerobes and anaerobes.

I cannot hope that your patience is not exhausted. But I trust that you are now convinced of the real danger that may result from neglecting Lister's teaching both in civil and military practice.

As to the latter, which for the moment occupies almost all our thoughts, I should be the last to say that there is only one way of salvation—that, for example, corrosive sublimate is dangerous, or iodine untrustworthy, or peroxide of hydrogen of little value. But I still think that undiluted carbolic acid is, according to our present lights, the antiseptic most likely to be practically useful in the rough-and-tumble practice of the battlefield.

And whether or not this may turn out to be the conclusion of our gallant brethren at the front, I would add that the experience of the present war is one of the strongest arguments for rallying to Sir James Crichton-Browne's battle-cry, "Back to Lister."

Long ago it was prophesied that science would stop war by making it too horrible. Are we nearing that blessed result? One fact stands out in spite of the faint hopes I have expressed—that nothing can stop sepsis in war except stopping war altogether.

[R. J. G.]

WEEKLY EVENING MEETING,

Friday, March 19, 1915

His Grace The Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

Professor G. H. Bryan, Sc.D. F.R.S.

The Piano-Player—Scientific Aspects.

There are very few modern inventions which have not been employed either directly or indirectly in the present war for the destruction of property and of human life. The pneumatic piano-player is one of the few conspicuous exceptions. It is also exceptional in several other ways. While other discoveries have been discussed and described in considerable detail in scientific, engineering and popular books, periodicals and papers, the piano-player possesses but a scanty literature outside the catalogues of the manufacturers. Again, the piano-player has never been associated with any inventor of distinction, and the general public knows nothing about its history. The aeroplane on the other hand is closely linked in popular thought, not only with a large number of modern names such as Wright, Langley, Farman, Blériot, and Cody, but also with the names of many designers and projectors of aeroplanes in the days when aviation was impossible, such as Dante of Perugia and Leonardo da Vinci. Yet, considered merely from an engineering point of view, the modern piano-player is a marvel of human ingenuity.

Almost overshadowing Mozart's house at Salzburg is an organ played by clockwork, for which Mozart composed several pieces. I have a music-roll of one of these which is also interesting from being one of the early attempts at roll cutting by Mr. J. H. Morrison, who carefully marked out the notes and chopped out the perforations with a hand punch, with many erasures and corrections.

The feature which distinguishes the modern piano-player from its predecessors is the element of controllability, which leaves the interpretation of the music largely under the control of the performer. Of recent years, however, accent devices, automatically operated, have been added. The construction of these devices is essentially an engineering problem, and their working as well as all the other engineering details of the modern player will be willingly explained to any one who visits one of the principal music warehouses. The separation of the scale into two independently controlled halves was certainly to be found in the old mechanical pianotist in 1902, which was played with thick cardboard or canvas rolls, and it may date from still earlier. I consider this feature unnecessary, and its use undesirable on account of its very limited capabilities.

The object of my experiments has been to apply dynamical and physical principles to the control of what I will call the striking action in piano-players, as it very soon appeared to me probable that by so doing it would be possible to obtain differences of effect that could not be produced by purely mechanical methods, and in particular to acquire a certain amount of independent control over the different parts of a chord the notes of which reached the tracker board simultaneously. Naturally these experiments have been regarded by the average "practical" expert with the same kind of scepticism and opposition as my earlier attempts to advocate the necessity of applying the principles of rigid dynamics to the stability of aeroplanes. For this reason it is necessary to discuss the problem in great detail here.

The ordinary practical man persists in asserting that in striking a note or chord on the piano everything depends on what he often inaccurately calls the "force of the blow." He says that you can increase or decrease the force of the blow, but that the only effect will be to play the whole chord louder or softer. He says that you cannot bring the bass or treble parts into prominence unless you connect the pneumatics with different degrees of vacuum, and it will be found that many commercial music rolls have the chords cut in very objectionable arpeggios in order to enable an air valve to be opened or some equivalent increase of pressure effected between the playing of one note of a chord and another. I have been told over and over again that it is mathematically impossible to produce the effects which I am continually producing on my player at Bangor, and without which I regard no piano-player as worth playing.

Now any mathematical physicist will understand that what these engineers call "force of the blow" is in reality a very complex phenomenon. In my piano I find that a fairly soft note is produced *in the middle of the scale* when the hammer strikes the strings with a velocity of 30 cm. per second, and that the hammers themselves rise through a height of about 5 cm. This means that *if the acceleration were uniform* the operation of depressing the key and releasing the hammer would occupy one-third of a second. But during the operation the pressure applied to the key may be varied in an infinite number of ways. It may be increased or decreased. It may be made very large at the commencement of the blow, sinking to zero at the end, as when a finger-pianist strikes the note from a height; or it may be very small at first and gradually increased, an action which some describe as a "caressing" touch. But the check action, which for a horizontal piano was first invented and patented by Erard, and for which equivalent devices will be found in the vertical piano, greatly influences the character of the accelerating force impressed on the hammer when regarded as a function of the time. The operation of playing a note is thus divided into two periods. At the end of the first period a support is withdrawn from the palet which raises the hammer, and the latter becomes disconnected. In the second period either the hammer may fly up freely and strike the note, or the palet may again overtake it and drive it forwards. To produce these different actions the variable forces applied to the key of a piano must evidently be similar in character to the "screw-back" and "following" strokes in billiards. It is, of course, theoretically possible that the hammer may fly up and strike the string during the first period, may rebound before the end of the second period, and may then be projected against the string a second time. The evidence I have obtained from outside sources seems to be negative, but certain results that I have obtained seem to indicate that this effect may occur in pneumatic playing. The mechanism of the check action may be such as to prevent the hammer from rebounding and thus striking the string twice when the action is at rest, but yet it may be capable, when in motion, of returning the hammer to the string, after the fashion of a tennis or cricket ball struck by a bat in motion, which would not rebound with sufficient velocity if the bat remained stationary. But it is very difficult to test this point.

Another important point is that, as the driving force is applied close to the base of the hammer, considerable flexural vibrations are liable to be set up in the shaft. These may probably differ in amplitude and phase, according to whether the hammer is released during the first or second period, and it is clear that the effects produced in striking the string may depend on whether the shaft is bent forwards or backwards at the instant of impact, and on the amount of bending, if any. A difference in this respect would affect the tone-quality of the note.

Now the bass and treble hammers of the piano differ considerably in weight, and therefore also in inertia, and the intermediate hammers vary in this respect continuously from one end of the scale to the other. It follows that when different parts of a chord are operated on by the same pneumatic tension, the treble notes will require less time to be set in motion than the bass ones. A short sharp blow will thus produce its greatest effect in the higher parts of the scale, while a longer sustained blow, or an increasing blow, will drive the bass notes forward with increased velocity even after the treble notes have been released.

When I first started these experiments, it appeared to me probable that the effects thus obtainable would be small, and that a very difficult effort would be required to produce them. The contrary proves to be the case. A very slight touch of the finger on a controlling lever may produce differences so marked as to appear paradoxical. A sudden short, sharp cut-off of the pneumatic tension often causes a note to "sing out" when it would not do so otherwise. What appears to me most remarkable is the amount of difference that it is possible to make even in comparatively short chords.

In the case of a repeated treble note with bass accompaniment, the second time it is struck accentuation is more difficult, and there is considerable danger of the note failing to sound owing to the bridge between the two notes buckling, thus continuously admitting air to the primary valves, and preventing the piano action from returning to its normal position. The best plan is to keep the tension low till the chord containing the repeated note has passed the tracker board, and then force the notes down hard. This appears to project the hammer of the repeated treble note at the second period of its ascent and often causes it to ring out above the bass. This action is exactly the reverse of what is necessary in the case of a non-repeated note. On account of the additional difficulty thus incurred in reading the music, I believe that it would be justifiable to cut the troublesome note a little after the accompanying chord, much as I object to this plan in other cases.

In order to understand the device by which these differences of effect can be easily produced, it will be necessary to have a clear idea of the working of the regulating bellows and accent lever which is to be found in different modified forms in almost every pneumatic player. In addition to the suction bellows which generate the vacuum, there is a large reservoir bellows, controlled by springs, which acts as an accumulator or condenser of considerable capacity. But between this part of the instrument and the playing pneumatics two channels of communication usually exist. One is through an accent valve A controlled by a lever (and in many modern instruments there is a third passage through an accent valve operated by punch-holes in the music). The other connection is through a smallish regulating bellows, which I will call B, controlled usually by a spring, and through an air valve connected with this bellows which opens or closes with it according to the degree of vacuum. This arrangement is sometimes called a "choker," or "equalizer," since its statical action tends to counteract any increase or decrease in the pressure of the air which is being withdrawn from the playing pneumatics.

Now it will be found that in playing the regulating bellows is in a continual state of vibration, and that this vibration has a very marked effect upon the tone-quality and expression. If it is attempted to produce accentuation and other effects by means of the accent valve and pedalling, the effects are liable to be largely modified by the induced vibrations (if I may so call them) set up in the bellows.

The patented device used in my experiments, and subsequently made by the Motomusic Co., Edinburgh, consists in controlling this regulating bellows by means of a lever carrying a sliding weight, the usual spring being removed. The sliding weight acted originally on the principle of the common steelyard, thus enabling the tension on the bellows to be varied from time to time in playing different passages. It is convenient to describe such changes as "sub-permanent." Further, the lever itself could be controlled by hand, and could have upward or downward pressures instantaneously applied to it for the purposes of accentuating individual notes, such changes being describable as "temporary." The usual expressson marks FF, F, MF, P, and PP, were placed against the different positions of the sliding weight in order to enable a beginner to obtain increased range of contrast by following the indications on his music.

The experiments lead to the following conclusions:—

1. By varying the sub-permanent tension independently of the pedalling, differences of effect are obtainable that cannot be produced by the ordinary methods of control. A light sub-permanent tension with fairly strong pedalling will give bright treble effects with light bass. A heavy sub-permanent tension with light pedalling produces a strong bass and a soft treble. These effects are undoubtedly due to the closing of the regulating bellows being accelerated in the one case and retarded in the other, thus varying the duration of the maximum tension and the character of the striking action.

2. Corresponding to every note of the scale, there is a position of the sliding weight which produces the maximum effect, this position varying continuously from one end of the scale to the other. The theoretical explanation of this experimental result may be that we have to deal with two systems which closely resemble each other, one consisting of the regulating bellows and loaded lever, the other of the playing pneumatic and pianoforte hammer. The maximum effect naturally occurs when their motions synchronize.

3. The brightest effects are obtained by keeping the accent valve open or partly so, as in this case the closing of the regulating bellows is affected by the pedalling. The effect of closing the accent valve is very similar to the use of the soft pedal. It is, however, sometimes necessary to use this valve for accent effects which are otherwise difficult to produce—i.e. bringing out single notes from the midst of a heavy accompaniment. In any case the spring of the accent lever is best removed so that it will rest in any position. Even in these cases the expression device greatly improves the accentuation, as there is far less risk of the regulating bellows rebounding and crashing out notes following the accented one.

4. In playing solo passages, light and heavy sub-permanent tensions produce different effects even when it is sought to maintain the corresponding degree of loudness by suitable variations in the strength and method of pedalling. This observation led me to believe in the existence of a relation between tone-quality and touch, which has since become the subject of a lively controversy. The

differences have, however, been noticed by most people to whom the experiments have been shown. While it is easy for an inexperienced person to produce the necessary differences of touch with a pneumatic player fitted with this controlling device (the only thing necessary being to shift the sliding weight, or other sub-permanent control), I find it very difficult to obtain the same effects by striking the keys of an ordinary piano with my fingers.

5. There appear to be two different ways of accenting particular parts of chords by hand pressure applied to the control lever. With a slight sub-permanent tension, treble notes are usually best accented by depressing the lever before the note has reached the tracker board, and subsequently allowing it to fly up smartly. With a heavy sub-permanent tension, it is necessary to jerk the lever upwards from below just after the note has reached the tracker board. For a bass note with light tension, the lever is firmly pressed down after the note has reached the tracker. With heavy tension the lever is previously raised, and then allowed to drop down with the note. In either case the action is supplemented by a corresponding action in pedalling.

6. With a heavy sub-permanent tension and the lever supported from below, it is possible to obtain very soft effects in which the treble parts ring out clearly and are not drowned by the bass. With a light sub-permanent tension and the lever pressed down the results are more brilliant. I attribute these differences to the inertia of the controlling weight, which has more influence in retarding the oscillations of the regulating bellows when acting at a long leverage than when acting at a short one.

7. There is a great satisfaction in being able to slam down a vigorous chord, hand and foot working in unison.

8. In the earlier experiments the connexion between the lever and the bellows was made firstly with strings and tapes, and subsequently with wires passing over pulleys. It was found, however, that the stretching of these connexions greatly interfered with the effects and led to the production of harsh results. That such a cause as the stretching of a wire should have so marked an effect clearly shows how small differences may have very great influence on the playing of a pneumatically operated piano.

9. Contrary to the usual belief, I obtain the best accentuation effects in parts of chords when the notes reach the openings in the tracker board at exactly the same instant. When they are cut unevenly it is necessary to make allowances for the irregularities, and sometimes this is difficult or even ineffectual.

10. With experience it is possible to learn the exact kind of effort required to accentuate a note in any part of the scale, and thus to obtain marked differences between the treble and bass parts of a comparatively short chord.

11. Where a note or chord is repeated a number of times in rapid

succession it is advantageous to hold the controlling lever firmly so as to stop all vibration in the regulating bellows. This not only improves the repetition but it obviates all the strain on the ankles in pedalling, which under ordinary conditions is considerable.

12. In every case the exertion of pedalling is greatly reduced, not improbably by about 50 per cent. This result admits of explanation, but it must be regarded as based on experimental rather than theoretical evidence.

13. Some players have separate regulating bellows for the bass and treble parts. In an experiment kindly arranged by Messrs. Broadwood at Easter 1914, it was shown that this did not interfere with the working of the device, the two regulating bellows being attached to a common connexion. In this case the controlling lever had to be connected with the bellows by a wire over six feet long passing over two pulleys, and yet this did not interfere with the production of marked effects, otherwise unobtainable.

14. A very remarkable result is that a very small effort often produces a considerable difference in the effect. It is possible to emphasize a particular note or chord by a suitable stroke of the pedals alone, and of course, the necessary pressure must always be supplied in this way, but the lightest possible touch of a finger applied to the lever will often produce a conspicuous improvement in the effect. On the other hand, the sudden jerk on the controlling lever often strains the connexions considerably, and I found that even strong wires were very liable to break with the strain, particularly when passing over small pulleys.

15. Experimenting with pianos of different makes, both upright and horizontal, operated by either detachable or interior players, has led to practically identical results.

A comparison of the present results with the subject matter of Professor Tobias Mathay's treatise on "The Act of Touch" will show that the underlying principle is the same in both cases, namely, that differences of expression are obtainable by suitably varying the pressure applied by the finger to the key, or by the vacuum to the pneumatic during the small fraction of a second occupied by the descent of the former or the collapse of the latter. It was not till after my experiments were practically complete that I became aware of Mathay's work, so that the present results were all obtained independently. Much might be said in regard to the physiological aspects of the problem, as a great deal depends on the state of tenseness of the muscles of the hands and feet in playing. I have always maintained that the existing practice of making everything depend on foot control can never lead to satisfactory results, but that the hand must be used as well, and must be made to actually *feel* the changes of tension in the wind chest as is rendered possible by my device. Again, an average critic reading Mathay's book would think he had indulged in an absurd amount of over-elaboration in classifying the different kinds of touch obtainable by the use of different muscles. But really my experiments with a piano-player have led to results which seem to indicate that Mathay may be right after all.

What makes the whole question more difficult to understand is that the causes producing all these different effects must be capable of being represented by differences in the shape of a single graph. If we take, for example, a piano key pressed down by a finger, then there are a large number of quantities associated with the blow, which are recognized in elementary dynamics. These include the time from the instant the finger touched the key, the distance the key has descended, the velocity of the key, its acceleration, the pressure of the finger on the key, the momentum communicated, the energy or work done in depression. But the principles of dynamics tell us that if we draw the graph connecting *two* of these variables, the relations between any other two are fully determined. Differences in the elasticity or tenseness of any particular set of muscles can only affect the shape of this graph. In pneumatic playing the same is evidently the case. And yet such differences *do* seem to influence the result.

Although I have been trying for a long time past to account theoretically for the observed effects, the results are still far short of finality. It is clear that neither Helmholtz's nor Kaufmann's mathematical investigations fully suffice for the purpose. There are, however, other difficulties. One is that, although it is easy to observe differences of effect which are, as a rule, quite conspicuous, it is not so easy to define exactly in what these differences consist. Again, the success of a piano-player as a musical instrument largely arises from the fact that the manipulation of the various controls, both for tempo and expression, soon becomes intuitive. A great deal evidently depends on the elasticity of the muscles of the hands and feet. It would be very difficult to ascertain precisely the effect of differences in this elasticity on the tension in the playing pneumatics while a note is being sounded. In short, while my early attempts at getting improved results with a piano-player were based on dynamical considerations, it appears probable that a complete dynamical theory of the observed effects will involve investigations of no small degree of difficulty. But those who have experience of such matters will probably understand that a still more difficult task arose when it was sought to develop and apply the device for practical purposes. The mere engineering of this control device into a form in which it can easily be adapted to any internally fitted player without interfering with the use of the piano for hand-playing, has given a great deal of trouble to the Motomusic Company, and the process of obtaining evidence from independent sources as to how far the results obtained in different hands justify our claims, still involves work strictly experimental in character for which it is difficult to obtain opportunities in the early stages of an invention.

EDUCATIONAL PROBLEMS.

The average children of player owners will not wish to spend much time in acquiring manual dexterity with scales and five-finger exercises. The exceptional genius who wishes to be free from the limitations of the music roll will still require this practice, but will be in the rare minority. In place of the present "practising," the children will require to practise exercises in player manipulation, and they will begin by learning the meaning of the expression marks on the roll. The exercises will be mainly devoted to :

1. The control of the speed regulator and the acquisition of the sub-conscious or instinctive faculty of playing every note or chord at the desired instant. Practice in accompanying.

2. The production of differences of expression and touch, including those described in this lecture, and the acquisition of the power of accenting parts of chords in any part of the scale. The pupil must not be satisfied until he has learned the exact action corresponding to every note on the keyboard. For school practice in player manipulation automatic accent perforations and separation of bass and treble halves must be forbidden. A school prize should be given for the best rendering of some composition.

The pupil will, however, require some training in the structure of the music, and he should also acquire some familiarity with musical notation. For this purpose there will be needed a scale to be placed in front of the tracker showing the black and white notes, and in addition special rolls marked in such a way as to illustrate :

1. The rulings of the treble and bass clef in the ordinary staff notation.
2. The relative value of semibreves, minims, crotchets, quavers, etc., and the corresponding rests.
3. The distribution of the sharps and flats in different keys.
4. The meaning of such terms as staccato, legato, arpeggio, trills, etc.
5. The lengths of the various musical intervals, such as major third and minor fifth.

Finally, in order to acquire practice in reading music, the pupil may learn to cut his own music rolls. Appliances for this purpose are already obtainable, and will doubtless become common in the future. A school class in roll cutting should prove an efficient form of manual training, and prizes could be given for the best cut roll.

I have recently tested one of these roll-cutting devices, and shall probably use it as occasion arises for acquiring familiarity with compositions performed at our local concerts which are not included in the makers' catalogues. Roll cutting has now become far easier than one would have imagined possible. I believe it may become an invaluable training in learning to read music, as it involves a degree of precision in assigning the proper length to notes and rests, such as is very apt to be overlooked in the attempt to finger out the notes of a difficult composition on the keys of a piano.

[G. H. B.]

WEEKLY EVENING MEETING,

Friday, March 26, 1915.

THE RIGHT HON. LORD RAYLEIGH, O.M P.C. D.C.L. LL.D. D.Sc. F.R.S., Professor of Natural Philosophy R.I., in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. Pres. R.S. *M.R.I.*,
Professor of Natural Philosophy, R.I.

Experiments on Slow Cathode Rays.

ELECTRIC waves and Röntgen rays are two of the most important discoveries in Modern Physics. Though the two radiations differ so widely both in their properties and methods of production, there is very strong evidence in favour of the view that both are vibrations of the same nature as visible light, and that though the wave length of electrical waves might be long enough to reach across London, while that of Röntgen rays is of the order of the radius of an atom, they were all members of the same family. The question arises whether the very wide gap between these extreme cases is filled up continuously with waves of different kinds, or where there are gaps in this long interval. Electric waves have been produced whose wave length is only a few millimetres, and this limit could no doubt be lowered if special experiments were made with that object; on the other hand, Rubens has isolated waves in the infra-red spectrum which have a wave length as long as one-third of a millimetre. There is thus no gap between electric waves and light waves. Is this true for the other end of the light spectrum, i.e. is there continuity between light waves and Röntgen rays? The shortest wave lengths known for light waves are those discovered by Lyman in what is known as the Schumann region of ultra-violet light. They have a wave length of about one ten-thousandth of a millimetre, while the longest wave length yet measured for Röntgen rays is of the order of one-millionth of a millimetre; there is thus a gap of about seven octaves between the two. This gap up to the present has not been systematically investigated, but its study promises to be of especial interest. The rays in the Schumann region and very soft Röntgen rays show considerable approach to each other in their properties. Both kinds of rays ionise gases when they pass through them, while light in the visible region of the spectrum does not do so. Unfortunately, too, for the experimenter, both are so readily absorbed that they are unable to penetrate even very thin layers of air, so that it is a matter of very considerable difficulty to investigate their properties. Both kinds of rays produced strong chemical and physiological effects when their energy is absorbed in the medium through which they are passing. Thus both, as I can easily show by holding starch paper in their neighbourhood, produce ozone; both, too, are disastrous to some of the lower organisms. Indeed, some of the latter under their influence undergo the most disconcerting experience an aquatic creature can suffer, for they gradually, when exposed to these rays, dissolve away in the water in which they are living. The study of the rays in this gap may thus become of high biological and medical importance.

The method I have used to produce rays between Schumann rays and ordinary Röntgen rays is to bombard a metal target with very slow cathode rays. The cathode rays which produce the Röntgen rays in an ordinary Röntgen ray bulb have speeds corresponding to 20,000 volts or so and upwards; the cathode rays I have used have speeds corresponding to from 5 to 2000 volts. It is well known that the greater the speed of the cathode particle the harder the Röntgen rays they produce. I can show this very distinctly by using a special and very convenient form of Röntgen ray bulb—the Coolidge tube—for the loan of which I am indebted to Messrs. Cossar. The essential feature of this tube is that the cathode produces its own supply of cathode particle, as it consists of a tungsten filament heated to redness by an independent heating circuit. The white hot tungsten gives out cathode rays which have no velocity when they start from the cathode; the velocity is communicated by the potential difference between the cathode and anode. As this is increased the velocity acquired by the particles before they strike against the target is also increased. The number of cathode particles does not depend upon this potential difference, but is determined solely by the temperature of the tungsten spiral. The vacuum in the bulb is an exceedingly good one, so good that you do not see the ordinary green phosphorescence on the glass when the bulb is in action. If I hold my hand in front of a platino-cyanide screen, and gradually increase the speed of the cathode particles by increasing the potential difference, you will see that at low speeds very few Röntgen rays reach the screen through my hand; as I increase the speed some get through the flesh, but are stopped by the bones, and at the very highest speeds an appreciable amount gets through the bones themselves.

I will now pass on to consider the production of soft Röntgen rays by the impact of slow cathode rays. The first method was as follows:—Fig. 1 is a Wehnelt cathode; the anode A is a brass rod with a hole bored through it to allow the cathode rays to pass through and strike against the target B; this is a copper plate which is at the end of and in metallic communication with a cylinder of wire gauze. The variation in the speed of the cathode particles is produced by putting between this gauze and the anode a potential difference tending to stop the rays. Thus, if V_1 is the P.D. between the anode and cathode, V_2 that between the anode and the gauze, the energy of the cathode rays when they strike the target is proportional to $V_1 - V_2$. To detect any radiation coming from the target a camera was placed at the end of the side tube T; a slit was put in front of the photographic plate, and half of it covered by thin slices

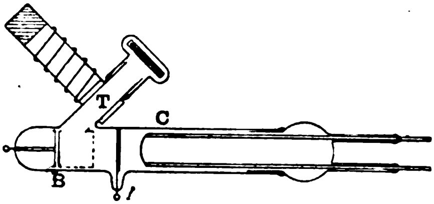

Fig. 1.

of paraffin wax, collodion, mica, glass, or fluorite, so as to be able to estimate the penetrating power of the radiation. A magnet was placed between the target and photographic plate, so as to deflect from the latter any corpuscular radiation from the target. The vacuum was made as low as possible by charcoal and liquid air; it was so low that no luminosity could be detected between the anode and the target. The plates used were Schumann plates. The times of exposure, which varied from one minute to two hours, were chosen so as to make the energy in the cathode rays striking against the target during the time of exposure constant. Thus, for cathode rays which had fallen through 10 volts the time of exposure would be ten times that for those which had fallen through 200; the latter gave dense photographs with an exposure of two minutes. I have obtained photographs with potential differences as low as 10 volts. These are not due to ordinary light coming from the discharge tube, for (1) they are not obtained when the beam of cathode rays is deflected from the target by a magnet; and (2) the rays which produce them are quite unable to penetrate very thin films of glass. To test whether or not they were due to corpuscular radiation from the target two methods were employed. First, a magnet was placed between the target and the photographic plate so as to deflect the corpuscular radiation from the plate; this did not affect the photographs. The second method was to place between the target and the photographic plate a pair of parallel plates and apply to them a potential difference of 1000 volts. The intensity of the photographs was not diminished when all the radiation which fell on the plate had passed through this strong electric field which would have stopped any charged particles. With regard to the penetrating power of this radiation, I have been able just to detect the photographic effect behind a film of collodion thin enough to show the colours of their plate, when the cathode rays had energy corresponding to 40 volts. When the energy corresponds to 80 volts the effect behind the collodion, and also behind a film of paraffin wax 4 μ thick, is quite appreciable, while after 200 volts there is very considerable penetration of the paraffin and collodion films.

This photographic method did not, however, lend itself easily to quantitative measurements. For this purpose I made use of the ionisation of gases produced by these soft Röntgen rays. The rays instead of falling on a photographic plate passed through a thin window into a tube leading into a chamber containing SO_2 or some other gas giving a large number of ions. The quantity of gas was sufficient to absorb all the rays, so that the number of ions produced is a measure of the energy which has passed into the ionisation vessel. Considerable difficulty was found in finding a suitable window, as the discharge tube itself was a very good vacuum, while it was necessary to have a finite pressure of gas in the ionisation chamber. The film had to be stout enough to stand a difference in pressure of a few millimetres of mercury on the two sides, and yet be thin enough to transmit the very easily absorbed radiation. The difficulty was solved by a device due to my assistant, Mr. E. Everett. Very thin films of celluloid were prepared by dissolving celluloid in amyl-acetate and pouring a thin layer of the solution on liquid mercury; this leaves the edges of the films free, if they are held tight the film cracks as it dries. Films prepared in this way were so thin that they showed the colours of thin plates. The other method is to measure the energy of the radiation given off from a target struck by the cathode rays. It is known that when ultra-violet light or Röntgen rays fall on a plate of metal, the plate emits negative electricity, and the quantity of this emitted is proportional to the energy as long as the type of radiation is constant. To apply this method the tube T, instead of leading to a camera, lead to a spherical bulb, silvered on the inside, and at the centre of the bulb there was an insulated plate of metal connected with a Dolezalik electrometer; the silvered bulb was maintained at a positive potential of several hundred volts positive to earth, while the plate of metal at the centre was, before the radiation fell upon it, at the same potential radiation as the earth. When the radiation fell upon it, it lost negative electricity, the amount of which could be determined from the deflection of the Dolezalik electrometer. In order to stop any charged particles passing from the discharge tube into the silvered vessel, parallel plates of metal were placed in the tube T, and a potential difference of 600 volts was maintained between them.

With each of these methods the following results were obtained. The energy in the radiation from the target is very small until the cathode rays have an energy corresponding to about 15 volts. The energy then rises quickly with the potential; then, when the energy is measured by the second method, reaches a maximum, and then decreases very slowly. This refers to the energy in the Röntgen radiation emitted by the target. It is known, however, that when a target is struck by cathode rays, negative electrons moving at a much slower speed than the primary cathode rays are also given out, and the variation in the number of these negative electrons with the voltage of the incident cathode rays has been investigated by Leitz, Campbell and others. This variation is very similar indeed to the variation in the intensity of the Röntgen radiation, for it begins between 10 and 15 volts, reaches a maximum between 200 and 300 volts, and then slowly decreases. The number of electrons does not depend on the nature of the target (carbon targets are an exception). As far as our experiments go at present a similar statement is true for the Röntgen radiation produced by the cathode rays; this does not change much when one metal replaces another for the target.

The close correspondence between the emission of negative electrons and of the soft Röntgen radiation supports a view of the origin of this radiation which I expressed some years ago; it is that the radiation originates in the return of an electron to an atom or a molecule which has been ionized, the kind of vibration depending on the nature of the electron which has been ejected when the atom was ionized. If this was one of the electrons on the surface, the radiation would be in the region of ordinary light; if it was from the innermost ring of electrons in the atom it would correspond to the K radiation; if from a ring not so close to the centre the L radiation; and so on. As each unit of radiation emitted corresponds to the return of a free electron, it is evident that there would be a close connection between the number of free electrons produced by the cathode rays and the energy in the Röntgen radiation due to those rays; as we have just seen such a correspondence does exist. We may hope to gain important information about the atom from the study of these soft radiations, for they are given out by the electrons not far from the surface of the atom, and it is just these electrons which determine the chemical properties of the atom.

[J. J. T.]

GENERAL MONTHLY MEETING,

Monday, April 12, 1915.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Honorary Secretary announced the decease of Professor Emile Hilaire Amagat, in March 1915, and of Professor Otto N. Witt, on March 23, 1915, Honorary Members of the Royal Institution; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the world of Science in the decease of Professor Emile Hilaire Amagat, Officier de la Legion d'Honneur, Membre de l'Institut (Académie des Sciences), Foreign Member of the Royal Society of London, and an Honorary Member of the Royal Institution since 1904.

Professor Amagat was well known for his valuable researches on the behaviour of Gases, Liquids and Solids under very high pressures, and of the conditions of their transition from one state to another.

The Managers, on behalf of the Members of the Royal Institution, desire to express their most sincere sympathy with the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the Scientific World by the decease of Geh. Regierungsrath Professor Otto N. Witt, Ph.D. F.C.S., late President of the German Chemical Society, Professor of Chemical Technology in the Technical High School, Berlin, and an Honorary Member of the Royal Institution for six years.

Professor Witt delivered a Friday Evening Discourse at the Royal Institution in 1902 on "Recent Developments in Colouring Matters." He was distinguished for his original Papers on Organic Chemistry and Chemical Technology, and presided over the International Congress of Applied Chemistry which was held in Berlin in 1903.

The Managers, on behalf of the Members of the Royal Institution, desire to express their most sincere sympathy with the family in their bereavement.

Sir James George Frazer, D.C.L., LL.D., Litt.D.
Lady Frazer,
William F. Higgins, B.Sc.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, No. 92. 8vo. 1915.

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Concrete for March-April, 1915. 8vo.
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Electricity for March, 1915. 8vo.
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Engineering for March, 1915. fol.
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WEEKLY EVENING MEETING,

Friday, April 16, 1915.

J. H. BALFOUR BROWNE, K.C. D.L. J.P. LL.D.,
Vice-President, in the Chair.

STEPHEN GRAHAM,

Author of "The Way of Martha and the Way of Mary,"
"With the Russian Pilgrims to Jerusalem," etc.

The Russian Idea.

THOSE familiar with ideas can tell at sight a German idea, an American idea, a Russian idea, a Roman Catholic idea, and so on. Each nation has its fundamental idea, its mother idea, the idea of which all other characteristic ideas are children. As Dostoieffsky says: "No nation has ever been founded on science reason; it has always grown about some central idea."

It is a remarkable fact that, although Russia is a great composite empire with an enormous number of small nations and tribes under her rule, she is not a country of mixed ideas. Her literature, art, music, philosophy, religion, her theatre, her dancing, is something intrinsically Russian. No Poles, Finns, Jews, Armenians, Kirghiz, contribute to it. No German-Russians contribute to it. Of all the names by which Russia is known as a nation mighty in art and in thought not one belongs to the subject nations. In literature—Dostoieffsky, Turgenief, Tolstoy, Gogol, Pushkin, Chekhof, Gorky, Balmont; in painting—Vasnetsof, Nesterof, Verestchagin, Sierof; in music—Tchaikovsky, Korsakof, Mossugorsky; in philosophy—

Solovyof; in history—Kluchevsky, Karamsim; in contemporary journalism—Rozanof, Menshikof, Doroshevitch, Merezhkovsky; even in Russian science, which is something apart from European science, Mendeleef, Metchnikof, all without exception are Russian names, the names of Russian people at once Christian and Slavonic. Nothing is contributed by Jews; nothing is contributed by Poles; nothing by Finns. These people each have their own characteristic separate literature and religion and art. They think in their own tongues, pray in their own churches, have their own characteristic ideas. There is not the blending we have in England, where we include in our national literature the works of Jews, Poles, Hindoos, of people proud of French origin, proud of German origin, and so on. The Russian idea is something purely Russian.

This is important not merely as a curious circumstance. It indicates the fact that the fundamental Russian idea should be something more easy to unravel, more evident, more mighty than other contemporary ideas. How much more easy, for instance, to determine just what is the national Russian conception of life than to determine ours, obscured and complicated by so many foreign elements.

There is a spirit abroad to-day which calls for the thing called cosmopolitanization, in other words, for that process of the mongrelizing of nations and ideas which is manifest to-day in America. It wishes the breaking down of national barriers—intermarriage. The doctrine seems to be promulgated chiefly by those Jews who have sold their priceless birthright, who have given up the Zionist ideal, and settled down to think they are no longer Jews but Englishmen, Americans, Germans, what not. They talk of the United States of Europe, as if the United States of America were not sufficient of a problem and a muddle.

Russia is the strongest bond of nationality, being the purest and clearest of the nations. Germany, France and England also tend to shake themselves free, and seek to find and to be themselves.

My quest at present is to unravel the Russian idea, and present Russia as she is in her spirit and her passion. By seeing Russia in this way we have a revelation of the majesty of a national idea. We obtain a notion how we should look if we could see ourselves as we really are.

Russia and England are akin, if it were only in the bond of Christianity. We have certain spiritual affinities. We could know ourselves much nearer to one another, though that depends on us rather than on Russia. She has much more to teach us than we have to teach her. It is only kindness to our politicians and progressive workers that could over suggest that they had anything to teach Russia—except the object lesson of the vanity of their tasks. Russia, alas! may learn wrong things of us and go wrong—Dostoieffsky's nightmare. The noisy middle-class Russia of to-day does indeed tend to follow after other gods. But for the moment I cannot pause to give actual pictures of Russia going wrong. I am in quest of the vital and fundamental idea of Russia, that which is the mother of her art, literature, music, of her religion and her traditional national life.

I am tempted to say that the Russian idea is an aspect of Christianity. Russia is the fairest child of the Early Church. Her national idea is identified with one of the Byzantine aspects of Christianity. But it would be impossible to deny that Russia draws her marvellous spirit from something earlier than Christianity. There is Nature-worship in the Russians; there is Scandinavian mythology; there is Oriental mysticism. The remote past still lends impulses of passions, dreams, fears, hopes, to the rustling and blossoming present. Yet all its past has been absorbed into Russian Christianity, though Russians have not yet explored and reproduced in art all the significances of that mysterious time in Russian history. We may say that the Russian idea is a Christian idea. Christianity has been great enough to include and say *yes* to all that was wonderful in the old.

What, then, of Russian ideas? Of the Russian idea?

When you first step into a Russian novel you come across symptomatic ideas, and when you go into Russia you find them again in the life of the people. Probably the most obviously characteristic thing is the love towards the suffering, pity. Russia is a remarkably tender and comforting nation. She is greatly concerned with her neighbour, and her heart is touched by his destiny. As Vladimir Rozanof writes:—

"Is there one page in the whole of Russian literature where a mock is made of a girl who has been betrayed, of a child, of a mother, of poverty? Even the thief is an honest thief. (Dostoieffsky's *honest thief.*) Russian literature is one continuous hymn to the injured and insulted. And as of such people there must always be a multitude in vain and gigantically-working Europe, it is possible to imagine the shout of joy which breaks forth when they are shown a country, a whole nation, where no one ever dare offend the orphan, the destitute, in the moral sense never dares to look insultingly upon the person left forlorn by circumstance, by destiny, by the break up of life. Of such people there are only too many. And what can the 'kings' of Victor Hugo say to them, or in general, the manifestly artificial subjects of Western writers? Russian stories can give consolation. For besides being taken from the habitual common every-day life, they have a tenderness. The Western man can say: 'There is a country where I should not have been despised; there is a country where I should not have been so coarsely insulted, where every man would have taken my part and interceded for me, where they would have taken me by the hand and raised me upon my feet again. I am cursed, but only in my own country, not on the whole planet.'

"That is the effect of Russian literature. Its significance is not a matter of the reviews of Western critics, not a matter of the noisy fame which has overtaken it; it lies not in its material triumph, but in a direct and absolutely unhampered affinity to the soul of the simple and universal reader. 'To some the Russian song is always pleasing.' . . . No—bigger, better. 'There are souls to whom the Russian song is the one thing necessary in life, to whom it is dearer than anything else in life—as to the hurt one, his mother,; as to the sick child, again his mother, perhaps neither a beautiful nor a virtuous one. 'Virtue'—it is, of course, somewhat strange to ask from Russians. . . . 'The Troika.' . . . But one thing there always is in Russia—sympathy, responsiveness. Perhaps it sprang up in Russia, and became exaggerated there just because so many people were crushed by various 'troikas.' However that may be, to be sung to sleep with Russia's cradle song many wish. . . ."

There is love towards the suffering one. It is part of a love towards the destiny of the individual. There is a remarkable absence of conventional standards. You are not looked at askance because you seem poor. The tramps and pilgrims on the road are never made ashamed of themselves. A contrast to America, where the tramp is an object of mirth, where he is regarded almost as an enemy of society. The Russian takes the tramp in. He has real hospitality, and not only the hospitality of hearth and home, the giving of food and a night's shelter, but also a more vital hospitality, that of mind and heart. He wants to know all about you. He asks you the human questions. He asks about father and mother and brothers and sisters, about your home and your calling and your goal. In return he tells you the intimate things of his life.

This is not only a matter of the road. How often the most utter stranger, met in a railway carriage or a post-station or at an inn, will after a remark about the weather or the crops begin to tell you the whole story of his life. He assumes the hospitality of your heart; a sure sign that in general people's hearts are hospitable, that in general there is a love towards destiny.

As a wanderer and a seeker I have myself experienced the ordinary material hospitality of hearth and home, and also this of the heart, having often been poor, strange-looking, and enigmatical enough. Russians have not looked askance; they have been brotherly. They have accepted a stranger naturally and simply as they would one near to themselves. More than that, knowing that I had a special quest, there have always been those who came forward and helped me in the spiritual things. Mysterious beings have, as it were, anticipated my coming, and have stepped out and recognized and said: "Read this; go to that one and talk to him; see this Russian picture." They love to preserve the mystery too. I have known people who had the aspect of having dreamed of my coming.

The first day I was in Vladikavkaz an old tatterdemalion standing by the bridge over the Terek came forward to embrace me and welcome me in the name of God. I had never met him before; I knew no one in the town. When I left Vladikavkaz last, to make my long and possibly dangerous Central Asian tramp, the most mysterious of my friends brought me a beautiful little copy of a painting of Martha and Mary to keep me from harm. And one night, months later, in a remote Moslem town on the fringe of the desert, I had a strange experience of adventure and terror when, as it seems to me, I was literally saved by looking at the picture. The giving of it was love towards destiny, hospitality of the heart.

It might be thought, however, that the Russian love stopped short with the honest, the religious, the seeking—that as long as a man could give a decent explanation of himself and his mode of life the Russian was on his side. But that would be to miss the real saliency of this love. The Russian loves the dishonest, the criminal, the despicable, the unpleasantly strange, the man who can give no explanation of himself, as much as she loves the other, even a little more than she loves the other; she has a "weakness" for the prodigal. Half her novels are expressive of love towards "criminals."

In English novels the plot is so adjusted that the author has scope to make a thorough out-and-out condemnation of the villain. He has a few pages where he lays himself out to show how inexcusable the villain's conduct was, what an abject scoundrel, what a disgraceful creature he is. The condition on which you may describe sin is that you condemn the sinner. In life also, as well as in literature, we are condemnatory; we love to pass judgment on others. How different in Russian literature! You find no condemnatory spirit there. The author's whole passion is to defend and explain the criminal, to evoke the tender sympathy of the reader. He makes you feel how strange, how pathetic, is man's destiny, how sordid his life compared with his spirit. Over the portal of Russian life and literature you might find the motto, "Neither do I condemn thee." Russia feels that however mean, however ugly and strange a man's life may seem, it is nevertheless a part of his great pilgrimage. He has got to go through it, he is learning something thereby, fulfilling something sacred thereby. This is exemplified very remarkably in Russia's legal system where, for instance, there is no capital punishment except under martial law. A man commits a murder, but he is not therefore condemned and hanged and turned over to God; he gets merely a dozen years in Siberia, and he goes on with his life.

Dostoieffsky, when he was in Siberia with forgers and murderers and highwaymen, was much concerned to seek out the gold in their character; and he remarks how a violent and dangerous man will even shed tears at the sight of a child suffering. "Murderers are much more simple than we take them to be," says he in another place, "so are we all."

The Russians are unashamed. Men and women confess voluntarily to having committed crimes or behaved abominably upon occasion. The man who lives an immoral life does not do so secretly to his wife. The black sheep of the family is not hidden in the background, "never mentioned," or subscribed for and sent to a distant colony; he is sitting at the table and is quite cheerful, and everyone takes him for granted. No one is ashamed to borrow or to be tremendously in debt; no one horror-struck at the idea of visiting the pawn-shop. All which exemplifies the love towards individuals and individual destiny.

This is why Russia is so free. It is almost a platitude to say that conventions determine the extent of personal freedom much more than the laws of the realm or the behaviour of the police. Yet it is a fact lost sight of when people talk of tyrannous government. In Russia love is towards the individual much more than towards the State. There is indeed no particular love towards the State. We uphold the State; to us the police and the police-system are almost sacred. We often condemn individual behaviour in the name of the State. We abhor "shirkers," "rebels," "breakers of the peace." Hence our comparatively limited British freedom. We believe in order. Our freedom is freedom within bounds. We allow ourselves to be disciplined along definite lines. In Russia it is different. There freedom often amounts to chaos. Even Russian order, *poryadok*, that which comes from Petrograd, is something borrowed from Germany to keep the nation together. Russians have no instinct for order. Watch our best troops marching—they give you the idea that each soldier has been turned out from a factory, and is of one and the same type and size. They march like moving patterns. But the Russians march anyway; their order is of the lowest kind. It is even tolerated to have wives and mothers marching in the ranks with their husbands and sons, carrying their bundles. Some men are marching; others are running. Each man has his own individual expression in his countenance; he has not merely a regimental expression. Russia does not care for ranks, for blocks of houses, for formal gardens, for churches with pews. She likes the individual to do as he pleases. Hence a divine disorder, a glorious promiscuity. The church perhaps shows the quickest picture of national life—the kaleidoscopic mingling of people and colours, the wonderful crowd encompassed by the frescoed walls, the faces of the saints, the great cloud of witnesses.

The same picture, though modified by Western influence, is shown in the theatre. Russia wishes the disenchanting of the footlights, the participation of the public in the action of the drama, the removing of stalls and chairs—a divine disorder in the theatre. She believes in the emotional communion of the theatre—the actors inspired by the people, the people inspired again by the actors, the dance and interplay of human thoughts and emotions. Shut your eyes to the material world and you realize there are no footlights, no separating river of light between the two worlds of stage and auditorium. There is a great and wondrous ballet of thoughts and impulses, hopes and fears, going forward and across and backward and across again between the priests of the drama and the conspirators, the worshippers.

The church service and the drama, the church and the theatre have much in common. The Mass has much in common with the mystery play. And the mystery play was originally the Mystery—at which you did not look, but *into* which you were initiated. You participated in the action. You were the victim sacrificed, or the priest, or one of the conspirators in the orgy. You were made one in the sacrifice, as in the Mass you are made one in the sacraments of bread and wine, symbols of the victim. Share is taken in the sacrifice, we consent unto the death. We are made one. We get free from the idea of separation, from space and time, realizing the everywhere-here, the eternal present.

In such a form is the Russian notion of the world and his conception of life. It is such a church, such a theatre, such a mystery play. It has its liturgies of beauty, its many processions, its sacrifices, its ecstasies; it is a great phantasmagoria of emblem. Nothing is without significance; every man has his part; by his life he divines it and fulfils it. Every common sight and sound is charged with mystery. Exerything is praising, everything is choric, everything triumphant.

To recapitulate and restate this is aphorism: Russian life is remarkable by virtue of its love towards the suffering, towards the individual destiny; by the absence of condemnation; by faith in life even if life should express itself in meanness, sordidness, crime; a feeling for the pathos and wonder of life as exemplified in the individual; no love towards " the State " or man's order, but great love towards the individual and individual instinct; a consequent freedom, amounting at times to seeming chaos, a divine disorder such as the disorder of the starry sky, as opposed to man's order, say the order in which stars might be classified in a book; by disorder such as that of the flowers and shrubs of the forest, rather than order as in a formal garden; a belief then in instinctive genius and divination impulse of one's place in the kaleidoscope of existence.

With such natural disorder comes an incapacity for " discipline," " efficiency," " progress." Life is a mystery play.

Whence may be inferred the following differentiation of ideas:

Instead of the God of the ten commandments, and the consequent ten condemnations, the Russian acknowledges the God whose service is perfect freedom.

Instead of the simplification of life, love of its complexity. The Russian says "yes" to the multiplicity of doctrines; he does not wish personal destinies to be unravelled and straightened out by the State, standardized and guaranteed by the State. He will not reduce the chess of life to the draughts of life. A religious belief in pure democracy; no belief in Socialism.

Instead of a belief in the Future, belief in an eternal Present.

Instead of life understood as a march, life understood as a ballet.

Instead of life understood as Evolution, life understood as a marvellous phantasmagoria.

Instead of Time understood as a passage or corridor, Time as a labyrinth.

Instead of the world-ideal of garden cities and carefully planned parks and squares, a belief in the maze of the world.

Instead of a belief in the coming of universal peace, a belief in the recurrence of wars. No belief in the "making virtuous of the world and all people."

No belief in any explanation as sufficient.

No prejudice against impossibilities; a cheerful acceptance of miracles, infractions of the "laws of Nature," of the significance of dreams and visions, of the design of destiny hidden in apparent accident; a predisposition towards superstition.

A belief that in apparent failure lies a truer destiny than in apparent success.

A saying "yes" to the mysteries of the Birth, Crucifixion, and Resurrection of Christ. The West would take down Christ from the Cross, heal His wounds, and save Him. The East would not do that. She knows that she must crucify Him.

[S. G.]

WEEKLY EVENING MEETING,

Friday, April 23, 1915.

DONALD HOOD, C.V.O. M.D. F.R.C.P., Vice-President,
in the Chair.

MAJOR P. S. LELEAN, R.A.M.C.

Anti-Typhoid Inoculation.

THE subject provisionally selected for my discourse to-night was that of "Military Hygiene of the War," but the earliest attempt at detailed preparation indicated that so extensive a field could not be covered—much less satisfactorily dealt with—in the allotted time. It was, therefore, thought that greater deference to the audience would be displayed by an attempt to deal more thoroughly with a single section of this vast field, and, with the approval of the authorities, anti-typhoid inoculation was chosen not only on account of its scientific interest, but as a matter of considerable practical importance at the moment.

This method of prophylaxis is so much more applicable to the protection of aggregations of men than of individuals, that it has hitherto remained a military rather than a civil method, and hence has not come under the notice of the civilian practitioner to an extent which has led to his mastering its details as fully as the medical officer has perforce had to do. Still less has it been considered by the non-medical part of the community at large. It is probably owing to the latter fact that anti-typhoid inoculation has, to our regret, not yet been made compulsory in the British Army.

So long, however, as medical officers were not interfered with in their efforts to persuade their men to voluntarily present themselves for inoculation, the principal result of the voluntary system was that an additional task of persuasion was thrown upon the hard-worked medical service. Matters assumed a different aspect when the formation of a national army led to the obtrusion of the anti-everything busybodies, with their wonted capacity for misrepresentation and a mischievous energy supported by funds which might have been devoted to a good cause. Indifferent to all considerations but that of proclaiming their faddist doctrines, these obstructionists thrust their organized influence between medical officers and men, and

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N.B.—The illustrations are by permission of Messrs. J. & A. Churchill, the publishers of "Sanitation in War," by Major Lelean.

jeopardized the lives of the latter by denouncing a method which offends their prejudices.

To the credit of the men it may be said that the great majority of them were sufficiently intelligent to be uninfluenced by these attempts, but the small minority who were influenced exposed themselves and the forces to which they belonged to so great dangers of acquiring enteric fever, that it was no longer possible to ignore the efforts of the busybodies. It became necessary that at least the civilian practitioners who volunteered their services should be fully informed as to the rationale of this method, and be fully armed with precise data supporting its claims to success in protecting the forces from the most dangerous scourge of armies in the field.

It is further advisable that a frank statement of the true merits of the case in favour of universal application in the army of this means of protection should be given a publicity which will enlist the support of a weight of public opinion fatal to the hopes of the faddists.

The problems relating to anti-typhoid inoculation can only be approached by an initial consideration of certain facts established by research into the phenomena of immunity—a research which has proved one of the most brilliantly successful laboratory studies. Unlike so many barren triumphs of the laboratory, this research has led to important practical results, for from it has been evolved the most effective contribution of preventive medicine to our means of safeguarding the lives of *our* men in camp and field.

I propose, therefore, to consider to-night:—

1. The general principles of active immunization.
2. The application of those principles to the problem of protection against enteric (typhoid) fever.
3. The scientific and practical results obtained by prophylactic inoculation.

A.—THE GENERAL PRINCIPLES OF ACTIVE IMMUNIZATION.

Beginning, then, with the general principles of immunization, one defines immunity as relative insusceptibility to disease, and in doing so stress is laid on the qualifying adjective "relative," for immunity is seldom, if ever, absolute.

Immunity, as thus defined, varies according to—

(*a*) The number of invading organisms. It is a matter of common knowledge that, thanks to dilution of the dose of infective material by good ventilation, relatively few among the many hundreds of thousands of persons who inhale tubercular bacilli acquire tubercular disease.

(*b*) The route of invasion. It is interesting to note that, while cholera vibriones are intensely virulent in the intestine and innocuous in the blood, tetanus bacilli conversely are harmless in the intestine, but most dangerous in the tissues.

(*c*) The virulence of the invading organism. The importance of this factor is illustrated by hydrophobia, from the developed disease of which no case of recovery has ever been recorded.

(*d*) Physiological resistance of the invaded person—a subject to which we shall revert in a moment.

Varieties of Immunity.

There are two main varieties of immunity—the natural and the acquired.

(*a*) *Natural immunity*, again, may be racial or physiological.

1. The racial is illustrated by the Barbary sheep in its insusceptibility to anthrax, which is so fatal to other species of sheep. Another striking difference is that between such closely allied species as the field mouse and the house mouse: one succumbs rapidly to septicæmia after a coccal injection to which the other is relatively indifferent.

2. Physiological resistance plays a most important part in the difference in susceptibility of individuals of the same species. This difference is most strikingly demonstrated by an instance reported from India, where cholera dejecta carelessly thrown from a tent contaminated a camp water-supply. Of the twenty-three persons who drank from this supply on the following morning, only eight developed cholera.

Further illustration is afforded by the familiar occurrence of boils in the debilitated and the diabetic.

(*b*) *Acquired immunity* may be obtained naturally or artificially.

1. If naturally acquired, it follows, and results from, an attack of acute specific disease. In some instances—as, e.g., influenza—the immunity is so short that an attack has even been supposed to leave increased susceptibility to fresh invasions by the same organism. It is generally recognized, however, that in the great majority of acute infective diseases one attack confers a high degree of protection against a second attack of that disease. In very rare cases a second attack has been known to occur even in diseases conferring so marked a subsequent immunity as scarlet fever or enteric. As such exceptions but prove the rule and afford no proof that immunity is not thus conferred in general, so the rare instances of enteric occurring in inoculated men cannot be regarded as arguments proving that inoculation is valueless. It would be unreasonable to demand more protection from artificial methods than is given by the natural processes which they aim at copying.

2. Artificially conferred immunity may be of the passive or the active variety.

The former is a curative and temporary result of the injection of protective substances ready-made. Its value is indicated by the fact that the use of anti-diphtheritic serum has reduced the mortality of diphtheria from 50 per cent. to 5 per cent.—which, in the United States alone, means an annual saving of some 30,000 lives. Unfortunately, for various technical reasons, this method is of only very limited applicability.

Active immunization, on the other hand, is effected by making the body manufacture its own protective substances, which are semi-permanent and prophylactic. The means adopted is that of injecting the stimulating material which produces this effect in the process of natural immunization occurring during an attack of the disease itself.

What measure of success has attended our efforts will be indicated later, but first it is necessary to study in detail the exact process of immunization as effected by natural means in the course of recovery from, and subsequent protection against, bacterial infections.

The Mechanism of Naturally Acquired Immunity.

A sound grasp of this complex subject can only be obtained by a review of the properties and inter-relationships of the various bodies which take part in the production of immunity.

In endeavouring to present this difficult subject in a manner which is both simple and comprehensive, one is faced by three main difficulties. The first is that of the starting-point—and, as lack of interest in bacteriology may have led to certain of my audience not having considered these matters previously, it appears desirable to err on the safe side and deal with relevant factors, however simple.

Next, it is difficult to illustrate all points by the details of any single investigation or process. It is advisable, therefore, to present the facts as far as possible dovetailed into one mental picture, without the distraction of breaking off constantly to explain that one or other fragment of the mosaic is derived from data relating to some special infective process.

And, lastly, it is impossible to avoid the use of technical terms, because any research which is so advanced as this must of necessity possess its own nomenclature. Use of technical terms is therefore inevitable, and not due to emulation of the example of the gentleman who would only refer to a bell as "a scintillating tintinabulator." One rather sympathizes with the coster who, when reproached concerning his language, remarked pathetically, "There's no other words for it."

In introducing the *dramatis personæ* it is advisable to take first those which are normally present in the body, and, secondly, those which only appear upon the stage after a bacterial invasion has occurred.

a. Protective Agents Normally Present.

1. The *fixed tissue-cells* of the lymphatic glands and bone-marrow (indicated by the number 5 in diagram *a*).

These, when stimulated by the poisons thrown into the system by the invading bacteria, manufacture the protective substances which play a leading part in the processes of immunization.

They may be regarded therefore as the arsenals of the invaded state.

2. The *phagocytes*, or wandering blood-cells, or—to give them their full title—the polymorphonuclear leucocytes (1 in diagram *a*).

These cells have the duty of devouring dead or damaged bacteria; they thus serve the function of removing the bodies from the battle-field, and may be considered as stretcher-bearers, or—in the friendly parlance of the army—the "body-snatchers."

It is noted that the substance which actually kills the invading bacteria is contained in the blood-serum, and that its lethal power, as compared with that in normal serum, is known as the "opsonic index." The opsonic index is determined by observing the average number of organisms ingested by phagocytes which have lain for a period in a bacterial emulsion to which blood-serum has been added.

To the phenomenon of phagocytosis is closely related that of chemiotaxis. Phagocytes are repelled by living organisms, but attracted by dead bacteria, these phenomena being known respectively as negative and positive chemiotaxis.

3. The *complement* is a substance which is only capable of attacking the invaders if it be provided with the appropriate weapon manufactured by the fixed tissue-cells (4 in diagram *a*).

It therefore plays the part of an unarmed defender.

It is normally present in blood and is destroyed by heat.

b. Protective Substances which only appear after Invasion.

1. *Antigen*, as its name indicates, is the initiator of the production of anti-bodies (number 2 in diagram *a*).

Any extraneous animal substance introduced into the body possesses this power. Unorganized proteins are concerned with the fascinating reactions of anaphylaxis; organized bacteria concern us to-night as the antigen which, when injected into the human body, leads to active immunization by stimulating the tissue-cells to manufacture protective anti-bodies.

The bacteria therefore represent the invading hosts.

It is as well at this point to answer the frequently asked question—how can the various substances concerned in immunization be accurately compared and have their power measured when they cannot be isolated?

The lethal power of bacterial emulsions, or of toxins, is determined by finding the minimal measured amount which, when injected, will certainly kill an animal of a given weight in a given time. This minimal lethal dose is known as the "M.L.D." Similarly, protective fluids are standardized by ascertaining the minimal amount which, when injected, will protect the animal against 100 "M.L.D.'s." This amount is known as the "Immunity Unit."

The affinities of these various substances for each other were brilliantly explained and visualized by Ehrlich by means of his side-chain theory, which invokes the aid of imaginary "receptors" by which the various bodies are attached to each other. This conception will be illustrated presently by means of a working model, the movements of which will be projected upon the screen.

2. The *toxins*, or poisons (3 in diagram *b*), are formed by the antigen and thrown off into the blood stream. They are ultra-microscopic, but so active that one fifteen-millionth part of a grain is estimated to be sufficient to kill a rabbit.

Being ultra-microscopic, they can in some cases be obtained in the filtrate from which all bacteria have been removed. In other cases the toxins, as those of typhoid bacilli for example, cannot be obtained apart from the bodies of the bacilli.

It is presumed that their action explains bcth chemiotaxis and the production of anti-bodies. If highly concentrated they drive away the phagocytes and kill the tissue-cells; if dilute they attract the phagocytes and stimulate the tissue-cells to manufacture protective substances.

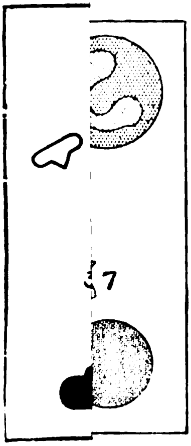
Their action is thus comparable to that of the fire of the host of invaders, which drives the defenders out of their immediate area, but stimulates defensive preparations throughout the rest of the invaded state.

3. *Anti-toxins* attach themselves to, and destroy, the toxins. They are formed as a result of injection of living bacteria, dead bacteria, or their toxins (6 in diagram *c*). It is they which confer the passive immunity which follows injection of blood serum in which they are formed as a result of inoculation of dead organisms into living animals. By means of successive injections of a horse, e.g., the resultant diphtheritic anti-toxins can be obtained so highly concentrated that one dose of its serum contains sufficient anti-toxin to neutralize 400,000 M.L.D.'s of toxin.

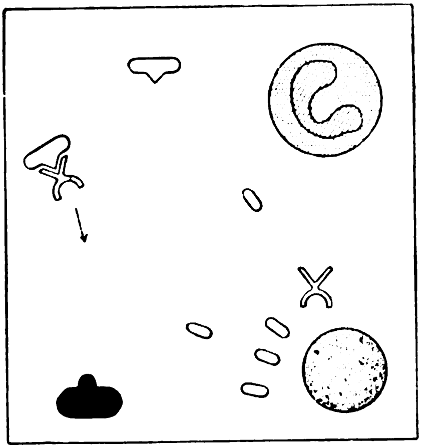
The primary duty of the anti-toxins is to protect the tissue-cells from toxins while the cells manufacture other anti-bodies which will kill the invading bacteria, which the anti-toxins themselves cannot attack. Although produced in excess, anti-toxins are rapidly excreted, and it is to this fact that the evanescence of passive immunity is due.

Anti-toxins are thus comparable to the screen of artillery fire which the defenders use to protect their arsenals, while the populace

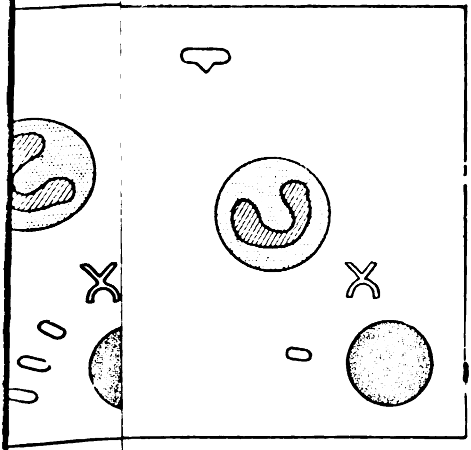
DIAGRAM PROCESS.



1*b*.—nti-toxins; by, T7).



1*c*.—Excess Anti-toxins after neutralization of Toxins; Amboceptor attached to complement.



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ndition of subsequent immunity—available Anti-toxin and Amboceptor remaining.

is being provided with arms with which the invaders may be exterminated.

4. Lastly comes the essential *amboceptor*—the immune body, antibody, sensabilitrice, or fixateur—which represents the bayonet supplied on mobilization to the unarmed defender, or complement. (Number 7 in diagram *d.*)

Amboceptor is not normally present in the body, and a special variety must be formed to deal with each different variety of invading organism. It is a remarkable fact that each is strictly "specific," i.e. will deal only with the particular variety of organism which has led to its production.

The best illustration of the degree of this specificity is afforded by reference to anaphylaxis. It has been found that if an animal be sensitized by injection of an antigen provided by human blood, that animal will react with a further dose of human blood, but will give no reaction with blood derived from any other animal. It has thus become possible to determine the identity of a blood-stain—a matter of the utmost practical importance in criminal investigations.

Being unaffected by heat, amboceptor can be obtained separate from complement, which heat destroys.

Having been formed, and always formed in excess in cases which recover, amboceptor resists excretion, and therefore remains in the blood for years—or for life—ready mobilized to repel a fresh invasion. Its precise rôle is to attach the complement to the organism, and thus enable the former to destroy the latter by direct lethal action.

The term immune-body includes a group of substances, comprising among others agglutinins, bacteriocidins, bacteriolysins and opsonins. The means by which each of these bodies kills bacteria by acting on some limb of their vital ring is best illustrated diagrammatically, and the method by which one of them is estimated will be described later, as an illustration of delicate accuracy of laboratory technique.

Popular Simile for the Soldier.

After a very considerable experience of the duty of persuading men to volunteer for inoculation, it has been found that the surest road to success is to explain as far as possible how the method works, and what it is supposed to do. If they can get hold of a general idea that is all that is necessary, as long as there are one or two points of which they can see the proof for themselves.

The proofs are simply provided—e.g. one of the most effective is to show a hanging-drop of motile enteric organisms and beside it a hanging-drop agglutinated by immune serum. The former alone is one of the most effective methods of obtaining volunteers. A man comes up with a cheery smile to peer down the microscope; as he catches sight of the seething mass of motile organisms he blenches; and one has the satisfaction of seeing him pass straight on into the inoculation room.

The explanation of the process is less simple, and matters must be put in a way which appeals to his imagination. There is no better way than by adopting the simile of the battlefield, somewhat on these lines: The organisms are the enemy invaders; the body the invaded country; the toxins the enemy's projectiles; the complement the unarmed population; the tissue-cells the arsenals: the anti-toxins the defenders' projectiles; the amboceptors the bayonets of the defenders; the phagocytes the "body-snatchers."

With these pieces one fights a war-game for the men, illustrating it by the diagrams as in Fig. 1, or by the working model now to be demonstrated.

Successful invasion. Unarmed defenders driven back by gun-fire (negative chemiotaxis and toxins). Arsenals put out of action (toxins kill tissue-cells). Capture of lines of communications (blood vessels). Capital reached (heart). Surrender of defenders (death).

Rally. The invaders' fire checked (anti-toxins). Defenders armed (amboceptors). Defenders converge on invaders (positive chemiotaxis).

Battle joined. Invaders' fire smothered (toxins neutralized). Defenders getting in with the bayonet (complement and amboceptor).

Victory. Invaders penned and annihilated (abscess cavity). Dead of both sides (organisms and complement) removed by body-snatchers (phagocytes). Invasion defeated.

Permanent mobilization. Defenders armed with bayonets which never become obsolete (the amboceptors—permanent). Shells for guns becoming obsolete (transient anti-toxins). Enough bayonets remain to enable defenders to crush any subsequent invasion before the enemy can develop gun-fire (permanent immunity by bacteriolysis before toxins can paralyse the defence).

B.—APPLICATION OF GENERAL PRINCIPLES OF IMMUNITY TO ANTI-ENTERIC PROPHYLAXIS.

Having considered the general principles of immunization, we have now to deal with their application to the problem of protecting our men against enteric fever.

Initial difficulties were experienced in many ways.

1. The view then held as to the immunity of animals to true enteric (a view since modified by experiments on anthropoid apes) led to its being thought necessary for all experimental work to be carried out on man. This factor also made it impossible for passive immunization to be undertaken as a curative measure in developed human enteric: as immunized animals produce no amboceptors, but

only anti-toxins, injection of their serum cannot kill the enteric bacilli, although some enteric toxins may be neutralized.

2. In the case of *B. typhosus* the toxins are endo-toxins—i.e. so intimately associated with the bodies of the organisms that they cannot by filtration be obtained free from the organisms. This made it necessary that the bodies of the organisms should themselves be injected as the stimulating antigen to promote formation of anti-bodies, and thus confer active immunity to enteric fever.

Auto-inoculation. This brings us to the point at which Sir Almroth Wright, in 1896, found progress barred along all other roads than that of auto-inoculation. It is with justifiable pride in our profession that we study the history of the advance along that perilous solitary road, and it is with a special pride in my corps that I recall the devotion to duty which led officers of the R.A.M.C. to offer themselves for this purpose. Without the possibility of a preliminary comparable test on animals, it became necessary for the officers experimented on to receive massive injections of dead enteric organisms, the effect of which could only be surmised by analogy.

As one can sympathize with the tension of this step in the darkness, so one can appreciate the satisfaction with which these officers found, in the subsequent development of protective substances in their blood, the promise of a brilliant outcome to their devotion and courage. It is interesting to note that they received in a single injection three times the number of enteric organisms which are now given as a dose.

The Methods now in Use.

1. *The Emulsion.* As the term "serum" is wholly inaccurate, and "vaccine" is often regarded as a term to be applied to a growth of living organisms, the fluid which is injected in this method is styled the anti-enteric (or anti-typhoid) emulsion.

The strain of organism used is one of very low virulence—as has been proved on two occasions when accidents with rubber connections have resulted in the laboratory personnel getting considerable intra-oral doses of the living culture.

The culture is a forty-eight hour broth-culture at blood-heat, and generally gives a concentration of about 3,000 million organisms per c.c.

The dose is 500 million dead organisms in the first and 1,000 million in the second injection. The reasons for this division of the dose are given later.

The standardization is obviously a matter of great importance in relation to the dosage. Various methods of determining the number of organisms per c.c. have been tried and successively abandoned for improved means.

(*a*) Weighing was regarded hopefully until it was found that

1,000 million enteric organisms weighed only 0·113 milligramme. A useful datum for men who are alarmed at the 1,000 germs is that they only weigh $\frac{1}{600}$ of a grain.

(*b*) Next, the technique adopted was to dilute the vaccine until 1 c.c. contained so few organisms that they might be counted as they grew into separate colonies on solid media, on which 1 c.c. of the diluted vaccine had been distributed. This means proved unsatisfactory owing to the tendency of the organisms to form clumps which however many organisms they contain, produce only a single colony per clump. In any event the process of diluting 100 million times does not lend itself to extreme accuracy.

(*c*) The next procedure was that of counting the numbers of red corpuscles and bacilli in a stained smear made from a mixture of equal parts of blood and bacterial emulsion. As the blood is known to contain, with remarkable constancy, five million corpuscles per c.mm., the number of bacilli per c.mm. can be readily calculated by their proportionate ratio to the corpuscles as counted under the microscope.

(*d*) The method now in use is the simpler and quicker one of diluting the emulsion and making a direct count of the organisms on a hæmacytometric cell under dark-ground illumination.

The laboratory precautions concern us closely because questions are often asked as to how the killing of all organisms in the emulsion is ensured.

The succession of precautionary measures speaks for itself, as to the security afforded by this chain of defences.

(*a*) Vegetative organisms are killed off by first heating the vaccine at 53° C. for seventy minutes and adding to the resulting emulsion, after it has cooled, 0·4 per cent of lysol. Subjection to a higher temperature would destroy the efficacy of the antigen.

(*b*) Samples of the emulsion are then plated to test for sterility under both aerobic and anaerobic conditions.

(*c*) Absence of spores is finally proved by inoculating animals with the emulsion and observing the effects for ten days subsequently. In order to afford an ample margin for considerations of massive inoculations, the animals get thirty times the proportional dose given to man.

2. *The injection.* There are certain important precautions to be observed when inoculating, but it is unnecessary to refer in detail now to the routine which is adopted with such care and success.

3. *Besredka's alternative method.* Before passing on, it is interesting to note that Besredka advises the use of a true vaccine—one containing living organisms.

His emulsion consists of living culture sensitized by contact with highly immune serum the complement of which has been destroyed by heat, thus leaving only amboceptor.

The rationale of the method is that when the sensitized living organisms are injected, the amount of amboceptor present with them enables the complement of the inoculated person's blood to destroy the bacilli with speed and certainty and without any danger of a general infection resulting (diagram *d*).

Injection of living organisms has not commended itself to bacteriologists in this country, and the method has not been adopted generally.

C.—THE RESULTS OF ANTI-ENTERIC INOCULATION.

We now turn to the results following this prophylactic measure, and they are best dealt with under the headings of (*a*) the early and (*b*) the remote effects.

(*a*) *The early Results.*

The most immediate, and in some ways the most practical, results are the *reactions* occurring in the inoculated. After considerable experience one can summarize the results as regards incapacitation as follows:

Incapacitated for five days, 1 per mille.
Incapacitated for three days, 3 per mille.
Incapacitated for less than three days, 996 per mille.

If due care be exercised, the danger of a reaction necessitating the man being admitted to hospital is almost negligible, and one force (I think it was the Canadian contingent) recently reported the inoculation of 27,000 men without a single hospital admission.

The constitutional reaction comes on usually about six honrs after inoculation, and consists of malaise, a slight temperature of perhaps 101°, with—in some cases—a tendency to faintness. If the man turns in early, he will probably sleep through the stage of constitutional reaction without being conscious of it.

The redness, tenderness and œdema around the inoculation site usually begin to subside after twenty-four hours.

It is safe to say that in 99 per cent of cases the man is capable of carrying out his duties at the end of forty-eight hours.

The *blood changes* follow later, and are of both scientific and practical interest, seeing that upon them depends the efficacy of the protection conferred by this method of prophylaxis.

The essential change is an increase in the amount of protective substances of the amboceptor group. It will serve if one substance of this complex group—the agglutinin—be used to illustrate both the degree of immunity conferred and the methods of estimation.

Agglutination is the phenomenon by which motile organisms in the presence of agglutinins first lose their motility and then collect together in groups, as if for mutual protection. Hanging-drops demonstrate both the initial motility and the complete agglutination which follows admixture with immune serum diluted one thousand times. The reaction is regarded as complete if at the end of half an hour no separate motile organisms can be seen. The concentration of agglutinins in a serum is measured by the extent to which the serum can be diluted without losing its power to "clump" a culture in half an hour. By means of capillary pipettes, as little as a ten-thousandth part of a drop of blood may be proved effective.

As normal serum has a slight agglutinative power, which seldom exceeds that represented by a dilution of 1 in 10, that amount is taken as the basis of comparison. We are now in a position to follow

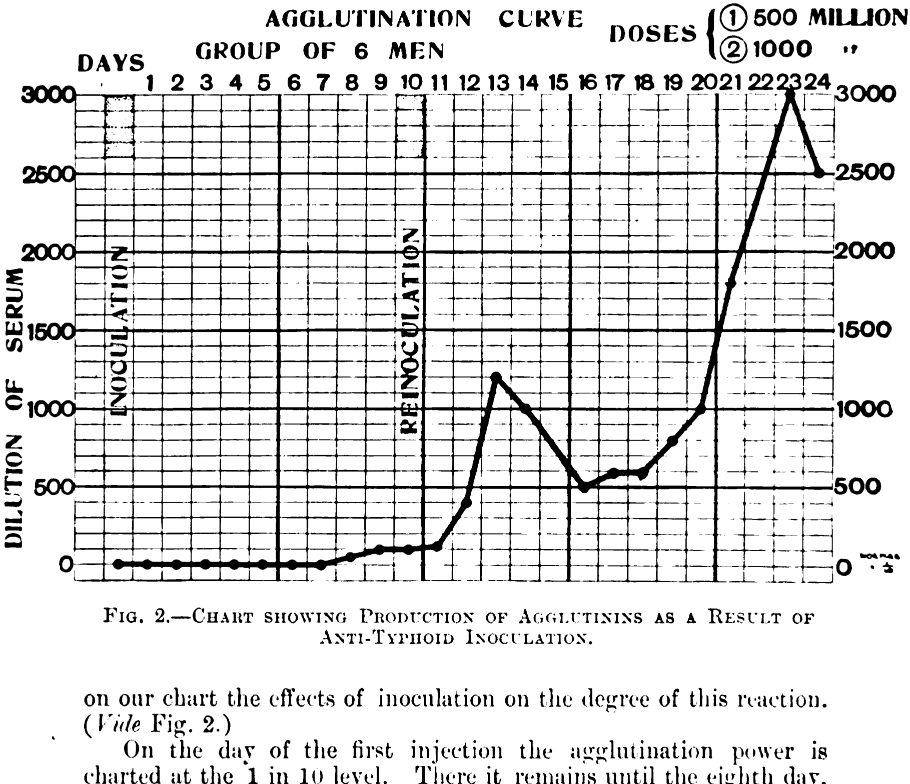

FIG. 2.—CHART SHOWING PRODUCTION OF AGGLUTININS AS A RESULT OF ANTI-TYPHOID INOCULATION.

on our chart the effects of inoculation on the degree of this reaction. (*Vide* Fig. 2.)

On the day of the first injection the agglutination power is charted at the 1 in 10 level. There it remains until the eighth day, when there appears a rise which progresses to its maximum of 120 times the normal on the thirteenth day, after which it begins to fall.

It was hoped at first that increase of the single injection dose would give a proportionately higher rise in the curve, but it was found that increased injections gave little better results, even if pushed to an amount which caused considerable constitutional disturbance, while massive doses of vaccine may produce an initial "negative phase." The effect was then tried of giving a second moderate injection ten days after the first, so that the second stimulus might be summated upon the first. The effect is shown by the wave sweeping up to 300 times the normal. A succession of doses at ten-day intervals would give still further protection, but a balance must be struck between the attainable protection and the inconvenience involved both to the individual and to the unit in the midst of military training. It is considered that the double dose strikes a happy mean.

The need for, and interval between, the two injections has been dealt with fully, because of the many questions which are constantly being asked about it, and because of the great utility of this chart when hung up in the re-inoculating room. There are very few men indeed who refuse their second dose when the chart has been shown them and fully explained.

After the third week there is a slight but rapid fall for a few days, and then the agglutinative power slowly recedes to the normal during a number of years. It is probable that normal is not reached for five or six years, but it is considered that protection should not be regarded as effective after the end of the second year, at which time it is advisable that men should be re-inoculated.

This has an important bearing on statistical data, as will be seen when we come to consider whether a man who has been inoculated more than two years previously should be included in the protected or the unprotected groups.

(*b*) *The remote Results.*

The remote effects of inoculation upon enteric incidence can only be estimated by statistical methods, with regard to which one factor calls for preliminary comment.

It is that—just referred to—of the classification of partially protected men, i.e. those who have had only one injection or who have been fully inoculated more than two years since. To include them among the fully protected would raise the enteric incidence in this group, while to place them among the wholly unprotected would reduce the enteric incidence in *that* group—either of which does injustice to the results of inoculation in the comparison of relative incidences. The only sound course is to place these men in a group by themselves.

With this initial remark one turns to the available statistics, an these have now become so numerous that only a few of the mor striking or interesting can be discussed. Three, of an increasing relative importance, are selected as indicating the results which have been obtained in our own army.

1. *The Meerut outbreak.* A cavalry regiment at Meerut (India) contained 200 inoculated and 300 uninoculated men. Sixty cases of enteric occurred in an outbreak among these men, all living under apparently precisely similar conditions in barracks. The cases were distributed among the groups as follows :

Fully protected	Partially protected	Wholly unprotected
Nil	2	58

Of the 58 cases among unprotected men, 10 died and 2 had to be invalided out of the Service as permanently incapacitated.

2. *Twenty-four test units.* Special medical officers were attached to twenty-four units for twenty months, with instructions to record the incidence of enteric in relation to prophylactic inoculation. The data thus obtained refer to a number of men equivalent to a whole division, living—as regards the groups in each regiments—under similar conditions for a considerable period. They therefore afford evidence on a scale which cannot be regarded as dominated by co-incidence.

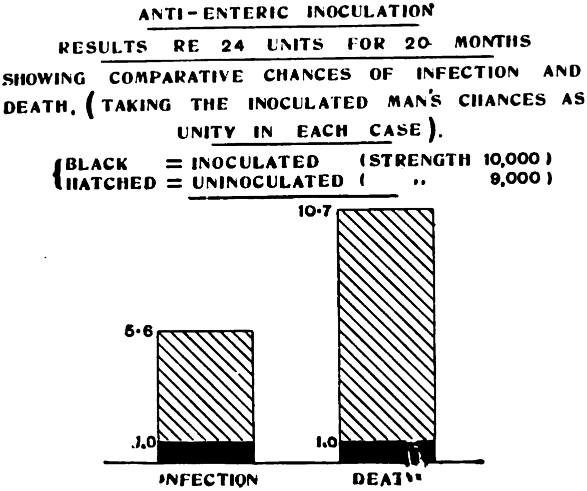

FIG. 3.

The relative incidences are shown in Fig. 3. If the wholly unprotected group data be compared with data from a group comprising all the other men—whether fully or partially protected by inoculation—the former give 5·6 times as many cases and 10·7 times as many deaths from enteric as the latter.

The most effective way of presenting these facts to the men is to state them in terms of sporting odds, thus: If two men of similar physique and age—one protected and one unprotected—are exposed to similar risks of enteric and one is going to get it, the odds are 5½ to 1 that it will be the uninoculated man, while if one of them is bound to die of it, the odds lengthen to 10½ to 1 that it will be the unprotected man.

That does not state the full value of the comparison, for if the partially protected be excluded altogether the results are even more striking.

3. *Data of the whole British Army in India during ten years.* The figures derived from the returns of enteric in the British troops in India from 1903 to 1913 cover a still wider field. The chart is shown in Fig. 4.

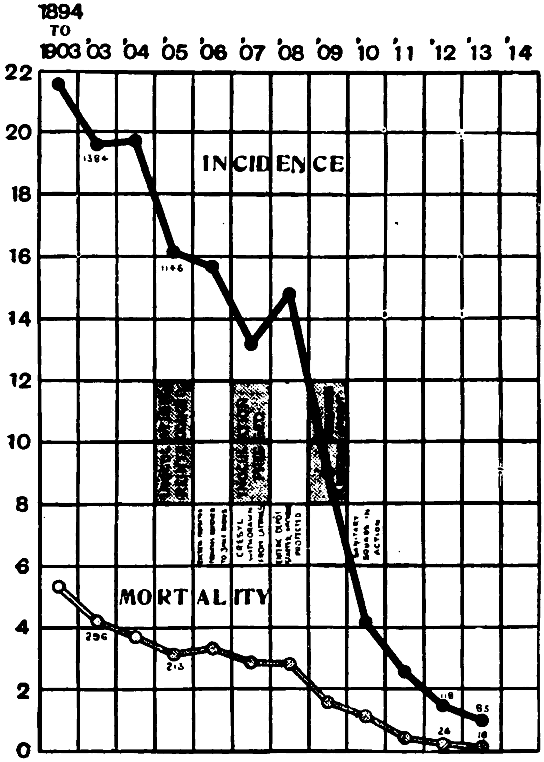

FIG. 4.—REDUCTION OF ENTERIC AMONG BRITISH TROOPS IN INDIA FROM 1903 TO 1913.

The following tabular comparison may be made between the enteric figures for the year in which inoculation was reintroduced, 1905, and the latest figures available, 1913, when 90 per cent. were inoculated.

	Admissions		Deaths
1905 . . .	1146	...	213
1913 . . .	85	...	16

The data for the Indian troops for these years may be contrasted.

	Admissions		Deaths
1905 . . .	130	...	35
1912 . . .	243	...	62

If it be urged that improving conservancy is alone responsible for the reduction in enteric among British troops, then the better sanitation of the cantonments of Indian troops should produce a corresponding decline in their incidence rates. Owing to native prejudices, prophylactic inoculation was not urged upon Indian regiments until 1911 and the numbers inoculated were inconsiderable.

What are the practical results of this reduction in enteric in the eight years since prophylactic inoculation was reintroduced? Two hundred lives saved annually; 250 less beds in hospitals; 103,000 less days of sickness in the year—or the equivalent of the whole British Army in India being out of action for a day and a half. In deaths and medical expenses alone this probably represents an annual economy of £50,000 a year, or a loan of over £1,500,000 at 3 per cent. interest.

I think that these results are sufficiently brilliant to justify the officers of the R.A.M.C. in looking back with satisfaction on the results of their collective efforts of the last ten years.

4. Striking as these figures are, we get figures more striking still if we turn to the *statistics of the army of the United States*, which has given us a lead in making anti-enteric inoculation compulsory, and has thus afforded an indication of the results which would follow adoption of a similar course for our army.

We may first take the contrast afforded by two divisions of similar strength (about 20,000 men) living in camp on United States territory in 1898—when no men were inoculated—and in 1911—when 100 per cent. of the strength was compulsorily inoculated.

The former division was camped at Chicamauga for ten months, and the latter on the Mexican frontier for four months.

	Enteric admissions		Enteric deaths
1898 (ten months) .	1,729	...	248
1911 (four months) .	1	...	nil

That the freedom from enteric in the latter instance was not due to absence of sources of infection is shown by the fact that among the uninoculated civilians in and around the camp there were 47 cases and 17 deaths.

The results of the introduction of voluntary inoculation in the United States army and then of making inoculation compulsory are shown in Fig. 5, which speaks for itself. The only note to be added, as strengthening the case, is that from 1908 to 1913 there was no death in this army from enteric fever, while in 1913 the army of 90,000 strong only gave three cases of enteric during the year.

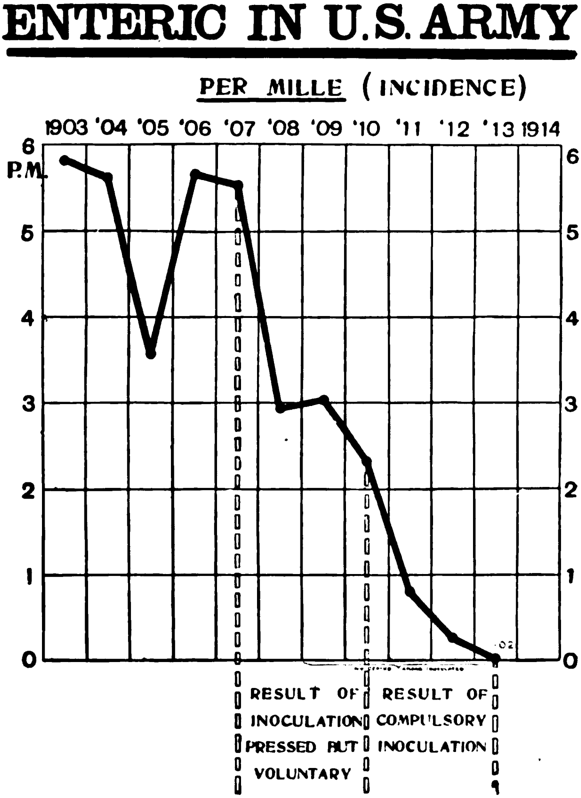

FIG. 5.—RESULTS OF ANTI-ENTERIC INOCULATION ON ENTERIC IN THE U.S. ARMY.

5. Finally we may just glance at the latest published figures bearing on the incidence of enteric upon our men at the front.

Out of 606 cases only 18 per cent. have been among inoculated men, while of the 50 recorded deaths only one has been an inoculated man.

In face of the overwhelmingly convincing mass of statistical evidence of the value of this means of protection, one can only look eagerly for the day when our men will be saved from their own vacillation, and rescued from the sinister influence of the anti-everything busybody, by having prophylactic anti-enteric inoculation made compulsory. Meanwhile the medical officer—already overtaxed by duties sufficiently strenuous—must undertake the thankless task of persuading men to voluntarily adopt a measure which not only protects themselves from a grave danger, but protects the whole army in the field from the risk thrust upon it by the obstinacy of the recalcitrant, who meets every argument and appeal with the parrot-cry of "I don't hold with it."

[P. S. L.]

ANNUAL MEETING,

Saturday, May 1, 1915.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1914, tesifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Forty-four new Members were elected in 1914.

Sixty-two Lectures and Nineteen Evening Discourses were delivered in 1914.

The Books and Pamphlets presented in 1914 amounted to about 338 volumes, making, with 652 volumes (including Periodicals bound) purchased by the Managers, a total of 990 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G, P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S.

Managers.

Sir Thomas Barlow, Bart., K.C.V.O. M.D. LLD. F.R.S.
Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. LL.D.
Andrew Carnegie, LL.D.
Charles Edward Groves, F.R.S.
Sir Robert Hadfield, J.P. D.Sc. F.R.S.
Charles Hawksley, M.Inst.C.E.
The Hon. Richard Clere Parsons, M.A. M.Inst.C.E.
Edward Pollock, F.R.C.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
The Right Hon. Lord Rothschild, Ph.D. F.R.S.
Alexander Siemens, M.Inst.C.E. M.I.E.E.
Alan A. Campbell Swinton, F.R.S. M.Inst.C.E.
Harold Swithinbank, J.P. F.R.S.E.
The Right Hon. Lord Wrenbury, P.C. M.A.

Visitors.

Rev. Edward S. Dewick, M.A. F.S.A.
Lieutenant-Colonel Henry E. Gaulter, F.R.G.S.
Joseph Gordon Gordon, F.C.S. F.I.C.
William J. Gow, M.D. F.R.C.P.
James Dundas Grant, M.D. M.A. F.R.C.S.
John W. Jarvis, F.G.S.
James Y. Johnson, M.R.I.
H. R. Kempe. M.Inst.C.E.
Francis Legge, F.S.A,
Francis K. McClean, F.R.A.S.
Ernest R. Moon, K.C., LL.B.
Richard Pearce, F.G.S.
Henry G. Plimmer, F.R.S. M.R.C.S.
Arthur J. Walter, K.C. LL.B.
Sir Henry J. Wood, M.R.I.

GENERAL MONTHLY MEETING,

Monday, May 3, 1915.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

The Honorary Secretary announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

Sir Thomas Barlow, Bart., K.C.V.O. M.D. LL.D. F.R.S.
J. H. Balfour Browne, K.C. LL.D.
Charles Hawksley, M.Inst.C.E.
Edward Pollock, F.R.C.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
The Right Hon. Lord Wrenbury, P.C. M.A.
Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S. (Treasurer).
Col. Edmond H. Hills, C.M.G. R.E. D.Sc. F.R.S. (Secretary).

Richard A. Beamish, D.L.
William E. Benton,
Charles A. Schunck,

were elected Members of the Royal Institution.

The Secretary read the following Resolution passed by the Managers at their Meeting held this day :—

The Managers of the Royal Institution of Great Britain desire to place on permanent record their grateful sense of the value of the services rendered to the Institution by Mr. Alexander Siemens as Secretary. During his period of Office, Mr. Alexander Siemens gave unsparingly of his time and attention to the duties, and largely contributed thereby to the success which has attended the Administration of the Institution.

Mr. Alexander Siemens is a Past President of the Institution of Civil Engineers, and of the Institution of Electrical Engineers. He has been a Member of the Royal Institution for thirty-three years, and during that time delivered the following Friday Evening Discourses :—"Theory and Practice in Electrical Science," in 1893; "Cable-laying on the Amazon River," in 1896; "New Developments in Electric Railways," in 1904; "Tantalum and its Industrial Applications," in 1909; upon all of which he spent much time and attention in their elaborate illustration by Experiments. He thus followed in the footseps of his relation, the late Sir William Siemens, whose Friday Evening Discourses at the Royal Institution are landmarks in the past history of Science.

The Managers rejoice to know that Mr. Alexander Siemens will as a Manager continue to give them and the Royal Institution the benefit of his assistance and advice.

The Special Thanks of the Members were returned to Thomas W. Dewar, M.D. F.R.C.P.E. F.R.S.E. M.R.I., for his Donation of £15 15s. to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Right Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. ScD. F.R.S. M.R.I., was re-elected Honorary Professor of Natural Philosophy, and Sir Joseph John Thomson, O.M. M.A. LL.D. D.Sc. F.R.S. M.R.I. was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Geological Survey Records, Vol. XLV. Part 1. 8vo. 1915.

Parliamentary Recruiting Committee—German Atrocities on Record. 4to. 1915.

Pamphlets and Leaflets to Encourage Recruiting. 8vo. 1915.

Accountants, Association of—Journal, Vol. VIII. No. 41. 8vo. 1915.

American Geographical Society—Bulletin, Vol. XLVII. No. 4. 8vo. 1915.

Asiatic Society, Royal—Journal, April, 1915. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXV. No. 5. 8vo. 1915.

Atkinson, Miss A. R., M.R.I.—History of the Scottish Highlands, Highland Clans, and Regiments. Edited by John S. Keltie. 5 vols. 4to. (New Edition.) n.d.

The Ruined Abbeys of Britain. By Frederick Ross. 2 vols. 4to. n.d.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXII. No. 12. 4to. 1915.

Carnegie Institution—Contributions from the Mount Wilson Solar Observatory. Nos. 93-95. 8vo. 1915.

List of Publications of the Carnegie Institution, 1902-15. 8vo. 1915.

Chemical Industry, Society of—Journal, Vol. XXXIV. Nos. 5-8. 8vo. 1915.

Chemical Society—Journal for April, 1915. 8vo.
Chemistry, Institute of—Proceedings, Vol. 1915, Part 2. 8vo.
East India Association—Journal, N.S. Vol. VI. No. 2. 8vo. 1915.
Editors—Agricultural Economist for April, 1915. 8vo.
Amateur Photographer for April, 1915.
American Journal of Science for April, 1915. 8vo.
Athenæum for April, 1915. 4to.
Author for April, 1915. 8vo.
Chemical News for April, 1915. 4to.
Chemist and Druggist for April, 1915. 8vo.
Church Gazette for April, 1915. 8vo.
Concrete for April, 1915. 8vo.
Dyer and Calico Printer for April, 1915. 4to.
Electrical Engineering for April, 1915. 4to.
Electrical Industries for April, 1915. 4to.
Electrical Review for April, 1915. 4to.
Electrical Times for April, 1915. 4to.
Electricity for April, 1915. 8vo.
Engineer for April, 1915. fol.
Engineering for April, 1915. fol.
Ferro-Concrete for April, 1915. 8vo.
Horological Journal for April, 1915. 8vo.
Illuminating Engineer for April, 1915. 8vo.
Journal of the British Dental Association for April, 1915. 8vo.
Journal of Physical Chemistry for April, 1915. 8vo.
Junior Mechanics for April, 1915. 8vo.
Law Journal for April, 1915. 8vo.
London University Gazette for April, 1915. 4to.
Marine Engineer for April, 1915. 8vo.
Model Engineer for April, 1915. 8vo.
Musical Times for April, 1915. 8vo.
Nature for April, 1915. 4to.
New Church Magazine for April, 1915. 8vo.
Nuovo Cimento for April, 1915. 8vo.
Page's Weekly for April, 1915. 8vo.
Periodical for April, 1915. 8vo.
Physical Review for April, 1915. 8vo.
Power for April, 1915. 8vo.
Power User for April, 1915. 8vo.
Science Abstracts for April, 1915. 8vo.
School World for May, 1915. 8vo.
War and Peace for April, 1915. 8vo.
Wireless World for April, 1915. 8vo.
Zoophilist for April, 1915. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIII. No. 246. 8vo. 1915.
Essex Archæological Society—Transactions, Vol. XIV. Part 1. 8vo. 1915.
Congress of Archæological Societies: Report of Committee on Ancient Earthworks and Fortified Enclosures. 8vo. 1914.
Florence, Biblioteca Nazionale—Bulletin for April, 1915. 8vo.
Franklin Institute—Journal, Vol. CLXXIX. No. 4. 8vo. 1915.
Geneva, Société de Physique—Comte Rendu, Part 31. 8vo. 1914.
Geological Society—Abstracts of Proceedings, No. 974. 8vo. 1915.
Journal, Vol. LXX. Part 4. 8vo. 1915.
Johns Hopkins University—American Journal of Philology, Vol. XXXVI. No. 1. 8vo. 1915.
London County Council—Gazette for April, 1915. 4to.
Meteorological Office—Monthly Weather Reports for March, 1915. 4to.
Weekly Weather Reports for April, 1915. 4to.
Summary of Monthly Weather Reports for Year 1914. 8vo. 1915.

Navy League—The Navy for April, 1915. 8vo.
New York, Society for Experimental Biology and Medicine—Proceedings, Vol. XII. No. 5. 8vo. 1915.
New Zealand, Registrar-General—Official Year Book for 1914. 8vo. 1914.
Pharmaceutical Society of Great Britain—Journal for April, 1915.
Calendar, 1915. 8vo.
Photographic Society, Royal—Journal, Vol. LV. No. 4. 8vo. 1915.
Physical Society of London—Proceedings, Vol. XXVII. Part 3. 8vo. 1915.
Română Academia—Bulletin de la Section Scientifique, II-ème Année 1913-14, Nos. 4-10; III-ème Année 1914-15, Nos. 1-8. 8vo. 1913-15.
Röntgen Society—Journal, Vol. XI. No. 43, April 1915. 8vo.
Royal Horticultural Society—Journal, Vol. XL. Part 3. 8vo. 1915.
Royal Society of Arts—Journal for April 1915. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXXV. Part 1. 8vo. 1915.
Royal Society of London—Proceedings, A, Vol. CXI. No. 628; B, Vol. LXXXVIII. No. 606. 8vo. 1915.
Sarawak Museum—Thirteenth Annual Report, for 1914. 8vo. 1915.
Tohoku Imperial University, Sendai, Japan—Mathematical Journal, Vol. VI. No. 4. 8vo. 1915.
United States Department of Agriculture—Journal of Agricultural Research for March, 1915. 8vo.
Experiment Station Record, Vol. XXXII. No. 4. 8vo. 1915.
United States Patent Office—Official Gazette, Vol. CCXII. No. 5: Vol. CCXIII. No. 1. 8vo. 1915.
Western Australia, Agent-General—Monthly Statistical Abstract for Dec. 1914. 4to.
Zoological Society—Annual Report for year 1914. 4to. 1915.

WEEKLY EVENING MEETING,

Friday, May 7, 1915.

COLONEL E. H. HILLS, C.M.G. R.E. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

PROFESSOR O. W. RICHARDSON, M.A. D.Sc. F.R.S.

Electrons and Heat.

WHEN electrified bodies are heated they are found to lose the power of retaining an electric charge. The charge leaks away from their surfaces. This is not a novel phenomenon. It has been known for nearly two centuries that solids glowing in air are capable of discharging an electroscope. Thus you observe that the electroscope is at once discharged when I bring near it a red-hot poker withdrawn from the furnace on the lecture-table. These effects are due to the emission of ions by the hot solids. For example, if the electroscope is negatively charged, it draws positive ions from the hot poker, and so becomes discharged.

Most bodies when heated in air at low temperatures emit only positive ions. At sufficiently high temperatures ions of both signs are emitted simultaneously. We can show this by a simple experiment in which the hot body consists of a loop of platinum wire and acts as its own electroscope. When a charged rod is brought near the loop a charge of opposite sign is induced on the latter, which is thus deflected owing to the electrostatic attraction of the rod. When the loop is cold this happens whatever the sign of the charge on the rod. If the wire is at a dull red heat it can only be deflected by a positively charged rod. When a negatively charged rod is brought near it the emission of positive ions causes the induced positive charge at once to stream away. Thus the wire is incapable of retaining a positive charge, and so no deflection is produced by a negatively charged rod. At very high temperatures you observe that the loop is undeflected whatever the sign of the charge on the rod. The wire is now liberating both positive and negative ions, and so is unable to retain either a positive or a negative charge.

If these effects are investigated in a vacuum, instead of in air at atmospheric pressure, it is found that the emission of positive ions gradually disappears with continued heating, so that a wire which has been well glowed out in a vacuum emits only negative ions in appreciable quantity. Thus, if we repeat the last experiment with an incandescent lamp, using one in which the filaments are not anchored, we see that the loops are attracted by a negatively charged rod, but not by one which is charged positively. They show, in fact, a behaviour which is precisely opposite to that of a wire at a dull red heat in air.

Now let us consider the nature of the ions which carry these thermionic currents, to use a term which I have ventured to apply to the currents which leak away from the surfaces of hot bodies in this manner. As is well known, the negative electrons which play such an important part in physical phenomena are very readily deflected by moderate magnetic fields, whereas ions of atomic or greater magnitude are not. I have here an arrangement which will enable us to apply this test to the ions emitted by hot bodies. An exhausted tube carrying a horizontal hot wire is placed in a vertical electric field. The electric field is arranged so as to drag the negative ions emitted by the wire to a suitable electrode, whence they flow through a galvanometer whose deflection is registered by the spot on the screen. Around the tube an electro-magnet is arranged, so that, when it is excited, there is a horizontal magnetic field which tends to curl up the paths of the ions. If I now switch on the electro-magnet, you observe that the current is at once reduced to a small value, showing that the magnetic field curls up the paths of the ions, so that they are now unable to reach the electrode. The carriers of this negative discharge are, in fact, electrons.

I have here a second tube, arranged to give a conveniently large positive discharge. When this is tested by the electro-magnet in a similar way, the magnetic field is found to have no influence on the thermionic current. The positive ions are, in fact, much more massive than the electrons; more elaborate experiments have shown that they are charged atoms.

We see from these experiments that the negative emission is characterized by the electronic nature of the carriers and by its permanence in a vacuum. The presence of a gaseous atmosphere is not necessary in order to maintain these currents. Thus the electrons must come from the heated body itself. I believe that this emission is a process which is closely analogous to evaporation. The essence of evaporation, of a liquid for example, lies in this: that, as the temperature is raised, the molecules acquire sufficient energy to overcome the forces which attract them to the liquid, and so become free molecules of the vapour. We know that all material substances contain electrons, and it is not unreasonable to expect them to behave, when the temperature is high enough, in a way analogous to the molecules of a liquid. Another analogy, in some ways more accurate, would liken the emission of electrons to the reversible evolution of a gas by the decomposition of a solid such as calcium carbonate. The similarity of this process to evaporation is well known to chemists.

This position is strengthened when we examine the way in which the electron emission depends on the temperature of the hot body. This may readily be done by surrounding the hot wire with a cylindrical electrode to catch the electrons, which then flow through a galvanometer whose deflection measures their number. The hot wire is arranged to lie in one arm of a Wheatstone's bridge; so that its temperature may be deduced from its resistance. Innumerable experiments with different substances have shown that this emission increases with great rapidity as the temperature rises, just as does the corresponding phenomenon in the case of evaporation. The correspondance is, in fact, exceedingly close. We may take the rate of emission of molecules from the surface of an evaporating liquid to be proportional to the vapour pressure. The proportionality is not exact, but it is sufficiently so for our purpose. The crosses on the next slide represent values of the vapour pressure of water, on the vertical scale, plotted against the corresponding temperatures from 0° C. to 90° C, on the horizontal scale; whilst the circles represent the emission currents from platinum plotted similarly against temperature over the range 1000° C. to 1250° C. All the points lie on the same continuous curve within the limits of experimental error. To bring about this coincidence, it is, of course, necessary to plot the temperatures on quite different scales in the two cases, but the agreement demonstrates in a simple way the similarity of the laws which govern the temperature variation in both cases.

Numerous cases of electron emission have now been examined, and it has invariably been found, provided there is no reason to suspect changes in the chemical nature of the emitting surface, that the relation between the current i and the absolute temperature T is expressed by a very simple equation. This is

$$i = \mathrm{A}\,\mathrm{T}^{\frac{1}{2}}\ exp - \frac{b}{\mathrm{T}}$$

or

$$\log i - \tfrac{1}{2}\log \mathrm{T} = \log \mathrm{A} - \frac{b}{\mathrm{T}},$$

where A and b are constant quantities for any particular substance. The theory underlying this equation shows that the quantity b is very nearly equal to half the energy change, expressed in calories, when one gram molecular weight of the electrons is emitted. Pursuing the analogy with evaporation, this quantity may be called the molecular latent heat of evaporation of the electrons. It is not, however, with the theory underlying this equation that I particularly wish to concern you now; but I do wish to impress the fact that this formula is not an empirical affair covering a small range of temperature and current. The most recent measurements,* made with tungsten, have shown that the formula expresses the results within the limits of experimental error, over the range of temperature from 1050° K. to 2300° K. At the lowest temperatures the currents were less than one-millionth of a microampere per square centimetre, and had to be measured with a sensitive electrometer, whilst at the highest temperatures they were comparable with one ampere per square centimetre, and could be measured on a commercial ammeter. Thus the equation holds true whilst one of the variables changes by the enormous factor of 10^{12}. There are not many physical laws which will stand so severe a test as this.

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* *cf.* K. K. Smith, Phil. Mag. vol. 29, p. 102 (1915).

Let us now turn to some other consequences of the hypothesis that the emission of electrons is analogous to evaporation. One of the familiar effects of evaporation is to cool the liquid which gives off the vapour, owing to the latent heat of vaporization. In an exactly analogous manner, a wire which is giving off electrons will be cooled thereby. I think I can succeed in demonstrating this effect to you, although the lowering of temperature to be looked for is not very large, and delicate means have to be employed to detect it. This tube contains a hot tungsten wire, which is made to act as its own thermometer by placing it in one arm of a sensitive Wheatstone's bridge. Minute changes in its resistance can thus be measured. The bridge is balanced with the electrode surrounding the hot wire negatively charged; so that the thermionic current does not flow. If I reverse the potential and thus start the thermionic current, keeping the heating current constant, you observe a sudden deflection of the spot of the bridge galvanometer. The direction of this deflection corresponds to a reduction of the resistance of the hot wire, and thus to a lowering of its temperature. By experiments of this kind, Professor Cooke and I succeeded in measuring the latent heat of evaporation of the electrons directly.

Just as a liquid is cooled by evaporation, so it is heated to a corresponding extent when the vapour condenses. In fact, an elementary experiment with which every student of physics is familiar, consists in measuring the latent heat of evaporation by blowing steam into water. A precisely analogous experiment can be made with electrons. A large electron current from a hot wire is driven on to a fine strip of the metal, whose latent heat of condensation for electrons is to be tested. The cold strip is made to act as its own thermometer by placing it in one arm of a sensitive Wheatstone's bridge. When the hot wire is charged positively there is no electron current to the strip, and the bridge is balanced under these conditions. The wire is then charged negatively, so as to make the electrons flow on to the strip. There is then an increase in resistance, due to the heat liberated by the condensation of the electrons, which is measured. In these experiments only part of the observed change of resistance arises from the effect under consideration. The remainder is caused by the kinetic energy given to the electrons by the auxiliary field used to drive them from the hot wire to the strip. This, however, is easily determined and allowed for.

I have now indicated to you three independent methods of deducing the values of the latent heat of emission of the electrons. Let us see how the latest and most accurate values obtained by these methods agree with one another. The numbers found, and the names of the experimenters responsible for them, are shown in the following table:—

Values of Latent Heat of Emission reduced to Equivalent Temperatures.

1. From the temperature variation of the rate of emission.
    Tungsten (Langmuir) $10{\cdot}5 \times 10^4 - 11{\cdot}1 \times 10^4$ calories per Mol.
    Tungsten (K. K. Smith) $10{\cdot}94 \times 10^4$ calories per Mol.
    Platinum (various) $12 \times 10^4 - 16 \times 10^4$ calories per Mol.
2. From cooling due to emission.
    Tungsten (Cooke and Richardson) $11{\cdot}24 \times 10^4$ calories per Mol.
    Tungsten (Lester) $11{\cdot}04 \times 10^4$ calories per Mol.
    Platinum (Wehnelt and Liebreich) $13{\cdot}9 \times 10^4 - 14{\cdot}5 \times 10$ calories per Mol.
3. From heating due to condensation.
    Platinum (Richardson and Cooke) $13{\cdot}5 \times 10^4$ calories per Mol.

Unfortunately, the vacuum value for platinum given by the first method is still uncertain, owing to complications caused by gaseous contaminants. Except for this, the agreement between the different methods leaves nothing to be desired.

We come now to the very interesting question of the velocity and kinetic energy which these electrons possess when they are emitted. The fact that they are electrically charged enables us to find out a great deal more about their emission velocities than we can do in the corresponding case of the emission of ordinary molecules. By applying an external electric field we can influence the motion of the emitted electrons, and the precise nature of the effect exerted by the field depends on the velocity with which the electrons are shot off from the hot body. It is clear that we have no such method of controlling the motion of ordinary molecules.

I shall now consider one of the arrangements which has been used in applying these principles to the analysis of the emission velocities. The hot emitting surface is a small strip of platinum, electrically heated, which lies at the centre of a much larger metal plate. The upper surfaces of the strip and the plate are flush with each other, and are maintained at the same potential. Vertically above this lower plate and a short distance away from it is a parallel metal plate connected to the insulated quadrants of an electrometer. An arrangement is provided by which a suitable difference of potential can be maintained between the two plates, so as to *oppose* the motion of the electrons from the strip towards the upper plate. It is clear that if the electrons have no velocity when they are emitted, any retarding field, however small, will be sufficient to stop them from reaching the upper plate and charging up the electrometer. If, on the other hand, they are shot off with a definite component of velocity normal to the strip, they will reach the upper plate, provided the corresponding kinetic energy exceeds the work they have to do to overcome the opposing difference of potential. Thus, if the electrons are not at rest when they are emitted, they will give rise to currents capable of flowing *against* an applied electromotive force if this is not too large. . I have here an arrangement, similar in principle to that just described, which will enable me to show to you the existence of these currents flowing against an applied electromotive force. The platinum strip is replaced by a very short tungsten filament, the upper plate by a surrounding cylinder, and the electrometer by a galvanometer. The apparatus is thus different in detail from that already referred to, but the principle is the same. You observe that the current is largest when the opposing difference of potential is zero, and falls off uniformly and rapidly as the potential difference is increased. By increasing the temperature I can cause a considerable current to flow against an opposing difference of potential of one volt.

The experiments just referred to are a kind of electrical analogue of the high jump, in which the measuring tape is replaced by a voltmeter. Corresponding to each emission velocity there is a definite equivalent voltage. The fact that the current falls off continuously as the opposing voltage increases shows that the electrons are not emitted with a single velocity, but with different velocities extending over wide limits. Careful experiments of this kind have enabled us to discover what proportion of them are shot off with velocities within any stated limits, to determine, in fact, what is the Law of Distribution of Velocity among the emitted electrons.

More than fifty years ago Maxwell concluded, from rather abstruse theoretical considerations, that the velocities of the molecules of a gas or vapour should not all be equal, but should be distributed in a certain way about the average value. This law, known as Maxwell's law of distribution of velocity, is somewhat similar to that which governs the density of bullet-marks on a target at different distances from the bull's-eye. The theoretical considerations which led Maxwell to establish this law for gases apply equally to the atmospheres of electrons outside hot bodies. Let us see whether the results of our experiments agree with Maxwell's predictions or not. If the law of distribution of the normal velocity component for the emitted electrons is that given by Maxwell, it is necessary (and sufficient) that the currents i_1 and i_2, which flow against potentials V_1 and V_2 respectively, should satisfy the equation

$$\log \frac{i_1}{i_2} = \frac{Q}{RT} (V_1 - V_2),$$

where R is the constant in the equation $pv = RT$ of a perfect gas, and Q is the quantity of electricity which liberates half a cubic centimeter of hydrogen at 0° C. and 760 mms. in a water voltameter. The requirements of this formula are found to be fully satisfied by the results of the experiments. Thus the logarithms of the ratios of the currents are found to be accurately proportional to the differences in the corresponding opposing potentials at a given temperature. Again, since Q is a well-known physical constant and the value of T was estimated during the experiments, we can use the experimental data to obtain a value of the gas constant R. Eight experiments, made under conditions as varied as possible, when treated in this way gave values of R which varied between the extreme limits $3{\cdot}08 \times 10^3$ and $4{\cdot}46 \times 10^3$ ergs per c.c. per deg. C. These values exhibit a rather wide variation which, however, is believed to be fortuitous, so that the mean value should be much more accurate. The mean of the eight values gives $R = 3{\cdot}72 \times 10^3$, whereas the number given by the gas equation is $R = 3{\cdot}711 \times 10^3$ in the same units.

The fact that the value of the gas constant can be deduced in this way from purely electrical measurements must be regarded as a remarkable confirmation of the general position. The results of these experiments, and others of a similar nature which I have not time to describe, show not only that the velocities of the electrons are distributed about the average value in accordance with Maxwell's law, but also that the emitted electrons are kinetically identical with the molecules of a hypothetical gas of equal molecular weight at the temperature of the hot metal. The experiments referred to formed, so far as I am aware, the first direct experimental demonstration of the truth of Maxwell's law of distribution of velocities, and, although many of the consequences of this law have been made visible by the beautiful experiments of Perrin on the Brownian movement, I believe that they still furnish the most direct experimental verification of its truth.

Quite recently a number of experimenters have called in question the general position which I have taken as to the nature of the process of electron emission from hot bodies, and have asserted that this effect is caused by chemical action between the hot solid and traces of contaminants, usually supposed to be gaseous, which have access to it. Whilst I feel that the value of the evidence in favour of the latter hypothesis has, generally speaking, been greatly overestimated, it would take too long to discuss this question with the completeness which it demands. I shall therefore content myself with directing your attention to some experiments with tungsten filaments which prove that only an insignificant fraction, if any, of the emission from this substance can be attributed to chemical action.

Tungsten is peculiarly suited to these experiments on account of its great refractoriness. It can be heated in a vacuum for considerable periods at temperatures so high that all known impurities are volatilized out of it. The preliminary treatment of the experimental lamps furnishes some novel features which may prove of interest. The ductile tungsten filaments are electrically welded to the supporting leading wires in an atmosphere of hydrogen. After mounting, the lamps are exhausted in a vacuum furnace (with an external air pressure of about 1 cm.) at 550–600° C. for about 24 hours until the evolution of gas becomes very small. A Gaede pump is used for the internal exhaust at first, and, later on, liquid air and charcoal in addition. In the final stages the tungsten is glowed at about 3000° absolute, and, for the best results, the anode is heated by subjecting it to an intense electron bombardment from the hot wire. The conditions as to freedom from gaseous contamination which have been attained in this way are far superior to those which result from any other method of treatment.

With lamps thus prepared I have carried out simultaneous measurements of the rate of emission of electrons on the one hand, and either of the variation of the pressure of the gas present or of the rate of loss of matter by the filament on the other. Particular experiments have led to the following numbers:—

(1) For each molecule of gas given off the number of electrons emitted by the filament may be as high as 260,000,000.

(2) At each impact of a gas molecule with the filament 15,000 electrons would have to be emitted, and

(3) Each atom of tungsten which disappears from the filament would have to cause the emission of 984,000 electrons.

The magnitude of these numbers entirely precludes the possibility that chemical action plays any significant part in this emission. Again, the *mass* of the electrons lost by a filament may exceed the mass of tungsten lost in the same interval, proving that the emitted electrons are not furnished *at the expense of* the tungsten. They must, therefore, flow in from outside points of the circuit. Thus these experiments furnish a direct proof that the electric current in metals is carried by moving electrons. The mechanism of metallic conduction becomes more mysterious every day, but this, at any rate, is a fact which has to be reckoned with.

Perhaps I can drive these matters home to you more effectually by means of a simple experiment, which shows that these electron currents from tungsten in high vacua are not minute affairs requiring elaborate apparatus for their detection, but, at high temperatures, are of such magnitude as to be worthy of the consideration of the practical electrician. I have here a tungsten lamp, containing a filament 14 mm. long and about 3 mils. in diameter, in series with an ammeter, a resistance, a battery, and a second ammeter. They are arranged in the order named, so that there is an ammeter at each end of the lamp. In addition there is a side line from the cylindrical electrode of the lamp, which can be switched through either a millammeter or an electric bell to the positive end of the battery. There is no auxiliary voltage in this side line. When I turn the current on, you observe that the ammeters read differently, showing that more current is flowing into the filament at one end than out of it at the other. The difference is, in fact, equal to the electron current which flows into the wire sideways and is registered by the millammeter. Those of you who cannot see the instruments will, at any rate, hear the electric bell when I switch the electron current through it. With a lamp which was somewhat better designed for the purpose than the present one, I have recorded a current of 0·7 ampere at one end, 0·45 at the other, and 0·25 in the branch circuit. So far as my experience goes, the only limit to the size of these electron currents is that which is set by the magnitude of the current which fuses the filament, provided the requisite driving voltage is available.

[O. W. R.]

WEEKLY EVENING MEETING,

Friday, May 14, 1915.

EDWARD POLLOCK, Esq., F.R.C.S., Vice-President,
in the Chair.

The Rev. E. H. PEARCE, M.A., Canon and Treasurer of Westminster

The Archives of Westminster Abbey.

WHEN the body of the late Lord Kelvin was laid to rest, by a right which there was none to dispute, in the Abbey church of Westminster, it was placed by the same kind of right close to the grave of Sir Isaac Newton. In the same corner there are the graves or the memorials of Darwin and Herschel, of Joule, and Gabriel Stokes, and John Couch Adams, to be joined shortly by memorials of Sir Joseph Hooker and of another Joseph who died Lord Lister. It was not likely that Kelvin would long lack some monument more impressive than the slab which covers his remains; and it was appropriate that the representatives of engineering science on both sides of the Atlantic should undertake the task of providing it. The walls of the Abbey are full to repletion, and to the destruction of some precious features. The floor-space, as the centuries that followed nearest upon the Reformation were apt to forget, is reserved for the purposes of worship. But the windows of the Nave offer to those who would honour the great dead a means of fulfilling their desire and of adorning the church at the same time. Kelvin, then, has his memorial in stained glass, and the window is one of a series projected by Dean Armitage Robinson and loyally accepted by his successor—a series in which there are placed side by side a King of England who contributed to the majesty of the building, and the Abbot through whom he worked. The King in Kelvin's window is Harry of Monmouth; but it is the Abbot who concerns us now; for in such a scheme the ecclesiastics are more difficult to justify than the monarchs, not because of unworthiness, but because hitherto there has been but little effort to appraise their worth; and in this case the Abbot is William de Colchester. He flits craftily across the scene in the *Tragedy of King Richard II.*, in company with a Bishop of Carlisle whom we shall meet again. When Bolingbroke announces his coronation as Henry IV. in Richard's stead, the Abbot bids his friends—

> Come home with me to supper; and I'll lay
> A plot shall show us all a merry day. *

In the next act he is reported dead:

> The grand conspirator, Abbot of Westminster,
> With clog of conscience and sour melancholy
> Hath yielded up his body to the grave; †

enough in point of fact he survived the event by twenty years. What I want you to notice is that Shakespeare's authorities do not give my Abbot a name, and that he has no article to his credit in the *Dictionary of National Biography*. I do not propose to unravel his conspiracies, if he was guilty of them, though it will appear from what follows that he was trusted by Henry IV. who ousted Richard, and by Henry V. who enthroned poor Richard in his grateful memory. My business is with our Abbey archives and what they say about an obscure individual.

Let me take you, then, up a turret staircase through a door in the East Cloister into a noble apartment of which that cloister is the origin. For when Henry III.'s builders came to the planning of the South Transept, known as Poets' Corner, the lines of the Great Cloister were already long established, and they must not minish aught from them. Therefore, whereas the North Transept has aisles on its east side and its west, the South Transept is aisled only on the east. So the roof of the cloister upholds the floor of the apartment which we enter. We look out into the Abbey eastward through three of Henry III.'s bays across a low wall split up by his dwarf pillars. There are signs of royalty in the room, crowned heads at the capitals of the colonnade pillars; and on a wooden wall which shuts off the southern section is the outline of a white hart, crowned, the emblem of Richard II. Professor Lethaby has suggested to me that such a point of vantage for marking what stones and what buildings are here, and for witnessing the entry and the circuit of some procession of State as it arrived at Poets' Corner door from the Palace, would naturally be appropriated as a royal pew. Be that as it may, it was devoted in very early times to the purposes of a muniment room; it contains a cupboard which may well have been set up under King Richard's hart in King Richard's reign, and at least one of its archive chests belongs to the fourteenth century. Here, then, from that century onwards, the great Convent gathered and arranged its papers. For the last twenty years the Dean and Chapter have endeavoured to atone for previous indifference to the value of their treasures by entrusting to Dr. Edward Scott, formerly of the British Museum, the task of describing the documents and compiling an index. I will not tell you how many hours he has worked or how many descriptions he has written with his own hand, for the information will be out of date at the next meeting of the Chapter. I am merely going to construct a record of William Colchester from documents which I have spelled out with Dr. Scott's help. The story will atone for its dullness by being at least untold before.

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* Act IV., sc. 1, l. 332–3.

† Act V., sc. 6, l. 19–21.

William Colchester, as his name implies, was a Colchester man. In his time, and for some considerable space after it, the customary designation of a brother was his Christian name and a place name, with or without the copula *de*; in his earlier years he signed himself William de Colchester, but the documents which concern him as Abbot mostly speak of him as William Colchester. We are not, however, left to guess-work as to the place of his origin. In old age he busied himself with the endowment of anniversaries for the good of his soul. Here is one* dated May 20, 1406, in which he bargained with the Prior of St. Botolph, Colchester, and paid 40*s.* to Henry IV.'s Clerk of the Hanaper to seal the bargain, that one of the canon-chaplains of that Priory should say Mass every week, at sixpence a week, for his soul and for those of his parents; that the Prior and Convent should observe his anniversary, again with a memorial of his parents, in the parish church of St. Nicholas, Colchester; that a set sum should be distributed to the vicar, the poor, and the prisoners in Colchester Castle; and that the tomb of his parents in St. Nicholas' churchyard should be kept in proper repair. We may assume, then, that this was his native parish. But he knew to a few yards the distance of Colchester from Westminster, and how easy it might be for the Prior to accept his money and not fulfil his conditions; so in 1407, when he was arranging for his anniversary at our Abbey, he inserted a clause in that deed† to the effect that the Monk-Bailiff, who looked after the Westminster estates, when he was inspecting property in Hertfordshire, should proceed or send to Colchester and make sure that our Abbot's bequest was being faithfully obeyed—as who should say: "I don't trust these provincial priors further than I can see them."

Also, from the grant‡ of another anniversary at the Abbey's daughter-priory of St. Mary, Hurley, in Berks, we get to know that his father's name was Reginald and his mother's Alice. He had a sister who in 1389–90 was living in Cambridge, for his Receiver gave a tip of 12*d.* to a man who came from my lord's sister at that town; and we shall see that he had other connexions, some poor enough to bring him a basket of poultry, some rich enough to receive from him a present of jewellery. Clearly he sprang of a burgher stock of no eminence, for whom the Church seemed the sphere in which the career was most open to the talents.

How he came to enter our Monastery we shall never know, for with all the wealth of our materials there survives not a trace of his or any other applicant's testimonials. He came, he was seen, he was admitted. The requisites for admission were that he must have examined his conscience and his motives in applying, that he must be sound in body, free in civil status, unburdened by debt or other obligations, and as a rule not less than eighteen years of age.* A socially anxious mother once asked a head-master whether all due care was taken to inquire as to the position of the homes from which his pupils came. "Let me assure you, madam," he replied, "that so long as your boy behaves and works, we shall not trouble about the social status of his parents." So it was at Westminster.

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* *Mun.* 5259. † *Mun.* 5260 A. ‡ *Mun.* 3571.

Our first trace of William Colchester's name on the books is in connexion with his ordination. I cannot tell you what Bishop admitted him to the ministry, nor where it happened; but I know that in the year 1361–2 he said Mass for the first time, and I know it, not because it seemed to be an event worth chronicling of itself, but because three of our officers in that year tell me that they expended 1*s.* 7½*d.* each in bread and wine as "exennia"—i.e., a complimentary gift—sent to him in honour of the event. He was then, we may assume, twenty-three years of age; he probably entered the Convent in 1356, and he was probably born in 1338.

Five years after his ordination, in 1366, William Colchester was chosen by the Convent as one of two of their number whom they thought specially apt to learning, and whom it was their duty to send up to Oxford to join other Benedictine students at Gloucester Hall, an institution set up by the Order at its General Chapter held in Abingdon in 1290.† The custom with us was that the Convent Treasurer paid £10 yearly to each Westminster student for his maintenance, besides his expenses going and returning, so that it is possible to compile from the Treasurers' rolls a fairly complete list of our Oxford scholars from 1356, when I came upon the first signs of a definite system, till the Dissolution. The plan tended to the great advantage of the monastery; it meant that its likely young men at an impressionable time in their lives were taken out of the narrow rut of cloistral life and associated with the world of learning and affairs; and it will be found that a large proportion of those who were sent to Oxford rose quickly to positions of trust in the Convent. William Colchester was at Oxford from 1366 to 1370. I cannot say that the Latin prose of which he was capable does credit to his University, for Latinity was seldom more vile than that in which his few letters are couched. I content myself with assuming that he learnt there how to deal with men, and I can see that the Convent which had sent him there was satisfied with the results of its expenditure. For two things happened upon his return.

First, in the month of October, 1371, he was promoted, as the

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* Customary of Westminster, H. B. S. i. 261, 404.

† Reyner, *de Antiq. Bened. in Anglia.* App. p. 55.

Westminster phrase went, to sit by the bell, "sedere ad skillam"; that is, he moved up to the seniors' table in the Refectory, where was the bell which gave the signal for grace to be said or for the reader to begin the lection. Like the day of his first Mass, this promotion, which came usually ten years later, was reckoned to be an occasion for a little relaxation of the usual fare, and I can date it because the Sacrist and the Infirmarer and the Treasurer each sent him bread and wine to the value of 2*s.* 3½*d.*, so that he might make merry with his friends.

Secondly, he begins to be recognized as an experienced person who can safely be sent on missions involving prudence and judgment of men. There is a payment made that same year 1371–2 by the Steward of the Abbot's mansion of 20*s.* for the expenses of Brother William Colchester and two valets, who were sent to Northampton for the meeting of the General Chapter of the Benedictine Order.

In the next year, as we learn from the Sacrist, there fell to his lot a still more delicate duty. It was on this wise. Among the precious relics given to the Abbey by Edward the Confessor was the girdle of the Virgin Mary—"zona beate Marie"—which she had made with her own hands and had herself worn. It was regarded as of especial virtue in securing a safe delivery to expectant mothers, and, when our famous book of Customs was drawn up, about a century before William Colchester's admission, by Abbot Richard de Ware, it was the rule that the Sacrist, or as he was sometimes called the Secretary, should carry the girdle of the Blessed Mother of God to any destination which it was appointed to reach, or should be at charges with any one who was to be the bearer of it in his place.* So here is our Sacrist paying the expenses of William Colchester, 13*s.* 4*d.*, and the more considerable cost of buying two horses for the journey, £6 16*s.* 8*d.* But he has something to enter on the other side, an offering of £2 from the Countess of March, the lady who craved the aid of the girdle. If any one is churlish enough to say that the bargain was a poor one—150*s.* spent on the journey and only 40*s.* received from the beneficiary—the answer is that the horses would be sold at the end of the return journey for almost as much as they cost. If, again, it is objected that the lady's gift was in any case money thrown away, I can only say that, while on February 12, 1371, the previous year, Philippa, granddaughter of Edward III., did present her husband, Edmund Mortimer, third Earl of March, with a daughter, who in process of time became the wife of that Harry Hotspur of the house of Northumberland who fell at the battle of Shrewsbury, yet it is not on record that she was equally blessed in 1372 or 1373.

Such duties sensibly performed, William Colchester was not long in attaining to administrative office. To begin with, the Abbot

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* Customary of Westminster, H. B. S. ii. 49.

Nicholas Litlington made him his Custos Hospicii, i.e., Seneschal or Steward of his household. We have the roll in which the young monk gives an account * of his stewardship for the year Michaelmas–Michaelmas, 1373–4, and as the doings it records represented his first experience of that monastic business in which he was to be involved for nearly half a century, we may stay a moment to get our own impressions. He found his master possessed of a considerable rent-roll in different parts of the country—Worcestershire, Gloucestershire, Oxford, Surrey, Middlesex and Bucks. The revenues from these lands ran up to £696 13*s.* 6*d.*, and the sale of stock, including an ox sold for 18*s.* 4*d.* and a cow for 13*s.* "timore pestilencie," brought the total up to £719 8*s.* 8*d.*

Large as this sum sounds, it was none too large for the needs of the Abbot. Household expenses, not given here in detail, came to £151 1*s.* 4½*d.* The purchase of live-stock—grey palfreys, bullocks, cows, steers, sheep, pigs, swans, poultry, no less than 966 pigeons (at about ½*d.* each)—required £63 2*s.* 10*d.*, and the outlay on dead stock, such as bacon, salt fish, five barrels of white herring, fourteen casks of red herring, and three casks of Scottish red herring, amounted to £31 8*s.* 4*d.* Lest it should be claimed that the last variety was a special delicacy, I am bound to add that it seems more likely to have been a means of Lenten penance, for the red herring from Scotland was priced at 4*s.* a cask as against 5*s.* 6*d.* for that which had reddened elsewhere. Also, in case any housewife concludes from the figures of all this provision that there has been great waste, I note that it is William Colchester's duty to turn over his roll and to give a stock-keeping account, from which it will appear that at the end of the year his lordship has indeed entirely run out of herrings, but he still has five salt fish; his score of oxen is fourteen up and two still to play, while all the eighty pigs are gone where all good pigs must needs go.

I must not pursue in too great detail the rest of the expenditure—corn and wine and clothing and gifts to visitors. The total outlay of the year seems to have been £684 11*s.* 7¼*d.*, so that there was but little left out of £719. But I do want you to realize the sort of duties that fell on the young seneschal. He would get a pair of my lord's boots mended for twopence, or would spend sixpence in stringing the great sportsman's bows, and twopence on two bags in which to carrry his arrow-heads. But he was probably much more interested in the goings and comings of persons—the squire of the Earl of Cambridge, Edmund Langley, fifth son of Edward III., who receives 20*s.* for bringing a letter to the Abbot from his lord; the Earl of Warwick's steward, who comes to sell a black palfrey; a brother-monk, Richard Excestr', whom we shall meet again, and who was just starting on his career at Oxford, to whom the Abbot gives a fatherly tip of 20*s.*; the Bishop of Durham's man, whose master we know as the builder of Bishop Hatfield Hall at Durham, and who is sent with a gift of two greyhounds to the Abbot. Several messages come from the Prince, i.e., the Black Prince, who is now at Wycombe and now at Kensington, and the Abbot makes several journeys by boat to call on the Bishop of Winchester, no less a man than William of Wykeham, who was in some disgrace at the time.

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* *Mun.* 24517.

But as he had thus served the lord Abbot well, Colchester's next responsibility was placed upon him by the whole Convent, whose Treasurer he was for the year 1375–6, as well as Coquinarius or Kitchener. Happily we still possess his Treasurer-account. I must not go through it at length, but I cannot omit one feature of it, which you will find among his expenses under the head of "pitancie et flacones." Pitances were any additional meals on special occasions to vary the dismal round of dry bread and sour wine provided in the Refectory. But "flacones" were pancakes, and pancakes are still a Westminster institution, though the cost of providing them for my colleagues and myself is, unhappily, no longer regarded by the Ecclesiastical Commissioners as a legitimate feature of the Treasurer's balance-sheet. First, then, the item itself: Paid in milk, "creym," butter, cheese and eggs bought for pancakes in Easter week, on Rogation days and at Whitsuntide, 64*s.* 8*d.* And now for some further light upon it. In 1389, when William Colchester had been three years in the Abbot's chair, the Convent Kitchener was one William Clehungre or Clayhanger, and you will see from his bill how pancakes have developed in the interval. It sets forth his expenses laid out for the pancakes prescribed for the brethren, and delivered to the monastery according to its custom during fifty-six days each year, namely, from Easter Day to Trinity Sunday, in the twelfth year of King Richard II., as appears by all the parcels:—

	£	*s.*	*d.*
Milk.—First 126 gallons of milk @ 1*d.* the gallon .	0	10	6
Butter.—Also 3 gallons 3 qrts. of butter @ 2*s.* 4*d.* a gallon	0	9	4½
Eggs.—Also 5816 eggs @ 10*d.* a hundred . .	2	8	5¼
Salt.—Also one peck of salt @ 3*d.*	0	0	3
Total .	£3	8	11¾

Our Kitchener makes some trifling assumptions in his multiplication for the butter and the eggs, and he robs the Convent of fivepence when he adds up the total. Let us return to his pancakes or his omelettes, whichever they were, and, with an eye to that wise saw which insists that you cannot make such things without breaking eggs, let us note that the total of eggs means an average of 103 and a fraction per day, which, when we consider that in 1389 the prior and Convent numbered forty-nine persons, works out at the by no means excessive rate of 2½ eggs a person per day. There is another reason for dwelling on the custom. Westminster School is ostensibly a Tudor foundation, but at the Abbey we stand by the conviction that its roots are deep down in the pristine monastic soil. Every Shrove Tuesday the school's selected gladiators make a fiendish assault on a single pancake. What if there is surviving in this custom some ancient protest of the Benedictines against being fed on pancakes every day for eight weeks? What more natural than that the champion of these protestants should be led to the Lord Abbot to receive the Lord Abbot's equivalent for a guinea?

An occasion now arose in which Colchester's powers were to be put to a more enduring test. We are all familiar with the name "St. Stephen's" as loosely applied to Parliament House. Perhaps we are not all equally familiar with the fact that the House of Commons, after sitting for long years in our Chapter House at the Abbey, removed itself after the Dissolution to the ancient chapel of St. Stephen in the Palace of Westminster. The history of that chapel does not concern us now except in so far as it is related to William Colchester's career. Placed where it was, it stood within the limits of our Abbot's jurisdiction, but its Dean and his twelve Prebendaries had good grounds for regarding themselves as a royal foundation, and they craved the sort of independence which to-day attaches to St. George's Chapel, Windsor. Our Convent resisted this claim, which on the other hand was supported by the Royal Court. In or about 1377 the suit was entered before the Roman Curia, and some one must be appointed to take charge of our Convent's interests and do the necessary "lobbying" among the Pope's *entourage*. Their choice fell upon William Colchester. I will not go into the merits of the case which at the end of about seventeen years was decided in our favour; the College of St. Stephen agreed to pay to the Abbey five marks yearly, and our Abbot's right to instal the Dean of St. Stephen's was upheld.* My concern in the matter is to produce to you William Colchester's bill of costs,† and to use it as a guide to his journey and his doings.

He left Westminster on June 10, 1377, and was absent, as he is careful to tell you, for two years, twenty-three weeks and three days. His first business, as any traveller to-day knows, was to get his papers in order, consisting in this case of royal letters "pro expedicione cause" from the Keeper of the Privy Seal; and he tells you first that he paid 3*s.* 4*d.* to the Keeper's servant to urge his master to dictate them and the like sum to the scrivener who would execute them. As he sets out, you can see him reckoning up the difficulties of his ordeal. It was arranged that he should go by way of Avignon, for Thomas Southam, the Abbey lawyer, was still there settling up Cardinal Langham's will. But the Pope was no longer at Avignon. In fear of losing all hold on Italy, Gregory XI. had left that scene of luxurious exile and ravenous extortion on September 13, 1376, and had entered Rome on January 17, 1377.* Most Englishmen had loathed the Avignonese residence, because it threw the Papacy into the hands of the French; but William Colchester, as he packed his slender valise, thought differently. "Non potuit reperire societatem versus Auinionem." There was no great chance of getting company on the road; and company meant so much the more security. So he went to the fourteenth century representative of Messrs. Thos. Cook & Son and hired a courier, one Gerard of London, for 20*s*., pleading as his excuse for this expenditure "diuersitatem lingue et viarum discrimina in partibus transmarinis." To get to Dover he bought for himself a horse and saddle for 34*s*. 8*d*., but I a little fear that he expected the man Gerard to walk, for he also laid out 26*s*. 8*d*. upon a horse, saddle and bridle for Gerard, urging as his reason that the man declined to cover the journey on foot. And so we get them to Dover, where for five days they failed to procure a passage, and the two men and the two horses ate their heads off at sixpence a day per horse and fivepence a meal per man. When they got a ship, the men's passage cost 3*s*. 4*d*. each, the horses, by virtue of their cubic displacement, costing double. And so to Calais, and within three days to Bruges, where again there is a long halt. For the royal letters have not come; Edward III. died eleven days after our travellers left London. But Colchester is convinced that an enemy hath done this; the issue of the documents has been prevented "per aduersarios"; and you will remember that the Dean and College of St. Stephen's were closer to the royal ear than our Abbot and Convent. Anyhow there is nine days' commissariat at Bruges to be entered; also a reward given to the hotel servants "prout modus est patrie," according to the custom of the country; we can all enter into his feelings as he writes up that particular tenpence. In brief, he has already spent nearly all the £10 which he received at starting from Brother John Lakyngheth, his rival for monastic promotion.

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* J. T. Smith, *Antiquities of Westminster*, 1807, p. 100.

† *Mun.* 9256 C. D.

So now he converts his balance of 15*s*. 8*d*. into florins, reckoning each florin at 3*s*. 2*d*. To that he adds seven florins by the sale of his own horse—he has done well here, for the horse cost 34*s*. 8*d*. in London and he has ridden him to Bruges and sold him for 22*s*. 2*d*.; but the man Gerard's horse was the worse for wear—"infirmabatur per viam quasi ad mortem"—it went for 9*s*. 6*d*., or three florins. Colchester borrowed twenty-three florins, and on they went, sometimes hiring a mount, sometimes getting a lift, sometimes in terror of the Frenchmen, who laid an ambush for them as they entered Dauphiné, so that our travellers hired a guide to lead them through by-ways "extra viam communem." On the twenty-seventh day after leaving Bruges they entered Avignon, and the day following they joined Master Southam.

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* L. Pastor, *Geschichte des Päpste*, i. p. 109.

I turn aside for a moment to other documents, for Master Southam had with him at Avignon another Westminster monk, John Farnago, who becomes Colchester's paymaster and has left an account* of what he laid out on his behalf. Here we get the date of his arrival with the man Gerard—July 24—and the cost of their bed and board till August 19. Farnago bought his brother-monk a fresh outfit—cape, tunic and hood of black Benedictine cloth, a scapular and cowl and a plain collobium—bought, this last, from Hagyuus, a Jew; probably his exact name was Hayyim. He also bought him a horse for his journey from Avignon to Marseilles, where he was to take ship, and gave him some cash in hand. So Colchester turned his back on Avignon, hardly realizing perhaps that when on August 14, five days before setting out, he had witnessed the probate of Cardinal Langham's will, he had been concerned with a document which was to exercise a vast effect on the development of the church and the convent buildings of St. Peter, Westminster.

William Colchester omits to date his arrival in Rome, but we can tell roughly the outside limit. He says that, having got there, he had to move on to Anagni, about forty miles southward from Rome on the road to Naples, and we know that Gregory XI., who spent the summer of 1377 there, returned to Rome on November 17.† No doubt our monk returned with him, for the first date that he mentions is November 20. But I shall leave him to retain his various counsel, Italian and English, and to pay their fees, only asking you to notice that we are still using his expressions—"retinuit duos aduocatos"—and are not always innocent of his practices, as, for instance, when he gives six florins to the valet—"cubicularius"—of the Cardinal of Milan, with a view to his stirring up his master to sign a certain document; he did so, he explains, for greater security, because at the moment there was a fierce altercation between the parties to the suit.

But the expenses rose suddenly, for Gregory XI. died on March 27, 1378, and there can have been few more exciting experiences for an observant traveller than the election‡ which set Bartolommeo Prignano, Archbishop of Bari, upon what Colchester calls the "apex of the chief apostolate." The choice must have pleased our monk, for the new Pope was a *protégé* of the French Cardinal of Pampeluna, the friend and executor of our Cardinal Langham. But the actual effect was disastrous. Urban VI. ruled with "Neapolitan fire." "Inter Papam et Cardinales," says Colchester, "magna oriebatur discordia." Many Cardinals and curials fled secretly, and among the latter the two advocates whom he had retained. But, if he had to cut his losses and fee others, there was this advantage with Urban VI., that, knowing the preference of the Cardinals for Anagni as a summer residence, he decided through sheer perverseness for Tivoli, and Colchester could get there in two days for a couple of florins. After spending six weeks within sound of Horace's waterfall, he turned homeward to Nice, where, I am sorry to say, he was robbed of a cloak and other goods; thence to Avignon again, and thence, as he hoped, in due course to the Abbey.

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* *Mun.* 9228.

† Pastor, *Geschichte*, i. p. 113.

‡ Pastor, *Geschichte*, i. p. 118; Creighton, *History of the Papacy*, i. 61 ff.

But once more he had to turn about and revisit the Roman Court: for while he tarried at Master Southam's lodging in Avignon in September, 1378, there came news of a murder committed in the choir of the Abbey church, while the Gospel was being read at High Mass, on August 11. The victim was one Robert Hawle, who had escaped from the Tower and taken sanctuary in the Abbey. The incident raised all kinds of perilous questions, and Southam's advice was that Colchester should hasten to Rome and counteract the plots of the malefactors "perpetrantibus illud factum horribile." So now there is another long roll of expenses—Avignon to Marseilles, a passage in a galley to Ostia, most of December spent in Rome; fees on various occasions to the Pope's janitors for free entrance to the Chamber and the Consistory, and to his valets for access to the Pope himself; an expensive struggle on each side to extract the sort of Bull that each side desired, in which our man was apparently successful; a journey of forty-one days from Rome to Bruges; a wait of three weeks at Sluis for a passage across the Channel—"quod non erat ausus transire per Calis' propter metum aduersariorum"—and at last, by a way that was devious for the same reason, to Westminster in November, 1379. It was not easy nor over-safe to be the chosen representative of Westminster at the Roman Court.

There are three indications that the Chapter which paid all these expenses was satisfied with his execution of the task entrusted to him. First, within a brief space, he was sent to Rome again in 1382, presumably on the same business. We should have no knowledge of the journey, if it were not for two stray documents. One is the roll in which the Chamberlain of 1382–3 records the various garments issued by him to the brethren; for, mark you, our Chamberlain is so conscientious that he produces a balance-sheet in cloth as well as one in £ s. d.; and he notes that he did not give out "panni nigri"—the black Benedictine habit—either to W. Colchester or to W. Halle, another monk, "quia fuerunt Rome." The other is a legal document* of which in all honesty I must give the purport. Probably when he left Westminster in 1382 he knew that Richard Excestr' was about to resign the Priorship; anyhow, attempts seem to have been made to induce the Pope to "provide" Colchester to the vacant office—action which was an outrage on the prerogatives of the Crown. This document, then, which bears date January 2, 1384, sets forth the offence, and for a fee paid to the Hanaper pardons the offender for any prejudice or contempt thereby caused to the King. The Priorship another took. Not, I suspect, because the brethren thought Colchester unworthy of that office, but because they needed his help at Rome, whither, as I learn from the Treasurers' rolls, they again despatched him in 1384–5 and 1385–6. The suit with St. Stephen's College still dragged on, and he alone had the knowledge to push it. Indeed, he must have known Rome as familiarly as Westminster; and I suspect that his love for Westminster increased steadily with his knowledge of Rome.

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* *Mun.* 9503.

As a second proof of confidence, we note that in 1382 he was Archdeacon of the Convent. He may have held the post earlier; he certainly held it in 1386. The office of Archdeacon is one that is proverbially puzzling to the lay mind, and I sometimes think that the Archdeaconry of Westminster is a puzzle even to the other Archdeacons. The fact is that the Abbot, in exercising jurisdiction over his Westminster area, needed the services of an ecclesiastical jurist in matters of divorce and the like; and needed also some one who would be his pastoral representative to those inhabitants of his area who were not actually on the foundation of the Convent. Colchester's experience marked him out for such responsibilities.

That these were extensive and peculiar may be gathered from a letter* in Norman-French addressed to Abbot Litlington by one who signs himself "William Conte de Salisbury," that is, William de Montacute, the second Earl, who fought at Poitiers and throughout the French wars of his time. He addresses the Abbot as his dear and faithful friend, and then unfolds his story. His servant Nicholas Symcok, a Londoner, has been robbed in the middle of June by highwaymen, one of whom, Richard Surrey, is commonly known as Richard atte Belle—or Dick Turpin. Having made his bag of silver plate and £40 in cash, the said Richard has taken sanctuary at Westminster; hotly pursued, however, by his victim, who finds all his lost property in the villain's pockets, less £5. Thereupon he has lodged his recovered goods in the hands of Dan William Colchester, one of his lordship's monks, who has placed his official seal upon the package. "Therefore, my good lord," says the Earl, "I pray you have these goods and chattels delivered to my servant." The year is not given; it must be before 1386; and possibly here is our William Colchester performing archidiaconal functions, though the office is not mentioned.

Fortunately, I have also one intimation of his personal circumstances as the holder of this office, dated November 9, 1386. It is an indenture† of lease of sheep. It sets forth that Thomas Charlton, the valet, and Henry Norton, the servant, of William Colchester,

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* *Mun.* 9615.

† *Mun.* 5984.

Archdeacon of the Monastery of Westminster, let on lease to John Waryn, butcher, of Westminster, 132 muttons—"multones"—3 rams and 168 ewes, of the average value of 20*d.* each, to be fed and kept safe and sound till Ash Wednesday next; and there follows a statement of the terms upon which the tenant of the sheep may acquire any or all of them. You see that the Archdeacon's servants make the bargain, and I wondered for a time whether the sheep were his or theirs, till I turned the parchment over and found on the back: "indentura Willelmi Colchester de ouibus suis ad firmam dimissis." To-day our beloved Archdeacon has indeed a large and devoted flock, but, as far as his colleagues know, he does not graze sheep in his back-garden.

The third measure taken by the Convent to mark its satisfaction with Colchester's conduct of its affairs was more exceptional. I give the record of it as it exists in the vellum volume which we call "Liber Niger Quaternus" (f. 86 b), a fifteenth century copy of an earlier black paper register compiled by a very active monk called Roger Kyrton, or Cretton, who entered our House in 1384–5. It says:—

"On 25 Sept. 1382 there was granted to brother W. Colchester Archdeacon of Westminster a chamber with that part of the Garden which belongs to the Lady Chapel together with a pension of six marks [£4] and an additional monk's allowance ["corrodium"] such as is enjoyed by the seniors; but on condition that if the said William be promoted to any prelacy elsewhere, the pension, the allowance and the chamber are to revert to the Convent."

It is more than probable that these chambers lay on the south side of Little Cloisters, where to this day there is an alternation of old doors and old windows that suggests a row of almshouses. The site of his garden is more problematical, for the Lady Chapel is to the north of Little Cloisters.

If anyone asks what were the conditions in which each brother lived by himself, the answer can be given with a fair abundance of detail. We have a strip of frail paper,* 3 ft. 7 in. × 5½ in., which deals with the post-mortem distribution of the effects of one whom Colchester must have known long and well—Richard Excestr', who said his first Mass in 1361–2 (the same year as Colchester), and became Prior in 1377; he resigned the office, as we saw, in 1382, and retired into simple monk-hood again, though he was given precedence after the Prior till his death in 1397. His modest belongings are divided (like the items of a modern auctioneer's catalogue when the sale takes place by order of the executors "on the premises") according to the rooms; and we gather that he has an *aula* where he receives his friends, with a special welcome for those who can play chess (for he has a "tabularium cum familia"): a

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* *Mun.* 6603.

buteleria, or pantry, for his little store of plate and crockery and napery, including thirteen silver spoons, to show that there is no sordid superstition in monasteries; a *camera*, or bedroom, where he sleeps on a white bed with a tester and (I regret to say it) with two mattresses; a *coquina*, or kitchen, not ill-equipped with utensils, some of which, such as the trivet, are candidly described as "debiles"; and a *studium*, with ten books and three maps—among which of course there was some scholastic theology; but there was also a copy of the book of Messer Marco Polo, as if to signify that the latest modern literature was by no means excluded; and I gather from the Provost of King's, who is our unrivalled authority in such matters, that this is a very early instance of English interest in the Venetian traveller's adventures. I am told also that it is still rarer at this period that among his maps there should have been one of Scotland.

But higher promotion was at hand. On November 29, 1386, there passed away at his manor of la Neyte, near Westminster, our great builder, Abbot Litlington, to whom we owe the south and west sides of the Great Cloister, the Little Cloisters, Jerusalem Chamber, the Abbot's Hall, much of the present Deanery and our great Missal. The vigour of Litlington's character can be seen in the determined fight which he maintained through William Colchester for the Abbey's privileges; but we have another proof of it. In the "Liber Niger" (f. 87) there is a record to the effect that a threat of invasion by the French King in 1386 produced a unanimous resolution in the Chapter that the old Abbot and two of his monks should don their armour and proceed to the south coast for the defence of the realm. Lest anyone should doubt whether Nicholas Litlington had any armour to don, I call in evidence a schedule* of such of his effects as passed at his death into the possession of his successor, and you will see that these consisted mainly of various accoutrements: six hauberks; a helmet ("pisanum"); seven basnetts (also helmets), with visors; a ketelhat (a *pickelhaube*); a pair of plated gloves to give him a mailed fist; "legharneys"; fore-braces and back-braces; and four lance-heads.

William Colchester was well-nigh disappointed of the high office to which he was entitled. He had spent so many years abroad, certainly the last nine years with scarcely a break, and all the time engaged in a cause in which the Court influence was against him, that it is not wonderful if Richard II. desired any other candidate to succeed; and, in fact, a Westminster chronicler, whom Dean Robinson believes to have been the rival candidate himself, has left a statement† that the King wrote thrice to the Prior and Convent urging the election of John Lakyngheth, the very Treasurer who, perhaps with the keenest satisfaction at thus facilitating his rival's absence, had disbursed to William Colchester the funds necessary for his long journeys and his law costs. But the Convent had made up its mind, and within a fortnight Colchester was elected by compromission. The King, though he was afterwards reconciled, refused for a time to receive the nomination. It is well to bear this incident in mind when you find Shakespeare's authorities suggesting that Colchester was an "arch conspirator" on Richard's behalf.

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* *Mun.* 5446.

† J. Armitage Robinson *An Unrecognised Westminster Chronicler*, 16, 22.

Certainly there is no doubt about Richard II.'s continued favour and generosity to the Abbey. Here is a deed of gift, May 28, 1389, of a set of Mass vestments of cloth-of-gold embroidered with representations of the Holy Trinity, St Edward the Confessor and St. Edmund the King. In 1394, after the death of his Queen, Anne of Bohemia, came his grant of £200 a year to maintain her anniversary, and his own when he should depart hence; which was followed in April, 1399, by a grant of manors and lands in Middlesex, Bedfordshire and Berkshire, from whence the sum of about £200 yearly could be permanently derived. These matters and the Abbot's arrangements for his own anniversaries at Aldenham in Herts, at Hurley in Berkshire, at Colchester and at Westminster, are well known, through the care with which our predecessors guarded their precious documents.

But we can show what is less known—how he managed his estates and ordered his life. Times were bad, and farmers fell in arrear. In 1388–9 * these arrears were £104, or one-sixth of his rental. But stock is plentiful. He has 58 horses and 19 foals; 351 heads of cattle; 2287 sheep and lambs; and 299 pigs. When he listened to the lay-clerks in the choir chanting the 144th Psalm he had every reason to desire that there might be "no complaining," and that our sheep might "bring forth thousands and ten thousands in our streets." But the rolls of his Seneschals—we have six between 1388 and 1403—are of interest for the light that they throw on the man. He did not ail much. He was not very well, indeed, in 1389, and once a doctor had a shilling from him for curing his tibia, which was, I find, a peculiarly vulnerable part of monkish anatomy. He does not seem to have been a mighty hunter before the Lord like Nicholas Litlington; otherwise his steward would hardly buy 359 rabbits, 41 woodcock, and a pheasant. Certainly he was careful and even frugal in his management, as a single document is sufficient to imply. I have already mentioned the monastic custom of "exennia," gifts in kind, on certain rare occasions of rejoicing in a monk's life. The advantage of being Abbot was that you had "exennia" every year from a number of folk who liked to be in your good graces, heads of daughter-convents, vicars of parishes on the estates, monks who liked the great man to know that they could ply the rod or bend a bow; the King's larderer with a dish of fish; Master Southam, the

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* *Mun.* 6165.

lawyer, who sends a whole pipe of wine as a reminder that he would have no objection to administering Colchester's estate in due course, as he had administered Cardinal Langham's; and, most pathetic of all, two women "of my lord's kindred, as they affirm," with a gift of six capons. The point is that each one of these eighty-three contributions is entered by the Seneschal at its market price, from the pipe of wine at 53*s.* 4*d.* to the six capons at 2*s.*, and the total value £14 11*s.* 6*d.* was added by the steward to his receipts for the year. Prudent housewifery—if the word may pass in a celibate context—can hardly go further.

It is natural to ask if there are signs of luxury or excess. Not in his furnishing. In 1401 he was adding to the accommodation of his London manor, la Neyte. For the new parlour he procured a cupboard at 10*s.*, two chairs for 4*s.* 6*d.*, six stools for 4*s* 4*d.*, and a deal table for the like sum. The word is not quite clear, but it looks as if his study window had a curtain, costing 1*s.* 8*d.*, and there must have been a fire-place in the parlour, for his Seneschal spent sevenpence on coal for it. Nor was it wine and strong drink, for his outlay on that was about a sixth part of what he spent on corn and meat. Nor, again, did he use his position to enrich his relations; it may be that we can detect a poor kinsman in one John Colchester, who was given 3*s.* 4*d.* by my lord's command at la Neyte in March, 1389, and it was quite possibly for his sister-in-law—it was at any rate for the wife of Thomas Colchester—that he ordered a gift of an "anulus de auro cum diamandys" on May 31 of that year at a cost of 40*s.*, perhaps because it was her birthday.

He liked to have one or two of the younger monks staying with him, especially Thomas Merke, afterwards Bishop of Carlisle and his fellow-conspirator against Henry IV. They went his errands for him, and on occasion he would give Merke and another half a mark between them "pro gaudiis," that is, for a treat—the word "gaudy" is still applied in Oxford to those college entertainments which for the moment are in patriotic suspense.

Even on his travels the Abbot maintained no extravagant state. There must be formal visits to the manors. There must be "command visits" to the King, and the fact that he had to wait upon Henry IV. three times in 1402–3 at Ware and at Windsor and at Berkhamstead does not give countenance to the idea that he was then regarded as a plotter against the new *régime*; but I mention it because the three occasions cost only £4. And when his Seneschal comes to give 1*s.* 8*d.* to a countryman for guiding my lord out of the forest of Rockingham I can only conclude that the Abbot's retinue was so small as to make him nervous of the attentions of Robin Hood. Certainly he can hardly be blamed if he was frequently visited on business by officials who were not averse from a tip. They mostly accepted less than the 20*s.* imparted to the Remembrancer of the Exchequer, but the three-and-fourpences mounted up when the Sheriff of Middlesex and his valets and his boy, a herald from the College of Arms with his boy, a messenger with a summons to Parliament, and Criers from the King's Bench, came hard upon each other's heels.

One luxury the Abbot did indulge in, whether from preference or at the dictates of fashion. Probably from preference, for otherwise there was no need to make two clerks carry to his manor at Pyrford a pair of organs from Westminster. Here is a boy dancing before my lord for sixpence, and a piper retained at Pyrford all Christmastide for 14s. Clearly his tastes got to be known, for William of Wykeham sent his pipers to strut their little hour before the Abbot at Pyrford; Henry de Spenser, the fighting Bishop of Norwich and a champion of Richard II., sent his minstrels to Birlingham; the Duke of Gloucester—Thomas of Woodstock—had a blind harper who performed at Denham; the Abbot of Eynsham's entertainer ("lusor") and the ill-fated Earl of Arundel's minstrels were other visitors; and even when he was at Northampton, for the General Chapter of the Benedictine Order, there are payments to "interlusores" or mummers.

If I turn to the man himself and his public career as Abbot, I see him conscious of the troubled conditions of the realm under King Richard's rule. Listen, for example, to the message* which he sent from his manor in Denham on the last day of August in a year not specified:

"My beloved sons in Christ, the King's most excellent Majesty has bidden us to be instant in prayer to the Most High for the whole estate of the realm; for enemies without and rebels within are confederate against the peace of the country. It therefore behoves you to whom is committed the administration of our Convent's concerns to put a limit—all of you save the Monk-Bailiff—upon your walks abroad and your ridings into the country. Call your brethren to chapter and bid them be content with their usual recreation within the walls; let them give themselves to prayer and contemplation such as the present stress and the evils of the times demand. Go in solemn procession every fourth day round the limits of the monastery and every sixth day through the town of Westminster and in the latter case summon all the chaplains and clerks dwelling within the parish of St. Margaret, and specially, in accordance with custom, the clerks of our Almonry. Fare you well."

But I see him also compelled to be as heretofore a busy wanderer in foreign lands in the service either of the King or the monastery or the Church, and mostly, I suppose, of all three at once. From 1387–90 he was represented at Rome by his proctor, John Borewell, who succeeded him as Archdeacon. In 1391 he was off again himself, this time on the King's business, as we learn from the "Liber

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* *Mun.* 6221.

Niger" (f. 87 b.); and then for some years he may have secured a rest, though he seems to have been in Ireland in 1399. But in 1407, when he was well-nigh seventy, the travels were resumed. I know it because his farm bailiffs bring the rents to his Receiver instead of to the great man himself, and because there is an undated letter of his written from Cologne * to two monks, who were slow about some business as to which the old man was keen and peremptory, and of whom one was dead by 1410. By the same clue, as well as by official documents, I can follow him abroad again in 1412 and again in 1414, this time to attend the great Council of Constance with a long retinue; and I like to think that before leaving he had a useful interview with the Primate, for the Abbot's Treasurer enters "*8d.* for boat-hire when my lord dined at Lambhyth with the Archbishop."

In between those two journeys—on Passion Sunday, 1413—there came the great and bitterly cold solemnity of Henry V.'s Coronation, and when the brave King's chantry was set up, the sculptor was bidden to recall Abbot Colchester's features as he stood at the monarch's left hand. Even the journey to Constance did not blind our Abbot to his country's danger and his own power to help. I was searching for something else the other day in the Treasurer's roll for 1414-5, the year of the preparation for Agincourt, and found an entry of £33 expended in a new chariot with six horses, with a complete set of harness, which had been given by the Convent to the King on his departure for France, together with the stipends of a valet, a boy and a page and keep for the horses for three weeks; it is noted in the entry that on his part the old traveller-Abbot had done the like. The two outlays together come to nearly £1000 of our money.

He died some time in the autumn of 1420 after an Abbacy of thirty-four years, and his tomb is still with us in St. John Baptist's Chapel. To the last his natural shrewdness was not abated. We have the account † for the last year of his life of his outlays in connexion with the General Chapter of the Benedictines at Northampton. There are payments in it to two Westminster monks for services rendered at that conference. They were Richard Harwden and Edmund Kirton, the two men who were elected in turn as Abbot in his place. It is not every man of eighty who can spot his successors for forty years ahead and encourage them to prepare themselves.

Once more, as he moved among men, treading no doubt at times in slippery places, he seems to have retained their regard. I have spoken to you of a young monk, one Thomas Merke, whom Colchester befriended, and who became Bishop of Carlisle, dying in 1409. We have a letter ‡ from him to our Abbot in which the latter is asked to give his aid to some cause at the Roman Court. "Men like," said the Bishop, "to know your Paternity's views on these matters, for they note your solidity, which is a rare virtue in our days, and they give you their confidence all the more." I believe that to be true, and not mere courtesy. I think that the man whom it has been my privilege to introduce to you, while he travelled as much, was a Ulysses also in men's estimate of his wisdom. He could say:

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* *Mun.* 1653. † *Mun.* 12397. ‡ *Mun.* 9240.

> I am become a name,
> For always roaming with a hungry heart
> Much have I seen and known; cities of men
> And manners, climates, councils, governments,
> Myself not least, but honour'd of them all.

[E. H. P.]

WEEKLY EVENING MEETING,

Friday, May 21, 1915.

Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S. Treasurer and Vice-President, in the Chair.

Edward Heron-Allen, F.L.S. F.G.S. F.R.M.S.

On Beauty, Design and Purpose in the Foraminifera.

In the Dawn of History the Tartars in their flight before the victorious army of Ladislaus, King of Transylvania, scattered money as they fled, trusting to the apparently already established instincts of the Teuton soldiers that their pursuit would be thereby arrested. But King Ladislaus prayed that this money might be turned into stones, and his prayer was immediately granted. Hence the Nummulites (Sl. 1).[1] This, at any rate, is the account given of the matter in the 16th century by the learned Clusius,[2] and it is probably the first mention of the Foraminifera in print. The equally learned Strabo, however, had recorded that the Egyptian Nummulites were the petrified remains of beans left behind them by the builders of the Pyramids,[3] in spite of the explicit statement of Herodotus that the Egyptians never grew or ate beans in any form.[4] This Nummulite, which rightfully claims to be the earliest recorded Foraminifer, is also the highest and most complex of its Order (Sl. 2), and it was based upon his study of this Family that Dr. Carpenter in 1885 claimed for the Foraminifera that they are the most highly specialized and structurally developed of the Protozoa.[5] "They stand at the summit of a long branch of the whole tree of life,"[6] and have with perhaps the single exception of the Globigerinidae, played a more important part in the building up of vast tracts of the Earth's surface than any other organism. The Nummulitic Limestones (Sl. 3) stretch in a broad band, in many places several thousands of feet in thickness, across Europe and Northern Africa, and through Asia by the Himalayas to China, the matrix, containing the perfect fossils, being a rock formed of their comminuted remains. The deposit is characteristic of the Eocene period; but the Nummulites have now died out, being represented to-day in the tropics by a single living species, *N. cummingii* (Sl. 4).

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[1] A list of the slides exhibited will be found at the end of the Discourse.

[2] Caroli Clusii et aliorum epistolae, Ep. xxxvii. Paris (c. 1550).

[3] Strabo, Geographica, bk. xvii. cap. i. 34.

[4] Herodotus, Euterpe, ii. 37.

[5] W. B. Carpenter, On the Structure of Orbitolites. Journ. Quekett Micr. Club, ser. 2, vol. ii. p. 102.

[6] P. Chalmers Mitchell, Art. "Evolution" in Encycl. Britannica, 11th ed. vol. x. p. 35. 1910.

Coeval with the Nummulites, and closely approximating to them in importance as world builders, is the genus Alveolina (Sl. 5), which is found in the same beds either gradually replacing them, or sometimes taking their place with startling suddenness in the strata. Off the extreme point of Selsey Bill, in Sussex, the locally named "Mixon reef" rises at the summit of the Eocene deposits, composed almost entirely of fossil shells of *A. boscii* (Sl. 6), indistinguishable from the living shells of the species which abound to-day in the shallow water and littoral sands of Australian and other tropical shores.[1]

With all respect, however, to the recent utterances of its most noteworthy protagonist,[2] the Nummulite is a mere parvenu compared with the species *Spirillina groomii* (Sl. 7), discovered in the Cambrian rocks of Malvern by Chapman,[3] and rediscovered by Arthur Earland and myself alive in the shallow waters of the West of Ireland,[4] which probably represents the earliest specific form of life to be found living at the present day. Even the conservative little Lingula shell has become slightly modified since its earliest ancestors wallowed in Cambrian mud a hundred million years ago.[5]

I have alluded to the Globigerinae (Sl. 8), which are to-day forming a geological deposit of unknown thickness over 48 millions of square miles in the modern oceans.[6] Agassiz has observed (Sl. 9) that "no lithological distinction of any value has been established between the chalk proper and the calcareous mud of the Atlantic,"[7] and it has been estimated that the time occupied by the deposit of the English chalk, arguing by the rate at which the Atlantic ooze is formed (which is about one foot in a century), must have been 150,000 years.[8]

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[1] E. Heron-Allen and A. Earland, The Foraminifera in their rôle as World-Builders. Journ. Quekett Micr. Club, ser. 2, vol. xi. pp. 9-11. 1913.

[2] R. Kirkpatrick, The Nummulosphere. London, 1913, etc.

[3] F. Chapman, Foraminifera from an Upper Cambrian Horizon of the Malverns. Q. Journ. Geol. Soc., p. 257. 1900.

[4] E. Heron-Allen and A. Earland, The Foraminifera of the Clare Island District. Proc. R. Irish Acad., vol. xxxi. (Clare Island Survey, pt. 64), p. 107, pl. ix. figs. 2, 3. 1913.

[5] Cf. E. Heron-Allen, Selsey Bill. London, 1911, p. 24.

[6] Sir. J. Murray, The Ocean, p. 207. London, 1913.

[7] A. Agassiz, Three Cruises of the Blake, vol. i. p. 150. London, 1888.

[8] A. J. Jukes-Brown, Handbook of Physical Geology, p. 130. London, 1884. The rate of deposition varies slightly according to depth. See Murray, op. cit. p. 224.

As Maury has picturesquely said, "The sea, like the snow-cloud, with its flakes in a calm, is always letting fall upon its bed showers of microscopic shells."[1] These are some of the Foraminifera (Sl. 10) that may be washed out of any ordinary lump of Upper Chalk. In many localities on a smaller scale the Foraminifera may be seen occupied in this process of World Building. The shore of Dog's Bay, in Connemara (Sl. 11), is composed of sands in which no grain of sand has a place. As far as the eye can see, and as deep as man can dig, preserving any measure of self-respect, the littoral deposit consists of pure Foraminifera (Sl. 12) extending far above high water-, and far below low water-marks. In a lesser degree the same thing may be seen a little to the north, just south of Emlagh Point (Sl. 13), while anyone who has taken the trouble to examine the grit shed in disconcerting quantities by a new Mediterranean sponge (Sl. 14) must realize what masses of Foraminifera make up the bulk of the shallow water sands in those latitudes.

Such then shortly is the occurrence of the Foraminifera, which, leaving on one side the doubtful record of *Strombus lapidus* by Gesner in 1565,[2] which Prof. Rupert Jones identified as a Vaginulina,[3] make their first appearance in the Micrographia of Hooke in 1665[4] as "Figures observed in small sand." He figures one of them (Sl. 15), which is clearly the common shore form *Rotalia beccarii*. In 1702 Dr. Plimmer's "Immortal Beadle,"[5] Antony van Leeuwenhoek, in a letter to the Royal Society,[6] figured (Sl. 16) the equally common and related form *Polystomella striato-punctata* "from out of the stomach of a shrimp," in which happy hunting-ground Reade recorded the presence of Foraminifera more than 150 years later.[7] There can be no doubt that they play an important part in determining the movements of many of our most important food fishes.[8]

Since the day of Leeuwenhoek the Foraminifera have continually engaged the attention of zoologists. Before Linnæus we have the works of Plancus,[1] Ledermüller,[2] and others, but between the time of Linnæus and the early years of the last century the era of monographs began; Walker and Boys in England,[3] Fichtel and Moll in Germany,[4] Lamarck in France,[5] Soldani in Italy,[6] have left behind them specialist works upon the Foraminifera which still form (sometimes to our serious embarrassment) the foundations of our study.

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[1] M. F. Maury, The Physical Geography of the Sea, 15th ed. p. 322. London, 1874. Cf. H. N. Moseley, Notes of a Naturalist on the Challenger, p. 582. London, 1879. "The dead Pelagic animals must fall as a constant rain of food upon the habitation of their deep-sea dependants."

[2] C. Gesner, De omni rerum fossilium genere, gemmis, etc. (Last sect. p. 165.) Tiguri, 1565.

[3] T. Rupert Jones, Q. Journ. Geol. Soc., pl. xxxiv. fig. 5, Vaginulina laevigata. 1884.

[4] R. Hooke, Micrographia, p. 80, pl. v. fig. x. London, 1665.

[5] H. G. Plimmer, Bedellus immortalis (Presidential Address), J.R. Micr. Soc., p. 121. 1913.

[6] A. van Leeuwenhoek, Sevende verfolg der Brieven, p. 196, pl. opp. p. 191, fig. 7. Delft, 1702.

[7] J. B. Reade, Trans. Micr. Soc., vol. ii. pp. 20-24. London, 1849.

[8] F. Pearcey, On the Movements and Food of the Herring, Proc. Roy. Phys. Soc. Edinburgh, vol. viii. p. 389. 1885.

The recent period may be said to have commenced in 1819, when the father of Alcide d'Orbigny wrote to the geologist Fleuriau de Bellevue that his son was studying "microscopic cephalopods" from the shore sands at Esnandes, near their native town, La Rochelle.[7] After this, captains of ships and travelling naturalists supplied young d'Orbigny with a mass of material from all parts of the world, resulting in the publication of his Tableau Méthodique,[8] in which a vast number of species both recent and fossil were recorded. His records from Madagascar in particular are of supreme interest for us, for we have recently examined a series of dredgings from Kerimba, (Sl. 17) on the adjacent African coast,[9] in which we have rediscovered most, if not all, of his Madagascan species. He recorded in particular the species *Pavonina flabelliformis* (Sl. 18), which after 1826 was entirely lost sight of for half a century, when it was rediscovered in Madagascan sand by Brady.[10] It is quite one of the most beautiful of the Foraminifera, whether viewed as an opaque object or by transmitted light (Sl. 19).

The true nature of the Foraminifera was not however understood until Dujardin in 1835[11] separated them from the Cephalopods, among which they had been grouped on account of certain superficial characteristics, and their extensile bodies. From this time onward the literature of the Foraminifera has expanded into a vast body of memoirs and monographs in every European language.[12]

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[1] Janus Plancus, De conchis minus notis liber. Venice, 1739. 2nd ed., Rome, 1760.

[2] M. F. Ledermüller, Mikroskopische Gemuths-und Augen-Ergötzung. Bayreuth, 1760–1.

[3] Walker and Boys, Testacea minuta rariora. London, 1784.

[4] Fichtel and Moll, Testacea microscopica. Vienna, 1798.

[5] J. B. de Lamarck, Système des Animaux sans Vertèbres. Paris, 1801.

[6] A. Soldani, Testaceographia. Senis, 1789–98.

[7] C. d'Orbigny, Journal de Physique, vol. lxxxviii. p. 187. Paris, 1819.

[8] A. d'Orbigny, Tableau Méthodique de la Classe des Rhizopodes, Ann. Sci. Nat., vol. vii. pp. 245–314. Paris, 1826.

[9] E. Heron-Allen and A. Earland, The Foraminifera of the Kerimba Archipelago, Trans. Zool. Soc. (Lond.), pt. i. vol. xx. (1914), p. 363; pt. ii. vol. xxi. (1915). (In the press.)

[10] H. B. Brady, Challenger Reports, vol. ix., Foraminifera, p. 375, pl. xlv. figs. 17-21. 1884.

[11] F. Dujardin, Observations nouvelles sur les pretendus Céphalopodes Microscopiques, Bull. Soc. Sci. Nat. France, no. 3, p. 36. 1835.

[12] C. D. Sherborn, A Bibliography of the Foraminifera. London, 1888.

The Foraminifera are to be found in all parts of the world and under all conditions, on the shore, in deep sea soundings and dredgings, and floating at all depths of the ocean, whence they are taken in tow nets, and they divide roughly into two great classes, the Calcareous (Sl. 20), which secrete from the surrounding waters a delicate and beautiful shell of carbonate of lime, and the Arenaceous (Sl. 21), which build their shells out of sand-grains, sponge-spicules, and other fortuitous materials, often affording remarkable indications of phenomena of purpose and intelligence to which I shall presently refer. A single species, *Carterina spiculotesta* (Sl. 22), builds its shell of fusiform calcareous spicules, secreted by the animal itself by a process which is at present entirely obscure. A remarkable feature of the organism is that whereas the resulting spicules on the upper surface follow the convolutions of the chambers, on the under side (Sl. 23) they are turned inwards so as to converge towards the central umbilicus.

The distinction between the calcareous and arenaceous shells is purely artificial, isomorphs existing between the three great classes, the hyaline (or perforate), the porcellanous (or imperforate), and the sandy (or arenaceous). A typical isomorphism (Sl. 24) is represented by the three genera Cornuspira, which is porcellanous, Ammodiscus, which is arenaceous, and Spirillina, which is hyaline.

I will now show rapidly some of the most striking species to be found in the sub-families of the order, beginning with the imperforate porcellanous Miliolidæ.

Nubecularia lucifuga (Sl. 25) is a wild growing and usually adherent form, which under its coarse and shapeless exterior has its chambers arranged on the plan of several other genera.

Biloculina serrata (Sl. 26) is one of a genus in which the chambers are arranged in regular pairs opposed to one another.

Spiroloculina nitida (Sl. 27). In this genus the chambers are spread out upon a single plane so that all are visible on either side.

Miliolina seminulum (Sl. 28) is one of the commonest of our shore species. The genus assumes a vast variety of superficial decoration (Sl. 29), one of the most striking being *M. berthelíniana*.

Massilina secans (Sl. 30) is one of our commonest forms, often forming thick white bands and patches on our shore sands.

In *Cornuspira foliacea* (Sl. 31) the monothalamous shell is arranged in a flat spiral.

A typical arenaceous form is *Saccammina sphærica* (Sl. 32), among the monothalamia, and *Haplophragmium crassimargo* (Sl. 33), among the polythalamia, which, broadly speaking, are either spirally wound, or lituiform, as in *H. pseudospirale* (Sl. 34), or with the chambers alternating, as in *Textularia agglutinans* (Sl. 35).

Webbina clavata (Sl. 36) clings to mineral or shell fragments, communicating with the surrounding medium by a delicate tube.

One of the most beautiful and delicate of the arenacea is *Reophax* *scottii* (Sl. 37), which is built entirely of minute flakes of mica cemented together at the edges.

The finer Textularians are often beautifully hyaline, e.g. *Bulimina aculeata* (Sl. 38) and *Bulimina subteres* (Sl. 39).

The Lagenidae (Sl. 40) assume an infinite variety of beautiful forms.

Nodosaria filiformis (Sl. 41) is one of the most typical species of its genus.

Vaginulina margaritifera (Sl. 42).

Frondicularia alata (Sl. 43).

Cristellaria costata (Sl. 44).

Cristellaria tricarinella (Sl. 45).

Polymorphina regina (Sl. 46) is a very handsome representative of a genus, which is frequently found attached to sand grains and shells by fistulose processes (Sl. 47).

The Globigerinae (Sl. 48), to which we have already alluded, are often merely stages in the life history of *Orbulina universa* (Sl. 49), which we may break open and find in it the earlier Globigerina.

The Discorbinae are a vast family, of which I show *D. vesicularis* (Sl. 50).

Pulvinulina is represented by *P. pauperata* (Sl. 51).

The list closes with the Nummulitinae, to which the Nummulites proper belong, as also the common shore forms *Polystomella crispa* (Sl. 52), and *P. macella* (Sl. 53), of which a specimen is shown viewed by transmitted light.

The object and significance of this variety of beautiful forms are entirely obscure, but have engaged the attention of many biologists both in the Foraminifera and in the Radiolaria.[1] It must be remembered that a Foraminifer like a Radiolarian is a unicellular animal consisting solely of a microscopic globule of undifferentiated protoplasm, vivified by a nucleus whose functions are little understood, but which are essential to the existence of the animal.

This protoplasm is extended in the form of fine filaments which intermingle freely with one another, called pseudopodia, and their mechanical functions are locomotion and the capture of food. I have made a series of photographs of a fine Gromia which emerged from the mud in one of my tanks (Sl. 54), climbed up the glass (Sl. 55) until it encountered a sea-weed stem, up which it crawled (Sl. 56), and finally drew itself off on to the glass again by means of its pseudopodia. It then got lost, but on punching a hole in a piece of black paper which was then gummed on to the glass and a strong beam of light directed through it (Sl. 57), it came back into the circle of light, apparently indicating that these creatures are sensitive to light.

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[1] Cf. F. Gamble in Ray Lankester, A Treatise on Zoology, pt. i. fasc. 1, pp. 130, 131. 1909.

The other important function is the capture of food, which is caught outside the shell and usually drawn into it, as in this case (Sl. 58) of *Miliolina durrandii*, which has ingested a smaller Foraminifer and a diatom.[1]

It is a most significant, and at present unexplainable, fact that these minute protoplasmic globules secrete such varied materials for the formation of their shells from the surrounding medium, as carbonate of lime, silica, and even sulphate of strontium, but the nature and character of their protoplasmic bodies which perform this function defies analysis or definition in the present state of our knowledge.

Their life-cycles have been carefully studied by Lister,[2] Schaudinn,[3] Winter,[4] and others, and the fact has been established that most, if not all, Foraminifera exhibit the phenomenon called dimorphism (that is to say, they start with a large or a small central chamber) (Sl. 59 and Sl. 60), and that the small chambered (or microspheric) individuals reproduce themselves by means of amoebulæ expelled from the shells producing the megalospheric young, whilst the large chambered (Sl. 61) (or megalospheric) individuals discharge flagellispores which conjugate with the flagellispores of other individuals and producing microspheric young, recommence or continue the life-cycle.

Within the last few months, owing to the initiative and manipulative skill of Mr. J. E. Barnard, a new and very striking method of investigation has been instituted which not only reveals the internal structure of Foraminifera without the need of cutting sections, and so destroying the specimens, but may have far-reaching results when applied to the study of the living protoplasmic bodies. This is the application of the X-rays to the shells, the results of which operation are highly interesting and significant.[5] Here is a very thick and opaque species, *Biloculina bulloides* (Sl. 62). The application of the X-rays (Sl. 63) reveals the internal arrangement of the chambers clearly. Here again is the coarse tropical calcareous form *Operculina complanata* (Sl. 64), the whole of the interior septation of which (Sl. 65) is perfectly shown by the skiagraph. The process is especially valuable in connection with the arenaceous forms. Here (Sl. 66) is one of the most rugose species, and one which is exceedingly difficult to sectionize, owing to the sand-grains imbedded in the calcareous cement of which it is formed, *Cyclammina cancellata.* The skiagraph (Sl. 67) not only reveals its intricate labyrinthic interior, but here, as in the other species, reveals the fact that the individual is of the megalospheric stage of the life-cycle. The method is invaluable for the determination of doubtful species. The two species (Sl. 68), *Jaculella obtusa* and *Botellina labyrinthica,* are externally very difficult to distinguish, but the skiagraph (Sl. 69) reveals the simple tubular cavity of the former as contrasted with the labyrinthic interior of the latter, and so determines the identity of the organisms.

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[1] E. Heron-Allen, Contributions towards the Study of the Bionomics of the Foraminifera, Phil. Trans. Roy. Soc. (Lond.) 1915. (*In the Press.*)

[2] J. J. Lister, Contributions to the Life-History of the Foraminifera, Phil. Trans. Roy. Soc. (Lond.), vol. 186, B. pp. 401-453. 1895.

[3] F. Schaudinn, Die Fortpflanzung der Foraminiferen, Wiegmann's Archiv. für Naturgeschichte, Jahrg. xlix. pp. 428-454.

[4] F. Winter, Zur Kenntniss der Thalamophoren, Archiv. für Protistenkunde, vol. vii. 1907.

[5] J. E. Barnard, X-rays in Relation to Microscopy, Journ. R. Micr. Soc. p. 1. London, 1915.

You may rightly ask yourselves in what, beyond the beauty of the shells, consists the interest and value of the elaborate and concentrated study to which the Foraminifera have been subjected. Their value is both scientific and economic. They are the largest of the unicellular organisms—which are the closest to the beginnings of life—and if ever the structure and nature of protoplasm is to be determined, it is, in my opinion, by the study of the Foraminifera that this conclusion will be arrived at. Economically they form the food of worms, starfishes, and many of the lower invertebrata, which in turn feed the food-fishes of the world.

I come now to the concluding and most important section of my discourse, which concerns itself with the phenomena of purpose and intelligence which I have claimed to be exhibited by some of the Foraminifera in the construction of their shells, a claim which has been, and is, denied by several very distinguished zoologists, and admitted, with some reservations, by others no less distinguished.[1] There are limits to what is known, but I refuse to admit that there are limits to what is knowable. As Prof. MacBride has justly observed: "To put forward an unknown entity as the cause of phenomena which we cannot unravel is not to explain, but in reality to give up the attempt at explanation."[2]

The method in which the arenaceous Foraminifera collect and adjust the materials from which they build their marvellous shells is obscure, and though a light begins to dawn upon the process it would take too long to go into the matter on this occasion. Surface tension no doubt plays an important part in the operation, but surface tension will not account for the mysterious fact that certain species such as, to take a single instance (Sl. 70) especially, *Haplophragmium agglutinans,* incorporate into their shells fragments of heavy gem minerals such as magnetite, garnet, and topaz, which are not by reason of their specific gravity to be found in the same sand-strata as the relatively light quartz-grains which are mainly used in the construction of the shell. The common *Verneuilina polystropha* (Sl. 71) of our shores exhibits this phenomenon also to a remarkable degree. It is, however, the intelligence (and I use this word with a full sense of the responsibility which I incur in using it) displayed in the manipulation of the material which compels the attention of the biologist. We are all familiar with the beautifully built tubes of the Caddis worm (Sl. 72), and some of the marine worms build tubes of no less remarkable ingenuity, as, for instance (Sl. 73), *Amphictene*, and one local variety of this worm constructs its tube (Sl. 74) as neatly as a bricklayer building a wall out of fragments of sponge-spicules of a carefully selected size. But these are Metazoa, higher animals, endowed with organs and senses. The Foraminifera, I must repeat for emphasis, are unicellular creatures without any differentiated organs or even structure of any kind whatever.

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[1] E. Heron-Allen, On Purpose and Intelligence in the Foraminifera, Proc. Zool. Soc., p. 1069. London, 1914.

[2] E. W. MacBride, in Nature, vol. 94, p. 304. Nov. 19, 1914.

Take the common arenaceous form (Sl. 75), *Psammosphaera fusca*, which builds itself into a roughly agglutinated house of sand grains. There is no selection here. There is none in the variety *P. testacea* (Sl. 76), which uses only the shells of dead and living Foraminifera—it uses them because it has nothing else to use ; but *P. parva* (Sl. 77), finding itself by its small size and free habit liable to suffocation in the ooze on which it lives, builds its house round a catamaran spar formed of a long sponge-spicule, which buoys it up upon the mud surface. Another species, *P. rustica* (Sl. 78), builds in the spaces of a tent-pole arrangement of such spicules—several individuals (Sl. 79) frequently combining to form a mutually supporting mass. This creaturè fills in the triangular spaces between the main tent-poles with broken spicules of successively graduated lengths, and when it arrives at an awkward terminal space finds and incorporates a truncated triaxial sponge-spicule (Sl. 80) to fill in the angle.

It is when we come to the devices employed by the Foraminifera for their protection from living foes, or the forces of nature, that their purposive intelligence becomes the most phenomenal. Many of the larger and doubtless more succulent forms are peculiarly liable to attack from parasitic worms—an elaborate study of which has been made by Prof. Rhumbler.[1] A striking instance of this occurs in the case of *Crithionina pisum* (Sl. 81), which has a softly agglutinated shell, which is often found (as in one of the specimens exhibited) bored by worms. Certain individuals (Sl. 82) have arrived at protecting themselves with a *chevaux de frise* of sponge-spicules, and these we never find, so far as our experience goes, suffering from these attacks. *Haliphysema ramulosa* (Sl. 83) is another easily attacked species, and it protects its aperture with a similar defensive apparatus. The same protective investment is assumed by (Sl. 84) *Hyperammina ramosa*, a species which ramifies in a most remarkable manner, so much so that Mr. Earland once constructed for me a Christmas greeting slide (Sl. 85) out of its many vagariant forms. (The "selection" in this case is rather that of Mr. Earland than of the Rhizopod.)

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[1] L. Rhumbler, Beiträge zur Kenntniss der Rhizopoden, Zeitschr. Wiss. Zool., vol. lvii. p. 589. 1894.

The genus Marsipella, of which the most familiar form is *M. cylindrica* (Sl. 86), is built up of sponge spicules set parallel to its axis, and is excessively friable, perfect specimens being very seldom found. It consists of a simple tube affording an easy prey to parasitic worms. It has consequently learnt to protect itself with a crown of spines (Sl. 87), which keep out these intruders. But some individuals, to which we have given the specific name *M. spiralis* (Sl. 88), have made the same discovery as did the prehistoric genius who invented string (Sl. 89). They increase their power of resistance to shock by twisting their spicules into a left-handed spiral (Sl. 90), by which means their power of resistance is enormously increased.[1]

But probably the zenith of purpose and intelligence is reached by the genus Technitella, a genus named by Canon Norman—the Little Workman—with good cause. The most familiar species, *T. legumen* (Sl. 91), builds its shell apparently of sponge spicules set parallel to its axis. The accidental smashing of a specimen, however, revealed to us the fact that only the outer layer of spicules is thus disposed (Sl. 92). The inner layer is set at right angles to the outer, thus producing the nearest approach to the woof and warp of a textile fabric possible in so rigid a material as sponge-spicules. This is clearly seen (Sl. 93) in a highly magnified fragment of a broken shell. The genus reaches its highest development of purposive selection, however, in our species *T. thompsoni* (Sl. 94), which, out of the vast and heterogeneous mass of material at its disposition, selects only the anchor-plates of a particular kind of Echinoderm, which it cements together at their edges with an invisible cement, and thus constructs what is certainly one of the most decorative, and certainly the most highly perforated shell in existence.[2]

In the presence of the phenomena which I have exhibited before you this evening there are zoologists who aver that there is no such thing as purpose or intelligence to be postulated as a motive for the behaviour, not only of the Protozoa, but even of much higher orders of animal life. Jules Fabre, who has by consent assumed the purple among the historiographers of the Insect World, denies intelligence even to the Digger Wasps, and to the Termites of Ceylon—at the end of all his amazing observations he says, "*Ils ne savent rien de rien.*" I prefer to go back exactly half a century to when Philip Henry Gosse, F.R.S., than whom no keener observer of marine organisms ever lived, said, "The more I study the lower animals, the more firmly am I persuaded of the existence in them of psychical faculties, such as consciousness, intelligence, and choice, and *that* even in those forms in which as yet no nervous centres have been detected."[1] A distinguished critic, Dr. Chalmers Mitchell, tells me that I might as well claim intelligence and purpose for such plants as the Tragopogons, whose seeds are fitted with a parachute, which enables them to travel to new pastures. I almost wonder that I am not accused of agreement with the whimsical suggestion of Samuel Butler, who looked forward to the day when we should see little engines playing about the doors of the engine sheds, whilst the parent engine smoked peacefully inside. I refuse to admit that the seed parallel has any bearing upon the case. I am dealing with the utilization of independent materials collected by the Foraminifera for a specific purpose. In the case of the seeds it is a development of a useful integral part and a consequent "survival of the fittest"—but if a bean in the kitchen-garden were to attach to itself the parachute of a Tragopogon and fly over the wall when in danger of its life at the hands of the cook, *that* would be an exercise of purpose and intelligence comparable with the phenomena which I have exhibited this evening.

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[1] E. Heron-Allen and A. Earland, On some New Astrorhizidae and their Shell Structure, Journ. R. Micr. Soc., p. 382. 1912.

[2] E. Heron-Allen and A. Earland, On a new species of Technitella from the North Sea, Journ. Quekett Micr. Club, ser. 2, vol. x. p. 403. 1909.

An evolutionary cycle is *ex hypothesi* continuous, and I refuse to allow a consistent evolutionist to postulate a discontinuity in his evolutionary cycle—he cannot at some unknown point introduce into his bioplasm an outside and novel influence to which he gives the name of "Intelligence." I claim that every living organism living an independent existence of its own is endowed with the measure of *intelligence* requisite to its individual needs.

We must accumulate facts, we must assimilate phenomena, we must strive after a comprehension of motive forces. To quote Prof. MacBride once more: "The use of hypotheses which assist in binding together the facts observed in the behaviour of living things, and in elucidating the laws which govern them . . . . may be regarded as neither vitalistic nor mechanistic, but as plain common-sense applications of the indicative method. In this way only it seems to me we shall ever make progress with "explanations" of the phenomena of life, for all "explanation" in the last resort consists merely in putting together similar things."[2]

But to arrive at a conclusion we must study the life-history of these lowly organisms, which, as Prof. Verworn has said, seem to be especially provided for the biologists, since of all living creatures they are nearest to the origin of life. We must not merely collect and classify them like postage stamps. The study of the Foraminifera has been grievously afflicted with a tendency to lie upon a platform between two points, on one of which sits the invalided Scientist who, forbidden by his doctor to work, has bought a pound of the comfit known to our youthful taste as "Hundreds and thousands," and who employs his time sorting out the red, the white, and the blue, setting aside as new species those globules which have been damaged in the process of manufacture—and on the other of which sits the Grammarian, whose sole regret upon his deathbed was that he had not devoted the whole of his life to the Dative case.

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[1] P. H. Gosse, A Year at the Shore, p. 247. London, 1865.

[2] E. W. MacBride, in Nature, vol. 94, p. 304. Nov. 19, 1914.

List of Slides shown.

1. A Nummulite. (Nummulites gizehensis—*Ehrenberg*).
2. Ditto. Section.
3. Section of Nummulitic Limestone.
4. Nummulites cumingii—*Carpenter.*
5. Alveolina Limestone. Mixon Reef, Selsey.
6. Alveolina boscii—*Defrance.*
7. Spirillina groomii—*Chapman.*
8. Globigerina ooze. N. Atlantic.
9. The same, more highly magnified.
10. Chalk Foraminifera. (Upper chalk), Herts.
11. Dog's Bay, Connemara.
12. Dog's Bay Foraminifera.
13. Emlagh Point.
14. Mediterranean Foraminifera.
15. Hooke's Rotalian Shell. 1665.
16. Leeuwenhoek's *Polystomella.* 1702.
17. Kerimba Foraminifera.
18. Pavonina flabelliformis—*d'Orbigny.* (Opaque.)
19. The same by transmitted light.
20. Calcareous Foraminifera.
21. Arenaceous Foraminifera.
22. Carterina spiculotesta—*Carter.*
23. The same, basal view.
24. Isomorphism of Cornuspira, Ammodiscus, and Spirillina.
25. Nubecularia lucifuga—*Defrance.*
26. Biloculina serrata—*Bailey.*
27. Spiroloculina nitida—*d'Orbigny.*
28. Miliolina seminulum—(*d'Orbigny*).
29. Miliolina bertheliniana—*Brady.*
30. Massilina secans—(*d'Orbigny*).
31. Cornuspira foliacea—(*Philippi*).
32. Saccammina sphaerica—*M. Sars.*
33. Haplophragmium crassimargo—*Norman.*
34. Haplophragmium pseudospirale—*Williamson.*
35. Textularia agglutinans—*d'Orbigny.*
36. Webbina clavata—*Jones and Parker.*
37. Reophax scottii—*Chaster.*
38. Bulimina aculeata—*d'Orbigny.*
39. Bulimina subteres—*Brady.*
40. Group of Lagenidae.
41. Nodosaria filiformis—*d'Orbigny.*

42. Vaginulina margaritifera—(*Batsch*).
43. Frondicularia alata—*d'Orbigny.*
44. Cristellaria costata—(*Fichtel and Moll*).
45. Cristellaria tricarinella—*Reuss.*
46. Polymorphina regina—*Brady, Parker and Jones.*
47. Polymorphina rotundata—*d'Orbigny*—fistulose and adherent.
48. Globigerina cretacea—*d'Orbigny.*
49. Orbulina universa—*d'Orbigny*—with contained Globigerina.
50. Discorbina vesicularis—(*Lamarck*).
51. Pulvinulina pauperata—*Parker and Jones.*
52. Polystomella crispa—(*Linné*).
53. Polystomella macella—(*Fichtel and Moll*).
54. Gromia oviformis—*Dujardin*—newly emerged from mud.
55. The same, half an hour later.
56. The same, after climbing up alga stems.
57. The same, lost among algae and attracted into light circle.
58. Miliolina durrandii—*Millett*—with ingested Foraminifera, etc.
59. Nummulites laevigata—*Lamarck*—megalospheric.
60. The same, microsphaeric.
61. Polystomella crispa—*Linné.* Micro- and megalo-spheric decalcified bodies (after Lister).
62. Biloculina bulloides—*d'Orbigny.*
63. The same. Skiagraph.
64. Operculina complanata, *var.* granulosa—*Leymerie.*
65. The same. Skiagraph.
66. Cyclammina cancellata—*Brady.*
67. The same. Skiagraph.
68. Jaculella obtusa—*Brady*; and Botellina labyrinthica—*Brady.*
69. The same. Skiagraph.
70. Haplophragmium agglutinans—(*d'Orbigny*)—with gems and magnetite.
71. Verneuilina polystropha—*Reuss*—with topaz and garnet.
72. Tubes of the Caddis-worm.
73. Amphictene auricoma—normal.
74. The same, built of sponge-spicules.
75. Psammosphaera fusca—*Schulze.*
76. Psammosphaera fusca, *var.* testacea—*Flint.*
77. Psammosphaera parva—*Flint.*
78. Psammosphaera rustica—*Heron-Allen and Earland.*
79. The same. Associated specimens.
80. The same. Detail of construction.
81. Crithionina pisum—*Goës.*
82. Ditto, *var.* hispida—*Flint.*
83. Haliphysema ramulosa—*Bowerbank.*
84. Hyperammina ramosa—*Brady.*
85. The same. Arranged as a Christmas Greeting.
86. Marsipella cylindrica—*Brady.*
87. The same. Aperture protected by sponge-spicules.
88. Marsipella spiralis—*Heron-Allen and Earland.*
89. The same, by transmitted light.
90. The same. Detail of construction.
91. Technitella legumen—*Norman.*
92. The same. Diagrammatic.
93. The same. Detail of construction.
94. Technitella thompsoni—*Heron-Allen and Earland.*

[E. H.-A.]

WEEKLY EVENING MEETING,

Friday, May 28, 1915.

J. H. Balfour Browne, K.C. D.L. LL.D., Vice-President,
in the Chair.

Sir John Jackson, C.V.O. M.P. LL.D. F.R.S.E. *M.R.I.*

Engineering Problems of Mesopotamia and Euphrates Valley.

The area which I propose to cover within this title may be taken as extending from the head of the Persian Gulf north-west to Mosul on the Tigris, and to Hit, up the Euphrates, more to the west. These lands bounded by the two rivers, Tigris and Euphrates, are now commonly regarded as the site of the Garden of Eden, and just as they were made so productive by the energy of men who lived prior to the time of the Babylonians, so, to-day, by similar agencies, they can be made one of the very best districts for raising cotton, cereals, and other products—a garden of the world indeed. The ruins of the ancient city of Babylon are now seen about fifteen miles to the south of the new Hindia Barrage across the Euphrates, which was completed by my firm some eighteen months ago. The remains of ancient canal irrigation works are to be seen to-day, and wherever water can be put on the soil even by the primitive methods at present used everything is fruitful and luxuriant. Without water the land remains desert. It is said that at present some ten million palm trees on ground easily irrigated fringe the Shat-el-Arab, supplying a date trade alone of a value of about half a million sterling per annum.

For hundreds of years the whole of this country in the hands of the Turks has been worse than misgoverned, and from my personal knowledge the feeling, not only of the people of Arab blood, but a large proportion of the more enlightened Ottoman subjects, have expressed themselves only anxious that the whole country should be brought under the influence and control of Great Britain. In considering this question, it is to be hoped that neither the British Government nor the British public will take narrow views, or be deterred by the prospect of larger responsibilities from dealing with a question of such wide interest and vast importance as the development of this country, and I venture to suggest that in its government the administration by a distinct service of officials from that of India may be advisable. Now, in view of, as we all hope, an early termination of the war, and the taking over of this country with so vast a scope for development, let us consider what particularly are the main and most urgent engineering problems to be dealt with. Our special interests in Persia and Mesopotamia require that the approaches by sea should always be kept open. Much, therefore, should be done in the way of improving the lighting and the navigation of the Persian Gulf, a most dangerous sea, particularly at its south-eastern end, where it is full of small islands only a little above the water-level, and sunken rocks, in many cases, only a few feet below the water-level. When I made my voyage from Bombay *via* Muscat to Mohammerah, I was surprised that in a sea over which, as I understood, our country was supposed to exercise control, I should not have seen a lighthouse between Muscat and the head of the Gulf. At the head of the Gulf, leaving Koweit on the west and entering the Shat-el-Arab, we were impeded by a great sand-bar at the entrance to that river. The little steamer of the British India Steam Navigation Company in which we travelled, drawing only some sixteen to eighteen feet, so far as I remember, could not cross this sand-bar all afloat, but having made one attempt had to go astern and practically ram its way through the sand for probably two or three feet in depth. Large steamers very often have to lay exposed outside the bar to transfer their cargoes to small craft for delivery at Busra. It appears to me that one of the first problems to be dealt with is the removal of this obstruction to the navigation of vessels of even moderate size. Some engineers have proposed to rely upon a dredging away of this bar, but I am inclined to think that this, without a proper system of training walls to somewhat curtail the width of the stream and increase its scour, would not be altogether satisfactory. As an alternative, many are of opinion that the better plan would be to carry a railway to Koweit, and make there a new harbour with much easier passage to deep water than over the bar at the mouth of the Shat-el-Arab, and in this opinion I agree. The river between Fao, at the entrance to the Shat-el-Arab, and Gorna, at the junction of the Euphrates and the Tigris, could easily be made a good channel. At the old city of Mohammerah, the headquarters of the great Sheik of Mohammerah, strikes off the River Karun, with a more or less good channel for river boats of not more than three feet draft, up to Ahwaz, an old Persian town from whence start the main caravan routes into Persia. Here most of the trade is in British hands, and it is upon this river that the largely increasing business of the Anglo-Persian Oil Company has to rely for water transport. The Anglo-Persian Oil Company is a purely British organization in which last year, in view of the great supplies of oil necessary for the purposes of the Navy, the British Government acquired an interest to the extent of about two million sterling.

At Mohammerah, where the members of my party were guests of Captain Wilson, the then British Vice-Consul, a man of exceptional capabilities for such a position, I met the great Sheik, of whom I formed a very high opinion, particularly having regard to his undoubted pro-English sympathies, and desire to see his country progress.

At Mohammerah ample provision for the transfer of goods coming down stream in river boats to deep-sea steamers should be made, and at Busra, occupying as it does a most important position, with, I believe, an annual trade which has already reached a value of some six million sterling, unless a new harbour is made at Koweit, proper port works with ample warehouses and transit sheds should be provided whether for trans-shipment to ocean steamers from the shallow-draft craft coming down the Tigris and Euphrates, or for goods coming over the proposed railway from Bagdad. Going on from Busra to Gorna by the Euphrates to the left or by the Tigris to the right, we immediately get into shallow water. I have not personally been along any *great* length of the Euphrates, but, from information at hand, it must be considered a much more awkward river to navigate than the Tigris, passing Amara up to Bagdad, a distance of some 350 miles. As far as I can judge from my voyage to Bagdad along the Tigris in a very shallow paddle-wheel boat, although we went aground thirteen times, I have no doubt at comparatively small expenditure the navigation of this river, at any rate as far as Bagdad, could be made quite easy for vessels of moderate draft; but in view of so vast a quantity of water under the proposed irrigation schemes having to be taken from the Euphrates and Tigris, it becomes a question whether, with a country without any extraordinary difficulties for railway making, it would not be better for transport to rely upon railways, more or less neglecting the rivers. It would be a great advantage if the railways were constructed either before or simultaneously with the proposed irrigation works, so as to afford a better means of transport for materials for the latter, and be an immediate solution of the problem which must arise as soon as the irrigation works are in operation, namely, how best to deal quickly and cheaply with the transport of cereals, fruits, and other produce.

At Bagdad you have a great ancient city with a magnificent river front, full of merchants of all nationalities, many of considerable wealth, a great centre for trade between Persia and the west. From Bagdad I and my party crossed the desert along the regular route for pilgrims journeying to the ancient shrine of Kherbela, where the remains of two grandsons of the Prophet are laid, to the little town of Museyib on the Euphrates. I was surprised at the number of pilgrims seen every day, and, while in Bagdad, I made a proposal to accept a concession for making a railway across this piece of desert, the granting of which I think was only prevented by arrangements that had been made between the Germans and the Turkish Government, which prohibited the granting of a concession for such a line except to Germans.

Kherbela is a sacred city which practically no Christians are allowed to visit, at any rate not the mosques, but Mr. Warbrick and some few others of our staff were there in connexion with the cleaning out of the Kherbela Canal, which had silted up, and the photos you will see of Kherbela were taken by Warbrick. We always, however, kept to the front Mohammedan engineers and supervisors.

I must tell you a little story of my journey from Museyib to the site of the old barrage. Coming from Bagdad, it getting dark, my son-in-law and daughter, who were with me, went on with an escort of two soldiers direct to Babylon, where we were the guests of the German explorers; and the others of our party, Mr. Eady, Fuad Bey, and myself proceeded to Museyib. The night was pitch dark—I never remember a darker one—and about eight o'clock we arrived at Museyib, when our two Turkish soldiers declined to go any further, under the excuse that the river was too dark, so I decided that if we could get two Arab boatmen, the three of us would venture down the river without any escort. We had been told that going down the river from Museyib we were safe to go with the stream until we saw a red light in the office of Sir William Willcocks' assistant, but they had not told us that this office was situated below the falls of the old barrage. On we went for a long time without seeing a light or yet the banks of the river. I then insisted that we should get near the one shore, which we did and shortly afterwards saw the light. Then, feeling safer, on we went, but it was fortunate that we were within a few feet of the one bank, for we shortly heard a fall of water. I reached out and gripped the bank, to find the following morning that we had been within about eighty feet of the falls of the Euphrates over the old barrage. I need hardly say our language used was pretty strong when we found we had not been warned that the light was below the barrage and not above it.

I will now refer generally to the great scheme of irrigation work got out for the Turkish Government by Sir William Willcocks, so well known in connexion with the early stages of the Nile irrigation proposals, as well as works in India. Sir William's scheme was most comprehensive, and involved an estimated expenditure of some eighteen to twenty million sterling, but as at that time the financial position in Turkey would not permit such large responsibilities being undertaken, it was considered the proper plan to only proceed in the first instance bit by bit with the construction of the most urgent works. It was in the year 1907 that Sir William was called in to report, and at the end of 1910 he made his report. At the time of my visit to Bagdad Sir William Willcocks with a small staff had already made a start with preliminary works in connexion with the first section of his scheme to be taken in hand—the erection of the barrage at Hindia—Sir William being assisted at the site of the work by a young engineer, Mr. Tabor. My firm having been invited by the Turkish Government to make proposals for the further carrying out of these works, returning from South Africa by Aden, accompanied by a son-in-law and my daughter Gertrude, I had proceeded to Bagdad to meet the late Nazim Pasha, the then Vali, with whom I made a contract for construction. This contract being agreed, we took the work over from Sir William Willcocks in the month of February, 1911, and the Hindia Barrage was completed by us and inaugurated by the succeeding Vali of Bagdad on the 12th day of December, 1913, well within the estimated time.

We also cleared and deepened about 65 miles of the Hilla Canal, and carried out other works at Faluja. At Habbania we commenced the construction of the Escape Works, by which through a canal cut from the Euphrates to a great depression in the ground, water can be held up and reserved, and in the dry months passed into the river again to increase its flow at a point near Faluja.

In passing I cannot help referring to the pleasant relations I had with, and of the high opinion I entertained of, Nazim Pasha, whom I found so intelligent, so fair dealing and honourable a man. It was indeed a sad day for Turkey when he was murdered in Constantinople to make room for men of a very different stamp.

Throughout the district in which we worked we found the Arab Sheiks generally favourable towards us English, while it was common knowledge that as a rule the Germans working in connexion with the Bagdad Railway were in anything but favour with the natives. It was in the year 1899 that the Germans launched the Imperial Ottoman Bagdad Railway Company for the construction of a line to link up the Anatolian Railway at Konia with Bagdad and the Persian Gulf. This important railway coming on from Constantinople it is understood has been completed by the Germans to the western side of the Taurus Mountains, and a length of some 80 miles between Bagdad and Sammarra recently opened for traffic. Once we have peace, and the Germans are out of control in Turkey, this railway should be completed across the Taurus Mountains, on to Bagdad and thence to Busra, at any rate, if not further on to Koweit. With the railway completed, and a direct line of only some 450 miles in length from Bagdad through Damascus to Beyrout, a huge trade would be opened out for the whole of this Mesopotamia district, and through Busra to the Persian Gulf and the East.

The total area to be irrigated and put under cultivation proposed in Sir William Willcocks' scheme is estimated to cover not less than 1,400,000 hectares, or say three and a half million acres, and there can be little doubt that any outlay on these irrigation works will be most amply repaid, as in the case of the great works of the Nile Valley and the Chenat Valley of India. Properly irrigated, this Mesopotamia district should become one of the largest and best granaries of the world.

According to the laws of Turkey I understand it is still almost impossible for Christians or non-Turkish subjects to hold land, but under a new and more enlightened government, with reasonable inducements to capitalists with regard to the ownership of land and otherwise, I am of opinion that this great Mesopotamia district should quickly develop and become, as it was in ancient days, one of the busiest and most productive parts of God's earth, with the further advantage of being able to give an outlet for the best of the surplus population of India, if necessary.

As it may be of special interest to you, I am having put on the screen some pictures of our work in connexion with the Hindia Barrage, other works we have so far done, and incidents of local travel, etc., and, if time permit, I shall give you a brief description of them, though in themselves they are mostly not problems for the future, but realities of the present.

In times long ago it had been by carefully devised systems of barrage and canals that such good results had been obtained from the land, but by neglect these works had become defective and the canals silted up.

As I have said, owing to the state of finances in Turkey, it was decided not to proceed immediately with Sir William Willcocks' scheme in its entirety, but to take it bit by bit, first of all taking in hand the great barrage across the Euphrates at Hindia in order to restore the supply of water to the Hilla Channel. This channel serves a large area of land, and passing near the ruins of the ancient city of Babylon, runs to Diwania, irrigating a district of immense agricultural value.

It was, as I have already said, upon my visit to Bagdad in February, 1911, that these and other works were put into our hands to carry out.

Having regard to the extreme difficulties of labour and transport, we considered the time estimated for completion of the Hindia Barrage a short one, but I am pleased to say that notwithstanding all the difficulties we had to contend with, and particularly with regard to the financial difficulty caused by the late Balkan war, we got through our work well within the estimated time. As an instance of the difficulty that had been experienced in getting the Arabs to work on the right lines I may tell you another story, told to me by Mr. Tabor, the Assistant Engineer. The Arab in the usual way digs the earth with a kind of grocer's ladle. He then puts it into a bag on his left shoulder, walks away with it and tips it into a bank. When I was there I ordered some planks and wheelbarrows to be sent out with a view to getting the Arabs to use them. Tabor having called the attention of an Arab workman to the effect that he could make much better progress by using the planks and barrows, the Arab raising himself erect in a most dignified way addressed Tabor as follows: "You people from the west, why, a thousand years ago no one knew you; my people have been here since before the time of Moses. Are you going to teach me how to carry earth." Now, however, as you will see by the pictures, planks and barrows are used by the natives. We also had a special difficulty to contend with on account of the absence of good fuel of any description at or near the site of the works. In two and three-quarter years, however, we completed our task, the barrage was put across the new diversion of the Euphrates, the original channel of the river dammed, the water turned into the diversion, and head regulators built for the Hilla branch. By reference to the plan it will be seen that no portion of the new barrage was built in the original bed of the river. On account of the importance of careful examination of foundations, we decided to have the dam wholly constructed in the dry, and only when it was entirely completed were the banks removed and the water of the old Euphrates river allowed to flow in its new course. The erection of the dam across the old river was a separate affair, to which I will refer later. The work of the barrage across the diverted river was built mainly of brickwork, with a certain amount of cement concrete. The bricks were manufactured on the site, and on the whole were very good. The lime came down from the old quarries of Hit (some say the Hit of the Bible, in the same neighbourhood from which no doubt the pitch referred to in the building of Noah's Ark also came). The barrage has thirty-six openings, each sixteen and a half feet wide, and there are two sluice-gates in each opening, each gate being 16 ft. 6 in. wide by 8 ft. in height. At one side of the barrage a lock has been built for the use of river traffic of a width of about 26 ft. and a length of about 325 ft., with a pair of gates in the middle dividing it into two parts, each of which is 162 ft. in length. The Hilla head regulator is very similar in construction to the main barrage, but it has only six openings, each 9 ft. 10 in. wide. The main barrage raises the ordinary summer level of the river some 16 ft., allowing ample supply of water to flow into the Hilla Channel. The construction of the dam formed across the old channel of the river to which I have referred was a little peculiar, being built according to a system which for many years had more or less been adopted by the Arabs. We made a series of long sausages (as they were termed) of brushwood, brought from many miles up-stream. These were constructed on the river bank and rolled into the water, floated into position, and sunk by weights to the bed of the river.

When I paid my visit to Bagdad, my chief assistant who accompanied me was Mr. Griffin Eady, M.Inst.C.E., who later took up the position of our chief engineer and representative at Constantinople. Since the commencement of the construction of the works, our chief engineer and representative in Mesopotamia, having entire control of the whole of the works, has been Mr. Arthur Noel Whitley, chiefly assisted by Mr. Percy Warbrick, A.M.Inst.C.E., the chief of the commercial department, under Mr. Whitley, being Mr. William Hill. The Turkish Government had a special engineer of European education, Monsieur Edmund Bechara, to make reports to them, whom we found a man of very good ability and character.

It has been a great disappointment to me that when our organization had become so satisfactory, and when we had finished this first barrage, that owing to the war our works should have been suspended and the whole of our valuable English staff detained by the Turks at Bagdad as prisoners of war.

[J. J.]

WEEKLY EVENING MEETING,

Friday, June 4, 1915.

Charles Hawksley, Esq., M.Inst.C.E., Vice-President,
in the Chair.

Professor Sir Ernest Rutherford, LL.D. D.Sc. F.R.S. *M.R.I.*
(Nobel Laureate).

Radiations from Exploding Atoms.

It is now well established that the radio-active substances are undergoing spontaneous transformation, and that their characteristic radiations—the α, β, and γ rays—accompany the actual disintegration of the atoms. The transformation of each atom results from an atomic explosion of an exceedingly violent character, and in general results in a liberation of energy many million times greater than from an equal mass of matter in the most vigorous chemical reaction.

In the majority of cases the atomic explosion is accompanied by the expulsion of an actual atom of matter—an α particle—with a very high speed. It is known that the α particle is an atom of helium which carries two unit positive charges, and which leaves the atom with a velocity of about 10,000 miles per second. In some transformations no α particle is ejected, but its place is taken by a swift β particle or electron. These β rays carry with them a large amount of energy, for in some cases they are expelled very close to the velocity of light, which is the limiting velocity possible for such particles. The expulsion of high-speed β particles is usually accompanied by the appearance of γ rays, which correspond to X-rays, only of greater penetrating power than has so far been obtained from an X-ray tube even when a high voltage is employed. The emission of energy in the form of γ rays is not negligible, for in some cases it is even greater than the energy emitted in the form of high-speed β particles, and may amount per atom to as much as 20 per cent. of the energy released in the form of a swift α particle.

By the application of a high voltage to a vacuum tube it is quite possible to produce types of radiation analogous to those spontaneously arising from radium. For example, if helium were one of the residual gases in the tube, some of its atoms would become charged, and would be set into swift motion in the strong electric field. In order, however, to acquire a velocity equal to the velocity of expulsion of an α particle, say, from radium C, even in the most favourable case nearly four million volts would have to be applied to the tube.

In a similar way, in order to set an electron in motion with a velocity of 98 per cent. the velocity of light, at least two million volts would be necessary. As we have seen, it has not so far been found possible to produce X-rays from a vacuum tube as penetrating as the γ rays. The study of the radiations from radio-active substances is thus of especial interest, not only for the information obtained on the structure of the atoms themselves, but also in providing for investigation special types of radiation of greater individual intensity than can be obtained by ordinary experimental methods. The enormous energy of motion of swift α and β particles must exist in the atom before its disintegration, either in a potential or a kinetic form, and may arise either from the passage of the charged particles through the intense electric fields within the atom, or from the very swift motion of these particles within the atom before their release. In any case, there can be no doubt that electric fields, and possibly magnetic fields, of enormous intensity exist within the very small volume occupied by the essential structure of the atom—fields many million times greater in intensity than we can hope to produce in laboratory experiments.

In order to explain certain experimental results, I have suggested that the main mass of the atom is concentrated within a minute volume or nucleus, which has a positive charge, and is of dimensions exceedingly minute compared with the diameter of the atom. This charged nucleus is surrounded by a distribution of electrons which may extend to distances comparable with the diameter of the atom, as ordinarily understood. The general evidence indicates that the α and primary β particles are expelled from the nucleus, and not from the outer structure of the atom. If this be the case, the α particle which carries a positive charge would have its velocity increased in passing through the strong repulsive field surrounding the nucleus; on the other hand, the β particle which carries a negative charge must be retarded in its escape from the nucleus, and must possess great initial energy of motion to escape at all. There appears to be no doubt that the penetrating γ rays have their origin in some sort of disturbance in the rings of electrons nearest to the nucleus, but do not represent, as some have supposed, the vibrations of the nucleus itself.

α RAYS.

A brief account was given of the recent work of Rutherford and Robinson in determining with accuracy the velocity of expulsion of the α particles from certain radio-active substances. This was done by measuring the deflection of a pencil of α rays in strong magnetic and electric fields. With the aid of intense sources of radiation, it was found that the value of the E/M—the ratio of the charge carried by the α particle carried to its mass—was 4820 units, a value to be expected if helium has an atomic weight 4 and carries two unit charges. This experiment also shows that the mass of the flying positive particle is not affected appreciably by its swift motion. From known data the initial velocity of the expulsion of the α particles from all other radio-active substances can be deduced with accuracy.

If the expulsion of an α particle from an atom is the result of an internal explosion, we should anticipate, from the analogy of a shot from a gun, that the residual atom would recoil in a direction opposite to the escaping β particle. The existence of these "recoil" atoms can be shown in a variety of ways, for the velocity of recoil is sufficient to cause the atoms to leave the surface on which they are deposited and to pass through a considerable distance in air at a pressure of one millimetre before they are stopped. It is to be anticipated that the momentum of a recoiling atom should be equal and opposite to that of the escaping α particle. Since the deflection of a charged particle in motion in a magnetic field is inversely proportional to its momentum, the deflection of a stream of recoiling atoms should be the same as for the α particles if the atoms carry the same charge. Dr. Makower has examined the deflection of a pencil of recoil atoms in a magnetic field, and found it to be exactly half of that due to the α particle, proving definitely that the recoiling atom carries only one unit of positive charge in place of two for the α particle.

We thus see that the simple application of momentum enables us to deduce the mass and energy of the recoiling atoms. Since the mass of the radio-active atoms is about fifty times that of the α particle, the velocity, and also the energy, of recoil is only about one-fiftieth of that of the escaping α particle. In a similar way, it can be shown that the ejection of a swift β particle should cause a vigorous recoil of the atom, though not so marked as in the case of the more massive α particle.

β Rays.

During the last few years notable advances have been made in our knowledge of the mode of emission of β particles from radio-active atoms. The work of Baeyer, Hahn, and Meitner, and of Danysz, has shown that the β rays from a radio-active substance like radium B or radium C contain a number of definite groups of rays which are expelled with definite velocities. This is best shown photographically by examining the deflection of a pencil of β rays in a magnetic field. In a uniform field, each of the groups of rays describes a circular path the radius of which is inversely proportional to the momentum of the β particle. By the application of special methods it has been found possible to obtain a veritable spectrum of the β rays. The spectrum of the β rays from radium B and radium C has been very carefully examined by the writer and Mr. Robinson, and found to give a large number of well-marked bands, each of which represents a group of β rays, all of which are expelled with identical speed. It was at first thought that most of the energy of the β rays was comprised in these groups, as some of the bands on the photographic plate were very marked. Chadwick, however, has recently shown that the fraction of the rays which give a line spectrum is only a few per cent. of the total radiation. The general evidence shows that the β radiation from these substances gives a *continuous* spectrum due to β rays of all possible velocities, on which is superimposed a *line* spectrum due to a small number of β particles of definite velocity comprising each group.

Lines in the β-ray spectrum have been observed for particles which have a velocity not far from that of light, but the photographic effect of the particles becomes relatively feeble for such high speeds.

It is known from direct measurement that each atom of radium B or of radium C in its disintegration emits *on an average* one β particle. In the β-ray spectrum of radium C at least fifty definite bands are observed, differing widely in intensity. It is thus clear that a single atom in disintegrating cannot provide one β particle for *each* of these numerous groups. It is thus necessary to conclude that each atom does not emit an identical β radiation. The results are best explained by supposing that the β-ray spectrum is the statistical effect due to a large number of atoms, each of which may only give one or two of the groups in its disintegration. In this respect a β-ray transformation is distinguished from an α-ray transformation, for in the latter case each atom emits one α particle of characteristic speed. It will be seen later that there is undoubtedly a very close connection between the emission of β and γ rays from radio-active atoms, and the probable explanation of the remarkably complex β-ray spectrum will be discussed later.

With the exception of one element, radium E, and possibly uranium X, all the radio-active substances which emit primary β rays give a line spectrum. For the majority of elements the strong lines in the β-ray spectrum have been determined by Baeyer, Hahn, and Meitner, but more intense sources of radiation will be necessary to map accurately the weaker lines.

γ RAYS.

The earlier experiments on the γ rays were mainly confined to a determination of the absorption of the more penetrating radiations by different kinds of matter. It was early observed, however, that some of the radiations appeared to be complex. This was shown by anomalies in the initial part of the absorption curve. In the meantime, a notable advance in our knowledge of X-rays had been made by the work of Barkla. He found that under certain conditions each element when bombarded by X-rays of suitable penetrating power gave rise to a strong radiation which was characteristic for that element, *e.g.*, the lighter elements from aluminium to silver emitted characteristic radiations called the "K" series, which increased rapidly in penetrating power with the atomic weight of the radiator. It was found that the heavier elements emitted in addition another characteristic radiation of softer type, which was called the "L" series. These results showed clearly that there must be definite structures within the atom which gave rise to a definite radiation under suitable conditions of excitation. From these results it seemed probable that the γ rays from radio-active matter must consist of the characteristic radiations of these heavy elements, analogous in type to the corresponding radiations observed in ordinary elements when excited by X-rays or kathode rays. These conclusions were confirmed by a series of investigations made by Rutherford and Richardson. The γ rays were analysed by means of their absorption by aluminium and by lead, the disturbing effects of the primary β rays being eliminated by means of a strong magnetic field. It was found, for example, that the γ rays from radium B, when examined by their absorption in aluminium, consisted of at least two types, one easily absorbed, and the other eighty times more penetrating. By further observations of the absorption of the γ rays by lead, Richardson found that the rays from radium B could be divided into at least four definite types, each of which was absorbed exponentially by lead. Similar results were obtained for all the radio-active elements which emitted γ rays. In some cases the soft γ rays, *e.g.*, those from radium B, corresponded to the characteristic radiation of the "L" series, and others to the "K" series. The general results, however, indicated that several additional series of characteristic radiations are present in some cases. It was clear from these experiments that the γ rays corresponded to the natural modes of vibration of the inner structure of the radio-active atoms. In the meantime the experiments of W. H. Bragg and W. L. Bragg, and of Moseley and Darwin, had shown that the characteristic X-radiations of the elements gave definite and well-marked line spectra. These spectra were simply determined by reflecting the rays from crystals. If this were the case, it seemed probable that the γ rays from the radio-active atoms would also give line spectra, and thus allow the natural frequencies of vibration of these atoms to be determined. During the past year, a number of experiments have been made to test this point by Rutherford and Andrade, using radium B and radium C as the source of γ radiation.

As was anticipated, it was found that the γ rays from radium B and radium C gave well-marked line spectra. The general method employed was to use an α-ray tube containing a large quantity of emanation as a source of radiation. The γ rays were reflected from a crystal of rock-salt, and the position of the spectrum lines determined photographically. Usually twenty-four hours were necessary to obtain a marked photographic effect. Special difficulties arose in these experiments which are absent in an investigation of a similar kind with X-rays. In addition to γ rays, the radio-active matter emits very penetrating β rays which have a strong photographic action; while the γ rays in their passage through matter themselves give rise to high-speed β rays. The disturbing effect of these radiations has to be eliminated by placing the whole apparatus between the poles of a powerful electromagnet. In this way it was found that the spectrum of radium B consisted of a large number of lines, of which the most intense were deflected at angles of 1° 46′, 10°, and 12°. The more penetrating radiation from radium C gave a strong line of 1° and a fainter line at 43′. The strong lines at 10° and 12° are due to easily absorbed γ rays, and undoubtedly correspond to the "L" radiation of radium B. The line at 1° corresponds to a very penetrating radiation which has a wave-length less than one-tenth of an Angström unit. The penetrating γ rays from radium C have by far the shortest wave-length so far observed. It does not seem probable that such short waves can be produced artificially in an X-ray tube unless possibly an exceedingly high voltage be applied.

There is one interesting result of these investigations that should be mentioned. The two strong lines of the radium B spectrum deflected at 10° and 12° were found to correspond exactly in position to the X-ray spectrum of lead. These experiments thus confirmed the view based on chemical evidence that radium B and lead were isotopic, *i.e.*, they were elements of practically identical chemical and physical properties, although their atomic weight differed by seven units.

CONNECTION BETWEEN β AND γ RAYS.

Before considering in detail the difficult problem of the connection between β and γ rays, it is desirable to summarise the main facts that have been established in regard to the relations between kathode rays and X-rays:—

(1) A small part of the energy of kathode rays falling on a radiator is converted into X-rays, the average frequency of the latter increasing with the velocity of the kathode particle.

(2) X-rays in passing through matter give rise to a β radiation. The initial energy of the escape of the electrons increases with the frequency, and is probably proportional to it.

(3) Electrons or X-rays of appropriate energy are equally able to excite the characteristic radiations in an atom.

The results which have been shown to hold for the X-rays hold equally for the β and γ rays, which have much greater individual energies, *e.g.*, Gray and Richardson have shown that the β rays from radio-active matter are able to excite the characteristic radiations of the elements in a number of substances, while γ rays in passing through matter give rise to high-speed electrons. It was long ago suggested by Bragg that β rays and X-rays are mutually convertible forms of energy, *e.g.*, a β particle falling on matter may be converted into an X-ray of the same energy, and the latter in passing through matter may in turn be converted into an electron of identical energy. This assumes that the energy of an X-ray and an electron are mutually convertible, and the energy may appear under suitable conditions in either of the two forms. While the general evidence indicates that this point of view may hold closely for the conversion of the energy of a single X-ray into that of a swift electron, it is very doubtful whether it holds for the converse case of the excitation of an X-ray by an electron. We shall see later from experimental evidence that in general the energy of the electron required to excite an X-ray of definite frequency is always greater than the corresponding energy carried off in the form of an X-ray.

It was early observed that there appeared to be a close connection between the emission of β and γ rays from radio-active matter. In all cases, the two types of radiation appeared together. A closer examination, however, showed that there were very marked differences between the relative energies of the β and γ rays from different radio-active elements. For example, radium C emits intense β rays and also intense γ rays; on the other hand, radium E emits intense β rays over a wide range of velocity, but exceedingly weak γ rays. Differences of a similar kind were observed amongst a number of the radio-active elements. One striking distinction, however, was to be noted. All the radio-active substances which give a marked line spectrum of β rays also emitted intense γ rays. On the other hand, a substance like radium E, which gave scarcely any γ rays at all, gave a continuous spectrum of β rays in which no lines have so far been observed. It thus appeared probable that the line spectrum of the β rays was intimately connected with the emission of γ rays, and this conclusion has been completely established by recent experiments. As we have seen, γ rays in passing through matter give rise to high-speed β rays. Using radium B and radium C as a source of γ rays, the β radiation excited in a number of metals by the passage of γ rays was analysed in a magnetic field by Messrs. Robinson and Rawlinson and the writer, and was found to consist in part of definite groups of β rays. When lead was the absorbing material, the magnetic spectrum of the β rays excited by the γ rays was found to be nearly identical with the primary β-ray spectrum of radium B. This striking result shows that those β rays escaping from the radio-active atom which give rise to a line spectrum must result from the conversion of γ rays into β rays in the radio-active atom. The slight differences observed in the spectrum for different metals is probably connected with the energy required to excite one of the characteristic radiations of the element used as absorber.

An explanation of the marked differences in the character of the β and γ radiation from different radio-active atoms can, I think, be given on the following lines. Some of the γ rays are broken up in their escape from the atoms, and the energy of each converted γ ray is transferred to an electron which escapes with a definite velocity dependent on the frequency of the γ radiation. Taking into account a large collection of disintegrating atoms, each of the possible modes of characteristic vibration of the atom gives rise to an electron of definite speed. In this general way we may account for the line spectrum of the β rays which is so commonly observed. On this view, we should expect to obtain a well-marked line spectrum of β rays when a substance emits strong γ rays—a result in accord with observation.

In order to account for the marked differences in the types and intensity of γ rays from different radio-active substances, it seems necessary to suppose in addition that the primary β particle always escapes from the nucleus in a *fixed* direction with regard to the structure of the atoms under consideration. For example, we have already pointed out that radium E, although it emits intense β rays which give a continuous spectrum over a wide range of velocity, emits very weak γ rays. Since there can be no doubt that the β rays have sufficient speed to excite the characteristic modes of vibration which must be present in the atom, we are driven to the conclusion that the β particle escapes in such a direction that it does not pass through these vibrating centres. On this view, the type of characteristic γ rays which are excited, and consequently also the corresponding speed of the β rays which arise from the converted γ rays, will depend entirely on the direction of escape of the primary β particle. The definite direction of escape of the primary β particle, which varies for atoms of different substances, also suffices to explain a number of other differences observed in the mode of release of energy from various radio-active atoms. It is supported by many other observations which indicate that the atoms of a particular radio-active substance break up in an identical fashion.

We have so far considered only in a qualitative way the relation between the groups of rays in a β-ray spectrum and the emission of characteristic γ rays. During the last few years there has been a growing body of evidence that the energy E carried off in an X-ray of frequency ν is proportional to this frequency, and is given by $E = h\nu$ where h is Planck's fundamental constant. If the whole of the energy of an X-ray can be given directly to an electron, the energy communicated to the latter should be $h\nu$. There is no doubt that in many cases this simple relation holds very approximately, but the measurements so far available are not sufficiently precise to settle definitely whether a part of the energy may not appear in another form.

Assuming that the transfer of the energy from an X-ray to an electron is complete, we should expect to find groups of β rays of energy corresponding to $h\nu$ where ν is the frequency of the γ rays found experimentally. Such a relation is found to hold within the limit of experimental error for three marked groups of low-velocity β rays emitted from radium B. On the other hand, it is found that many of the high velocity groups of β rays both from radium B and radium C have energies many times greater than correspond to any observed frequency. Not the slightest evidence, however, has been obtained that the corresponding high frequencies of vibration exist in the radio-active atom; in fact, all the evidence points to the fact that these high-speed electrons arise from one or more of the observed frequencies in the γ-ray spectrum.

In order to account for such results, it seems necessary to suppose that the γ rays of high frequency are not necessarily emitted as single pulses, but consist of a train of pulses either produced simultaneously or following one another at very short intervals. Each of these pulses has an energy $h\nu$ corresponding to the frequency ν, but the total energy in the train of waves is $ph\nu$ where p is a whole number, which may have possible integral values 0, 1, 2, 3, . . . etc., depending on the structure of the atom and the conditions of excitation. The penetrating power of such a train of waves corresponds to that of a single wave of frequency ν, but on passing through matter the energy of the whole train of p waves occasionally may be transferred to an electron which consequently is expelled with an energy $ph\nu$. There is very strong evidence of the general correctness of this point of view, for most of the stronger lines in the β-ray spectrum of radium C have energies which correspond to an *integral* multiple of the energy corresponding to the strong lines actually observed in the γ-ray spectrum. It seems probable that under the ordinary conditions of excitation by kathode rays in a vacuum tube, the X-ray contains only one pulse or wave, but under the far more powerful stimulus of the very swift β particle escaping from the atom, a long train of waves, each of the same frequency, is produced. The energy of the whole train of waves may under suitable conditions be given to an electron, which consequently has a speed very much greater than that impressed upon it by a single wave of the same frequency.

LIMIT TO THE FREQUENCY OF VIBRATION OF THE ATOM.

There is one question of fundamental importance which arises in considering the modes of vibration of the atom, viz. whether there is a definite limit to the frequency of the radiation which can be excited in a given atom. Theory does not provide us with an answer to this problem, since little is known about the conditions of excitation, nor even of the nature of such high-frequency vibrations. A study of the frequency of the γ rays from radio-active substances is of great importance, as it throws much light on this problem.

As we have seen, the energy of the β particle escaping from the nucleus of radium C is equivalent to that acquired by an electron moving in an exhausted space under a potential difference of several million volts. This high-speed electron passes through the electronic distribution in its escape from the atom. Notwithstanding such ideal conditions for the excitation of high-frequency radiations of the atom, the highest frequency in the radiation emitted by radium C is only about twice that obtainable from an ordinary hard X-ray tube excited by 100,000 volts. It thus appears probable that there is a definite limit to the frequency of the radiation obtainable from a given atom, however high the speed of the disturbing electron. This limiting frequency is determined not by the speed of the electron but by the actual structure of the atom. Since the γ radiation from radium C gives a line spectrum, it would appear that the highest frequency obtainable is due to a definite system of electrons which is set into characteristic vibration by the escape of a β particle. In order to throw further light on this point, Prof. Barnes, Mr. H. Richardson and myself have recently made experiments to determine the maximum frequency obtainable from an X-ray tube for different constant voltages. The Coolidge tube, which has recently been put on the market, is ideal for this purpose, as it provides powerful radiation at any desired voltage. The anti-kathode is of tungsten of atomic weight 184, so that we are dealing in this case with the possible modes of vibration of a heavy atom. The maximum frequency of the radiation was deduced by measuring the absorption by aluminium of the most penetrating rays emitted at different voltages. The absorption of X-rays of different frequencies by aluminium has been examined over a very wide range, and can be expressed by simple formula. It was found that for 20,000 volts the frequency of the radiation was slightly lower than that to be expected if Planck's relation held. With increasing voltage there is a rapid departure from Planck's relation. The frequency reaches a maximum at about 145,000 volts, and no increase was observable up to the maximum voltage employed, viz. 175,000 volts. The experiments thus show that the frequency of radiation reaches a definite maximum, which is no doubt dependent on the atomic weight of the particular radiator employed. It is of interest to note that the maximum penetrating power of the X-rays from the Coolidge tube in aluminium is about the same as the γ rays from radium B, but is about three-tenths of the γ rays from radium C. There is evidence which suggests that the very penetrating γ rays from radium C correspond to the octave of the "K" characteristic radiation of that element. If this be the case, it may prove possible that a still more penetrating radiation might be obtained from tungsten, but in order to excite it a voltage of the order of a million volts would probably be required. In any case, it seems clear that Planck's relation does not hold for excitation of high frequencies by swift electrons, but may hold very approximately for lower frequencies corresponding to the radiation excited by a few hundreds or thousands of volts. On the other hand, the evidence obtained from a study of the β rays excited by X-rays or γ rays certainly indicates that the relation $E = ph\nu$ holds at any rate very approximately for the highest frequency examined. It is thus obvious that the emission of β and γ rays from the radio-active atoms is clearly connected with the general theory of radiation, and it seems likely that a close study of these radiations will throw much light on the mechanism of radiation in general.

There can be little doubt that the penetrating γ rays from active matter have their origin in the vibration of electronic systems in the structure of the atom outside the nucleus. The nucleus itself, however, must be violently disturbed by the expulsion of an α or β particle. If this leads to the emission of a γ radiation, it must be of exceedingly high frequency, as the forces holding together the component parts of the nucleus must be exceedingly intense. We should anticipate that this radiation would be extraordinarily penetrating, and difficult to detect by electrical methods. So far no experimental evidence has been obtained of the existence of such very high-frequency radiations, but it may be necessary to devise special methods before we can hope to do so.

[E. R.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1915

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Honorary Secretary announced the decease of Dr. Hugo W. Müller, F.R.S., on May 23, 1915, and of Sir Arthur H. Church, K.C.V.O., F.R.S., on May 31, 1915, Members of the Royal Institution; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:—

RESOLVED, That the Managers desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Dr. Hugo W. Müller, Ph.D. (Gottingen) LL.D. (St. Andrews) D.Sc. (Manchester), a Fellow of the Royal Society, a Past-President of the Chemical Society, and the Author of numerous Papers.

Dr. Müller was a Member of the Royal Institution for forty years, and served as a Visitor, a Manager, and Vice-President. He was a generous contributor towards the Funds for the Promotion of Experimental Research, and in the year 1910 gave a handsome Donation of £1000 towards the Funds of the Institution.

Dr. Müller collaborated for many years in Scientific Research with the late Dr. Warren de la Rue, and the Royal Institution possesses many Scientific Instruments that were used in their important Experimental Researches on the Electric Discharge with the celebrated Chloride of Silver Battery, consisting of 14,400 cells.

For the last twelve years Dr. Müller has been a Worker in the Davy Faraday Research Laboratory of the Royal Institution, and during that time communicated many Papers of the highest merit and originality in the field of Organic Chemistry, more especially directed to the elucidation of the Nature of the Chemical Structure of Products of the Vegetable Kingdom.

Dr. Müller was one of the most original of the old School of Scientific Workers, and contributed thirty-five Papers since 1870, sixteen of which were in conjunction with Dr. Warren de la Rue.

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Mrs. Müller and the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss the Institution and the World of Science have sustained by the decease of Sir Arthur Herbert Church, K.C.V.O. D.Sc. (Oxon) F.C.S., Fellow of the Royal Society; President of the Mineralogical Society, 1898–1901; a Fellow of King's College, London; and Professor of Chemistry in the Royal Academy, 1879–1911.

Sir Arthur Church was a Member of the Royal Institution for forty-six years, and served as a Visitor, a Manager, and Vice-President. He delivered two Friday Evening Discourses—one in 1893, on "Turacin, a Remarkable Animal Pigment containing Copper," and the other in 1907, on "Conservation of Historic Buildings and Frescoes." He also delivered an important Course of Lectures in 1891, on "The Scientific Study of Decorative Colour."

Sir Arthur Church was the Author of numerous Works, including:—"Laboratory Guide for Agricultural Students"; "Colour"; "Precious Stones"; "English Earthenware"; "English Porcelain"; "Food: its Sources, Constituents, and Uses"; "Chemistry of Paints and Painting"; "Classified Papers and Letters in Royal Society Archives." The work of Sir Arthur Church showed great variety of interests and originality in many directions.

Resolved, That the Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their sincere sympathy with Lady Church in her bereavement.

Lieut-Colonel A. G. Hadcock, late R.A.,

was elected a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of Department of Agriculture: Botanical Series, Vol. VII. Nos. 2, 3.
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Church Gazette for May-June, 1915. 8vo.
Concrete for May-June, 1915. 8vo.
Dyer and Calico Printer for May, 1915. 4to.
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Electrical Industries for May, 1915. 4to.
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WEEKLY EVENING MEETING,

Friday, June 11, 1915.

THE RIGHT HON. LORD WRENBURY, P.C. M.A., Vice-President,
in the Chair.

HENRY WALFORD DAVIES, Mus.Doc. LL.D.

Music and Poetry.

THE following Illustrations were sung by the Folksong Quartet, and Others, during the Lecture:—

1. *Fragments from Oxford Book of Verse.*

a. Kilmeny looked up with a lovely grace,
Nae smile was seen on Kilmeny's face;
As still was her look and as still was her ee
As the stillness that lay on the emerant lea,
Or the mist that sleeps on a waveless sea.

b. Never again deluding love shall know me,
For I will die;
And all those griefs that think to overgrow me
Shall be as I;
For ever will I sleep.

c. Amarantha sweet and fair,
Ah! braid no more that shining hair,
But shake your locks and scatter day.

d. But there was Jeff the provost's son,
Jeff the provost's only son.

2. *Blow, Blow, thou Winter Wind.*

Blow, blow, thou winter wind,
Thou art not so unkind
As man's ingratitude;
Thy tooth is not so keen,
Because thou art not seen,
Although thy breath be rude,

Heigh-ho! sing, heigh-ho! unto the green holly:
Most friendship is feigning, most loving mere folly:
Then, heigh-ho, the holly!
This life is most jolly.

Freeze, freeze, thou bitter sky,
Thou dost not bite so nigh
  As benefits forgot:
Though thou the waters warp,
Thy sting is not so sharp
  As friends remember'd not.

Heigh-ho! sing, heigh-ho! unto the green holly:
Most friendship is feigning, most loving mere folly:
  Then, heigh-ho, the holly!
  This life is most jolly.

"As You Like It," Act ii, Scene vii.

3. *Now is the Month of Maying.*

Now is the month of maying,
When merry lads are playing;
    Fa-la-la!
Each with his bonny lass
A-dancing on the grass,
    Fa-la-la!

The spring clad all in gladness
Doth laugh at winter's sadness;
And to the bag-pipes' sound
The nymphs tread out the ground.

Fie! then, why sit we musing,
Youth's sweet delight refusing;
Say, dainty nymphs, and speak—
Shall we play barley-breek?

4. *The Flowers o' the Forest.*

I've seen the smiling of fortune beguiling,
  I've tasted her pleasures and felt her decay;
Sweet was her blessing, kind her caressing,
  But now they are fled, they are fled far away.

I've seen the morning with gold the hills adorning
  And loud tempests storming before parting day,
I've seen Tweed's silver streams glitt'ring in the sunny beams
  Grow drumlie an' dark as they rolled on their way.

O fickle fortune! why this cruel sporting?
  O why thus perplex us poor sons of a day?
Thy frown canna fear me, thy smile canna cheer me,
  Sin' the flowers o' the forest are a' wede away.

Mrs. Cockburn.

5. *The Seven Virgins.*

All under the leaves and the leaves of life
  I met with virgins seven,
And one of them was Mary mild,
  Our Lord's Mother of Heaven.

"O what are you seeking, you seven fair maids,
  All under the leaves of life?
Come tell, come tell, what seek you
  All under the leaves of life?"

"We're seeking for no leaves, Thomas,
  But for a friend of thine;
We're seeking for sweet Jesus Christ,
  To be our Guide and thine."

So down they went to yonder town
  As fast as foot could fall,
And many a grievous bitter tear
  From the virgins' eyes did fall.

"O peace, Mother, O peace, Mother,
  Your weeping doth me grieve:
I must suffer this," He said,
  "For Adam and for Eve.

"O Mother, take you John Evangelist
  All for to be your son,
And he will comfort you sometimes,
  Mother, as I have done."

"O come, thou John Evangelist,
  Thou'rt welcome unto me;
But more welcome, my own dear Son,
  Whom I nursed on my knee."

Then he laid his head on his right shoulder,
  Seeing death it struck him nigh—
"The Holy Ghost be with your soul,
  I die, Mother, I die."

O the rose, the rose, the gentle rose,
  And the fennel that grows so green!
God give us grace in every place
  To pray for our King and Queen.

Furthermore, for our enemies all
  Our prayers they should be strong;
Amen, good Lord; your charity
  Is ending of my song.

(*Anonymous.*)

6. *Saint Stephen.*

Ancient Ballad.

Saint Stephen was a clerk
In King Herodës hall,
And servëd him of bread and cloth
As ever King befall.

Stephen out of kitchen came,
With boarës head on hand,
He saw a star was fair and bright
Over Bethlehem stand.

He kist[1] adown the boarës head
And went into the hall:
"I forsake thee King Herod,
And thy workës all.

"I forsake thee, King Herod,
And thy workës all;
There is a child in Bethlehem born
Is better than we all."

"What aileth thee, Stephen?
What is the befall?
Lacketh thee either meat or drink
In King Herodës hall?"

"Lacketh me neither meat or drink
In King Herodës hall;
There is a child in Bethlehem born
Is better than we all."

"What aileth thee, Stephen?
Art thou wode[2] or thou ginnest to breed?[3]
Lacketh thee either gold or fee
Or any richë weed?"[4]

"Lacketh me neither gold or fee,
Ne none richë weed;
There is a child in Bethlehem born
Shall helpen us at our need."

"That is al so[5] sooth, Stephen,
Al so sooth, i-wis,[6]
As this capon crowè shall
That lieth here in my dish."

---

[1] Cast. [2] Mad. [3] Upbraid. [4] Dress. [5] As. [6] Certainly.

That word was not so soon said,
That word in that hall,
That capon crew, "Christus natus est,"
Among the lordès all.

"Riseth up my tormentors,
By two and all by one,
And leadeth Stephen out of this town,
And stoneth him with stone."

Tooken they then Stephen
And stoned him in the way,
And therefore is his even
On Christès own day.

7. *Magdalen.*

Magdalen at Michael's Gate
Tirlèd at the pin;
On Joseph's thorn sang the blackbird,
"Let her in! Let her in!"

"Hast thou seen the wounds?" said Michael,
"Know'st thou thy sin?"
"It is evening, evening," sang the blackbird,
"Let her in! Let her in!"

"Yes I have seen the wounds,
And I know my sin."
"She knows it well, well, well," sang the blackbird,
"Let her in! Let her in!"

"Thou bringest no offerings," said Michael,
"Nought save sin."
And the blackbird sang, "She is sorry, sorry, sorry."
"Let her in! Let her in!"

When he had sung himself to sleep,
And night did begin,
One came and open'd Michael's Gate,
And Magdalen went in.—*Henry Kingsley.*

8. *It was a Lover.*

It was a lover and his lass,
With a hey and a ho and a hey nonny no,
That through the green corn-fields did pass
In spring time, in spring time,
The only pretty ring time,
When birds do sing,
Hey ding a ding a ding,
Sweet lovers love the spring

This carol they began that hour,
With a hey and a ho and a hey nonny no,
How that a life is but a flower
In spring time, in spring time,
The only pretty ring time,
When birds do sing,
Hey ding a ding a ding,
Sweet lovers love the spring.

(*From "As You Like It," Act v, Scene iii.*)

I.—PRELIMINARIES.

The relation between music and poetry is of very wide interest, because song is to be found everywhere—in homes, in fields, in places of work and of recreation, in places of worship, and on battlefields. For good or ill, poetry and music are put into double harness daily. When melody and words get into each other's stride, when they have unity and even identity of interests, it is obviously a most happy partnership. But there is sometimes conflict between them. Tennyson, we are told, was annoyed with music that "went up when it ought to go down, and down when it ought to go up." On the other hand, Canon Ainger remarked that he supposed it did not matter in the least what the words were provided the music was beautiful.

Tennyson and Ainger were not alone in finding music and poetry but little help to each other on occasion. A listener may find it a good working plan to ignore one art and centre attention upon the other. Yet it may be questioned whether the patient acceptance of misfits as between words and music has not become far too patient. A few days ago I read in a London paper of very wide circulation the following:

> Britain's answer to the crime of the *Lusitania* and Germany's other atrocities, has found expression in a stirring song. By permission of the publishers, we give above the words and music of the chorus.

The excerpt contained words which had energy strong enough to start a lofty strain of music, and which had a rhythm and accent of their own. But in this setting was to be found a rigid musical rhythm so violently and conflictingly superimposed upon the verbal rhythm that the reply to German atrocities became a very questionable one. (The instance was quoted.) In the larger spheres of musical and verbal partnership—in cantata, oratorio and opera—are incongruities less regrettable? They were, it is true, more violent in earlier times; and we, being removed from eighteenth and nineteenth century conventions, are able to see their absurdities very clearly. The great Handel was not immune, though, perhaps, his mightiest gift lay precisely in this, that at a stroke he could bring into being a musical theme which was the inevitable counterpart and companion of the verbal phrase which started it. Notable instances are to be found in "I know that my Redeemer liveth," "Surely He hath borne our griefs," "Total eclipse," and numberless other movements. But even he could make music ridiculous in "Samson," one of his most spontaneous works, by elaborate choral treatment of the following words:

To man God's universal law
Gave power to keep his wife in awe;
Thus shall his life be ne'er dismayed
By female usurpation swayed,

with numerous roulades on the third syllable of "usurpation," and an imposing choral cadence. (Illustration given.) Still more notably has opera brought the verbal and musical quarrel into prominence. While it must be freely admitted that the attempts since Gluck's time to make sung-dialogue congruous have been heroic and splendid, yet the absurd situations it still creates should not, I think, mislead us nor be accepted as something inevitable. Sung-drama which is meant to be reformed and purged of absurdities and incongruities, makes its men and women hold ecstatic conversation on high A, B♭ or B, with their heads very close to each other, natural enough in stage love-making, but grotesque indeed when, for vocal purposes, their mouths are open about 1¾ inches, each to each; operatic heroes, after they receive their death-blow, indulge in ten or fifteen minutes' hard singing, in which effort they, perhaps, review their lives, and sum up the world in general in terms of real music, but of hopelessly unreal drama; trivial conversations are deliberately declaimed in song. These and numberless like absurdities should at the least be frankly recognized as a mere makeshift till the veracious verbal and musical compact shall overthrow the present traditions of what is, as I believe, an automatically impossible combination. For it is clear that as the scope of music, the younger art, increases—and it does increase by leaps and bounds; lately it has literally cried aloud with growing pains—and as the divergence of aptitude of the two arts becomes more apparent, it is more desirable, and even urgent, for the general musical public, as well as musicians and librettists, to discern clearly the relation between the "Sphere-born harmonious sisters, Voice and Verse," and to recognize the conditions under which they are able, happily and without violence to each other, to "wed their divine sounds and mixt powers employ," that is, without raising mixed issues or meaning mixed meanings. That Milton was strongly alive to this need is shown by his praise of the musical settings of Harry Lawes, which presumably went up when they ought to go up, and down when they ought to go down.

It may easily be seen that the task of reconciliation is often very difficult. No one who has in any way realized Beethoven's struggles to solve the problem in the "Choral Symphony" and in the "Mass in D" can doubt this. Great words evoke mightier music than the words will hold, so the music evoked is apt to strain the verbal utterance when sung; and in the case of Beethoven the musical idea was often so titanic that the words seem to twist and break under the stress of their musical setting. This being so, it would appear wise, as music grows more potent and eloquent, to give up the problem of words. Yet the human voice remains the most lovely musical instrument; moreover, music with words will always have a wider audience than music alone. And it is the object of this lecture to show as clearly as it may, that, beyond these two reasons, there is so strong a native bond between the two arts that they cannot be put asunder; though independent of each other in their own sphere, they will always have a domain where they hold complementary and united sway.

Before going into details, I would remind you that both music and poetry, both notes and words, are vital things. They move us; and it is impossible entirely to detach vital things for analysis. A mechanism can be held in the hand, examined, analyzed, described minutely. But a melody or a line of poetry is in the last resort as elusive as a motive of the human heart. It is also highly necessary to remember that a single note, as well as a single word, could scarcely make an identical impression upon any two people. To use a homely simile, it is well to approach so vital a matter much as a dentist approaches a tooth he is to work upon, but not extract. He has sundry paraphernalia with which he does his best to isolate it from the rest of the organism; but a decent dentist never forgets that it is still indivisible from the living body of which it is a sensitive part.

II.—Fundamental Attributes.

Remembering, then, that music and poetry are inseparable from the men who make and enjoy them, let us try, for the purpose of correlating them, to isolate and describe them.

Both are obviously forms of human activity. Behind all human activities there is energy; behind some there is energy plus purpose. The purposeful doings of man are art in its wide sense, while the purposeless doings are rightly called artless, and still have a charm of their own. Now, purpose is manifested in recognizable order or design, that is, in some correlation of things or of movement that appeals to the intelligence of man, however slightly. In fact, it is energy and design, *life* and *form of life*, that are the two perpetual interests of the being and mind of man; and is not the negation of human interest to be found in their complete opposites—death on the one hand, chaos on the other? There is clearly no such thing as lifeless or formless art; failing either life or form, all art fails; and it may be noticed in passing that the work of reformers has always been either to infuse life into existent form, or to give form to life. It may, indeed, be regarded as axiomatic that Music and Poetry, manifest life and form. But, like many other lovely things, they are distinguished by a third attribute, hard to define, but none the less so real that we generally deny that they are music or poetry at all until we have discovered that they possess this attribute. It is a nameless quality to which we apply numberless incomplete names. It is the choosing force of man. It is present in artless things as whim or fancy, and we call it irresponsibility. It must needs be present in all fine art, and there it actually appears as responsibility. In its various aspects we loosely call it, or the result of it, personality, fantasy, inspiration, imagination, free-will, spontaneity, creativeness, enthusiasm, and all sorts of other names. We delight in it. We admire it. Life is no fun without it. Of course its greatest and commonest name is Love; and roughly we may say that to be intelligibly active for love is to be an artist, while to be intelligibly active without choice, at some one else's bidding, is to be an artizan. When the purpose in art is not your purpose you are not the artist; when it is so entirely your purpose as to evoke a spontaneous, joyous act, you really arrive not only at art, but at fine art. George Herbert expounded the practical policy of all artizans who would be artists in his famous couplet about sweeping a room.

Now, Music and Poetry are everywhere acknowledged to be among fine arts. I think it is well at this point consciously to resist the confusion which arises from the frequent distinction between *useful* and *fine* arts. In the fundamental sense this distinction makes nonsense. An art may be dull and useful, but it cannot possibly be fine and useless. It may even be said that fine arts are those that happen to be exuberantly useful. Just as felicitous prose is as much fine art as poetry or oratory, so bare tones of voice in any prosaic utterance, when lit with enthusiasm, become music in their inflection, colour and rhythm. Sir Hubert Parry, writing on Melody in Groves's Dictionary, says: "Melody probably originated in declamation through recitative, to which it has the closest relationship," and he describes how recitative "no doubt merged into melody at times, much as prose in passages of strong feeling merges into poetry." Exuberance and enthusiasm transform the dullest things. A short while ago I showed the Temple organ diapasons of various dates to a literary friend. A few days later I read with pleasure in the *Spectator* that "the soul of three centuries speaks from its keys."

I think it is hardly sufficiently recognized that it is strong enthusiasm that carries words or notes into attractive or impressive patterns, that enthusiastic energy actually makes for formal design. It is easy to see that a striking design strengthens the mental appeal, and thus we say that rhyme or rhythm makes facts more memorable. It would be covering the ground better to say that they actually augment life, and are the spontaneous expression of augmented life.

Stammerers know this, and cure their stammer by augmentation of speech into song. I know a man, seventy-five years old, who still will repeat the historic dates of England with some zest because he made up some delightful doggerel at school which lit the dry facts up and made them both alive and memorable then and ever since, childish as was the jingle :—

> In the year ten hundred and sixty-six
> William the Conqueror Harold licks;
> In the year ten hundred and eighty-seven
> William died: did he go to heaven?

Welsh preachers have an art all their own, in which prosaic utterance merges into oratory and oratory into a species of chant, which becomes defined and dignified in proportion to the enthusiasm and vision behind the utterance.

Both Music and Poetry may, then, be described in common with all fine arts as Energy in Design, characterized by a certain spontaneous exuberance, a joyous Inspiration. It is their exuberance which forms the strongest natural bond between the two arts; but, further, since both happen to be expressed in terms of sound, and approach us through the same avenue of sense, they naturally both express themselves along the same lines. This is important. They have the same outward attributes of expression, which we may call their identities, and which I will ask you very carefully to note.

III.—Identities of Speech and Song.

It is obvious that since every sound, whether it offers itself to the ear as a word or a tone—in other words, whether as a communicative or an attractive unit of expression—has its three measurements (it is high or low, long or short, strong or weak); variety of form is, therefore, expressed by variations along three lines :

1. By Inflection.
2. By Rhythm.
3. By Emphasis.

And since practically all sounds are composite, there is almost always a fourth inherent attribute, most important of all, which we call quality, so that variety is expressed also

4. By distinctions of Quality.

These may be called the four identities of speech and song, and their familiar applications to the two arts may be tabulated as follows :

TABLE OF COMPARISON.—I.

Identical Attributes of Verbal and Musical Expression.

Attributes of utterance.	Verbal application.	Musical application.
1. Inflection.	In rise and fall of the speaking voice.	In melody.
2. Rhythm.	In verbal quantities (metre).	In rhythmics of every kind.
3. Emphasis.	In verbal accent.	In loud and soft (as indicated by so-called *marks of expression*).
4. Quality.	In verbal colours and *timbre* of speaking voice.	In harmony and *timbre* of singing voice and of musical instruments.

For purposes of illustration the Oxford Book of Verse was opened haphazard, and the passages printed in your programmes were selected from the chance page. In these the working of the four identities—[illustrations were here spoken and sung]—may so easily be traced that it is hard to see how the language of music can escape from becoming the Esperanto of all nations. The relation is so intimate that many poems almost set themselves to music. But at this point we may trace some interesting tendencies in the illustrations before us. Look at the biting satire and insistent rhythm of No. 2 on your programme, with its savage accents on the first word of each stanza, and then listen to Arne's setting; a startling discrepancy shows itself. Fortunately, an excellent modern setting by Mr. Roger Quilter exists which preserves the identities, and while it is perhaps less elegant or pleasing melody than Arne's, it enhances Shakespeare's rhythms, inflection and emphasis in a way that convinces and makes the words more easily memorable.

The next illustration shows the constant tendency of musical rhythms to repeat and extend themselves beyond all possible needs of the words themselves, and shows also how the old composers met this by ingenious use of the slender but light-hearted and good-natured *fa-la-la* refrains. To these and like refrains we shall doubtless return increasingly in future choral music.

The next example directs your attention to a significant instance of verbal colour or quality which calls for exact and faithful matching of harmony and tone of voice in the musical setting:

I've seen Tweed's silver streams glittering in the sunny beams
Grow drumlie and dark—

IV.—DIVERGENCES AND DIFFERENCES.

As one listens to examples such as these, though it may grow increasingly clear to the hearer that relationships at once so fundamental and audible should secure the consistent partnership of the two arts for always, yet it becomes more apparent that while they grow in their own ways, real discrepancies do arise which are not all due to simple miscalculation of composers. Their divergences of aptitude may now carefully be noted.

They can be most easily traced at their source, that is, in the first prattle of infants. Of course the very early noises of babies have very little in them that can truly be called speech or song. But a crude language soon emerges. A mouth wide open emits an open vowel, so that the animation of pleasure in a laugh or pain in a cry both naturally resolve themselves into the baby-music of *ah*, with all its subtle variations of colour and rhythmic recurrence which connote life to the baby and a definite language to the mother. Between the vocal energies of pleasure and pain there lies a consonantal region of calm when the muscles are at rest. *M* (lips closed) seems the natural sound of contentment, and is associated with the name of mother in almost all languages. *N* comes next in such coined words as *na-na*, generally used to indicate a second-best companion. Indeed, an infant's complete vocabulary for a while seems to be summed up in such sounds as *ma-ma*, *na-na*, *da-da*, with a few supplementary efforts (*ga*, *ka*, and the like), which, taken together with the vigour of rhythm and vividness of vocal colour with which they are uttered, convey aversion or attraction very effectively, while they are not more complex than the sounds of small birds or lambs. In this extremely limited language the baby is true both to verbal and musical principles. The words it invents come each to mean *exactly one kind of object in the world*, while they at the same time express *the state of feeling associated with that object.*

If the development of tonal and verbal utterance be traced as it proceeds it will be seen that music, on the one hand, tends to express every shade of vitality in infinite variety, coining new words (so to speak) as it goes along, and in this way it becomes more directly vital and more expansive than words. Language, on the other hand, specializes in *associated* significances; in it we decide, whatever the changes of *vital* significance may be, always to utter the same sound in connexion with one object or circumstance in our experience. By this means words acquire by degrees a surprising power of precision and a definiteness of suggestion to which music does not aspire. For example, by the arbitrary expression *equilateral triangle* it is possible instantly to call up one and the same image in a thousand minds of utterly different calibre in entirely different environment. This exact image or impression was called up five hundred years ago, and will be, presumably, five hundred years hence. Music can do no such thing as this, but aspires in other directions. So it comes about that words tend to move us indirectly by the things they are able to call up or suggest, while music tends to move us not so much by association as by direct tonal energy. While words cultivate the expression of the infinitely minute, music aspires to voice the plenitudes of the universe.

But it should be carefully observed here that the two, while they diverge and specialize, never entirely part company. The fundamental link between tones and words survives in the most highly organized languages, and music in its turn cultivates a necessary definiteness very like verbal definiteness. Tone is, and remains, a vital part of verbal as of musical art, and probably every language preserves a large leaven of true tonal significance in its strongest words. Think of the hoot of the abusive appellation *fool*, or the hiss of *ass*, as the schoolboy uses it; think of the gentleness of the sound in the words *love*, *long*, *linger*; and, in passing, note the tonal effect of numberless pictorial words, such as *rush*, *swish*, *kick*, *hullabaloo*, and so forth. Verbal expression from the first keeps its hold on words that are, like music, vitally expressive of the energies they present. And similarly, though its divergent aptitudes lead music to specialize in direct and vital expression, yet chords and melodies, like words and sentences, also gather associated meanings as they are used; indeed, there exists a strangely definite harmonic language, though still in embryo, and though the constant tendency of music is away from definite significances towards those that may be called infinite. In short, the two arts preserve their likenesses while they develop differences. These differences may now be more thoroughly enumerated and tabulated.

TABLE OF COMPARISONS.—II.

Divergence of Aptitude in Verbal and Musical Utterance.

VERBAL UTTERANCE.	MUSICAL UTTERANCE.
1. Develops aptitude to express indirect associated meanings.	1. Develops aptitude to express direct vital meanings.
2. Primarily quickens the hearer by what it suggests, and secondarily by its own sound.	2. Primarily quickens by its own energy, and secondarily by suggestion.
3. Is apt to become precise, particular, local, definite in its significances.	3. Is apt to become vast, vague, general, universal in its significances.
4. Remains vague and wayward in its inflections, rhythms, lights and shades, and colours.	4. Becomes clear, definite, highly organized in melody, harmony, rhythm, light and shade.
5. Uses the device of repetition primarily for emotional and secondarily for structural purposes.	5. Uses repetition primarily for structural and secondarily for emotional purposes.
6. Tends to quicken thought by diversity of detail.	6. Tends to augment energy rather than thought by diversity.
7. Attains distinction in minuteness.	7. Attains distinction in magnitude.

The attempt to make this table of comparisons complete has made it too cumbersome to memorize, and too extensive to discuss in detail here; but out of it three main facts emerge which are sufficiently comprehensive for our present purpose, and which may be stated as follows:

1. *Words develop definiteness of outline and tend to vitalize by that which they indirectly suggest to the mind, whereas music tends to develop direct expression of the vitalities themselves.*

Perhaps this divergence becomes clearer by analogy with the arts of line and colour in painting; the one gives definition of image, indicating, e.g., a church tower, a tombstone, or a gate against the sky; the other gives vitality and possibly superhuman beauty to the picture. One excels in minuteness of suggestion, and moves us indirectly by the force of the associations it quickens; the other excels in vastness, variety, or magnitude of suggestion. This particular analogy is doubly helpful because it enables us to see, further, that as colour can fulfil the function of line, and as line upon line in shading can simulate colour, so music may sometimes fulfil the function of words by its definiteness, and word upon word may be so accumulated as to amount to something like music in monochrome, not with the rich colours of tone, but with the shadings and tints of verbal art. The second main fact is this:

2. *The device of repetition acquires different significances in the two arts.*

Repetition in art has its vital uses and its formal uses. In verbal art you will readily see that repetition of word or phrase is mainly used to quicken emotion; it augments feeling and rather dilutes thought. This is the secret of repeating a battle-cry, when men are not wanted so much to think as to rush on and act. Repetition in music, on the other hand, though also emotional, is primarily a constructive device. I think the difference of the mental appeal of the two arts is well conveyed in the words *information* and *conformation*, or by the companion words *instruction* and *construction*. Words quicken thought by processes of *information*, music by processes of *conformation*. Thought-quickening poetry eschews repetition and pours in more informing words, whereas thoughtful music will be as full of recurrence of rhythms and inflections as its conformation needs; it will fulfil its design much as a cathedral does by arch upon arch, window upon window, pillar after pillar. It may easily be seen that this second divergence can be fatal to the partnership. It has led to the most amusing failures in oratorio; I dare not pause to describe them. It is far pleasanter to observe that the device of repetition can be so used as to fulfil both ends simultaneously. A convincing instance of it is to be found in a passage in the savage triumph song of Deborah and Barak:

> At her feet, he bowed, he fell, he lay:
> At her feet, he bowed, he fell:
> Where he bowed,
>   There he fell,
>   Down,
>   Dead.

Here the emotional and constructional repetitions are completely at one. Another illuminating example of how a poet does or does not meet music half-way is to be found in comparing a stanza from Tennyson with a stanza from Burns. In one the poet seems to have no need of the composer:

I leave thy praises unexpressed
In verse that brings myself relief,
And by the measure of my grief
I leave thy greatness to be guessed.

In the other the emotional repetitions are as perfectly conformed to music's structural needs as they could possibly be, and consequently the verse cries out for music:

I see her in the dewy flowers,
I see her fresh and fair,
I hear her in the tunefu' birds,
I hear her charm the air.

Keble has paid this latter stanza the tribute of imitation in sacred verse, and acknowledges his debt in a footnote.

The third fact is this:

3. *By the nature of things the mere dimensions of poetical and musical expression are bound to diverge.*

If words are to move us not by what they are but by what they call to mind, then their strength is in their own modest dimensions. If music moves us by its own light and shade, its own colour, its own rhythmics and dynamics, it will obviously grow as much vaster than words as a sunset sky is vaster than an artist's neck-tie. As a matter of fact, a speaking voice moves within an octave or so, and wastes no lung-power either in shouting or sustaining syllables. The violin moves through four or five octaves, and an organ through nine; their beauty is in *sostenuto*, and it is one of their advantages that they never need break that *sostenuto* to take a breath.

V.—Results of Divergences.

We have now very briefly considered the four identities of words and music, and their three main divergences: (1) in method or technique of appeal; (2) in use of the device of repetition; (3) in mere physical magnitude. What practical conclusions may writers of, and listeners to song deduce which can be helpful? It is desirable before concluding to note a few.

It clears the listener's judgment to remember that the so-called identities will not change, for they are in nature, and must remain as long as sound is sound and speech and song approach us through our ears. In order to enjoy the words and music of a song as a whole, and to form a consistent judgment upon it, the listener should look for a double joy, but a single issue. The superimposition of an alien musical rhythm upon a familiar verbal rhythm confuses the issue. This does not mean that the music must slavishly reproduce the bare verbal rhythm, for it may enhance it; nor does it preclude the most subtle interplay of two or more rhythms, for they may be implied in the poem. But it is an unwritten law of both arts that a superfluous word or note is a wrong word or note, and a law that holds in each art obviously holds in both. A superfluous rhythm is a wrong rhythm. In *metre* and *accent* poetry already possesses its inherent rhythm and emphasis, and the song fares badly whose composer is deaf to this. In *inflection* and *quality* (in other words, in *melody* and *harmony*) music can expand poetic significances, the singing voice far outstripping the speaking voice, provided expansion never obliterates or distorts the inner beauty of the words themselves—provided, in other words, that it is never irrelevant. Music can only be irrelevant when the poem already is as vital as the hearer can bear and when its mental appeal is complete. This seems to be the case in the Tennyson stanza just quoted. Dean Beeching goes so far as to say some poems are so complete that they make their best impression on the written page in silence. Words, on the other hand, are irrelevant in sonata or symphony when music is not only as vital by itself as we can bear, but when its conformation or structure fulfils our mental attention—when, in fact, the listener is as busy as he can be enjoying the purely musical experiences. These are the occasions upon which, if explanatory words are needed to hitch us on to the energies of music, a composer fights shy of having them sung, and so writes what is called programme-music. Perhaps all abstract music is programme-music, only that each hearer can have a wide field for his own programme. Between the two extremes, and close to their edges, there are two kinds of vocal music—one in which the poetic interest is so great that the musical share is just reduced to a minimum, as in dry recitative; the other, in which the musical interest is so great that the verbal contribution is reduced to one single utterance, such as the word *Sanctus*, or *Hallelujah*, or *Amen*. There is striking testimony to the natural human need of human interest in the quaint foreign comment on our English Festivals: "Ah, you English, I know your Music Festivals. It was Hallelujah, Amen, for four days!" Between the two border regions the divergences of the two arts can be made to complement each other, they can play into each other's hands; and when they do it well the experience can be perfect.

The remaining illustrations may be said to lie in this mid-region. They will be sung through without comment or interruption, and the listener is asked to set his mind chiefly upon the factor of *adjustment* as between the vital and formal appeal. Ideally they should approach one as a whole, simultaneously, indistinguishably fulfilling both needs; and only a certain sense of amplitude and of complete satisfaction testifies that they are both working. This should be felt pre-eminently in such ballads as the "Seven Virgins." (The illustration was sung.) But sometimes the emotional and mental appeal are alternately stronger each than the other, and in this case are more distinguishable and more capable of conscious analysis. To illustrate this I have roughly set the striking old legendary ballad called "Stephen" to music, in which the alternations of the poem are reinforced by musical alternations of voices, melodies, cadences and keys, which are all conformed to the structure of the stanzas. (Illustration sung.)

The next illustration exemplifies the curious fact that a poem sometimes enhances its appeal by an attempt to suggest natural sounds by speech akin in colour or rhythm to those sounds, and that music returns the compliment by re-translating speech into notes that are music, and that yet approximate to the natural sound originally suggested by the poet, in this case the cry of a bird. ("Magdalen" was here sung.)

The final illustration is refreshing because it seems to complete the indications that at every point music and poetry are ready to play into each other's hands, and it is well to end with an example (taken from old Morley and the golden age of vocal art) which suggests a new point of contact, one that will undoubtedly receive the attention of composers as they grow in sensitiveness to the art of song. It is the only case I recal in which the music consistently *rhymes* its melody with the verbal rhyme. ("It was a lover and his lass" was sung.)

VI.—Conclusions.

To sum up these inquiries in a very few words, it may be said that we shall best appreciate the relation of the two arts if we remember that they are both the free-will expression of vitality; hence they imply a joyous interest in life. We cannot be said to be joyously interested in the slightest thing or experience till we find both freedom and intelligible order or purpose there. Hence it comes that Energy, Freedom, Order—or in other words (alliterated to help the memory) Force, Fantasy, Form—represent the true trinity of art, if not of all human attainment whatsoever. These three may be more fully expressed as Life, Love of Life, Love of Form of Life. Music and Poetry have four identical ways of presenting or expressing these three. But music diverges by itself into ways which, though vital, are so detached from ordinary associations and surrounding interests that it is in danger of ceasing to interest localized mortals. The exquisite orderliness of euphonious sound and all lovely correlation of sounds are refreshing to contemplate. But we are only susceptible to them when they hitch on to our personal lives. It so happens that verbal art specializes in personal, local, or associated meanings, while music does so in a vaguer, vaster fantasy of impersonal energies. Happily, the two arts, united at their infant source, identical in their fundamental attributes, both develop along ways which enable them to supply each other's wants and enhance each other's abilities. There is no end to the treasures of melody, harmony, rhythm, light and shade. They are truly as vast as the universe. Poetry has, it is true, linguistic limits, but her very limitations give her the power of definite appeal which music often lacks. In a word the compact is this: While music can illumine and strengthen words which otherwise might want wings, poetry can give definiteness to high-flying music in a way which links it to man's immediate needs. Only let one fact at least stand as an axiom, that whenever poetry and music are to be heard together *no violence shall be done to either.* Verbal plus musical veracity should be exacted in this sacred compact. That they have identity of attributes, divergence of aptitudes, and unalterable differences of dimension as they pursue their aptitudes—these are, after all, armchair facts, frankly to be acknowledged and made a practical basis of understanding between poets, composers and audiences.

They can serve each other to perfection, now by blend, and now by alternation of interests. It is a vital and mystical union which, as Milton has expressed it for all time, enables them together to pierce dead things with inbreathed sense—

> And to our high-raised fantasy present
> That undisturbed song of pure consent.

[H. W. D.]

GENERAL MONTHLY MEETING,

Monday, July 5, 1915.

His Grace the Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

William H. Glaser,
Mrs. Skinner,

were elected Members of the Royal Institution.

The Managers reported, That in conformity with the Trust Deed, they had appointed Sir James Dewar, LL.D. D.Sc. F.R.S., Fullerian Professor of Chemistry.

The Right Hon. Lord Wrenbury stated that he was authorized to report to the General Meeting of Members two Resolutions passed by the Managers at their Meeting this day, and which under the special circumstances the Members might desire to support:

Resolved, That the Royal Institution is desirous, as it always has been, of rendering to His Majesty's Government in relation to the War every assistance which lies within their objects as defined and governed by the Charter and Act of Parliament governing the Royal Institution, and by the Conveyance and Deed of Trust of the Davy Faraday Research Laboratory.

Resolved, That, with a view to giving effect to this desire, letters be addressed to the First Lord of the Admiralty, the Secretary of State for War, the Minister of Munitions, and the Chairman of the Inventions Board of the Admiralty, requesting them to communicate with the Managers, in case there is any field of Research in relation to or connected with Chemical and Physical Science, or either of them, to which the Professors, Assistants, and Staff of the Royal Institution or of the Laboratory can usefully direct their attention with a view to giving assistance to His Majesty's Government in the conduct of the War, or in case they desire that any Members of their respective Staffs shall be admitted to work in the Laboratory in the prosecution of any such Research.

The above Resolutions were independently supported by the unanimous Vote of the Members present.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute, Pusa Bulletin, Nos. 47, 50. 8vo. 1914.
  Report on the Progress of Agriculture in India for 1913–14. 8vo. 1913–14.
  Agricultural Journal of India for April, 1915. 8vo.
  Kodaikanal Observatory, Bulletin, No. 46. 4to. 1915.
  Geological Survey Records, Vol. XLV. Part 2. 8vo. 1914.
Astronomer Royal—Annual Report to the Visitors of the Royal Observatory, Greenwich, for the Year ending May 10, 1915. 4to. 1915.
Accountants, Association of—Journal, Vol. VIII. No. 43. 8vo. 1915.
American Geographical Society—Bulletin, Vol. XLVII. No. 6. 8vo. 1914.
Astronomical Society, Royal—Monthly Notices, Vol. LXXV. No. 7. 8vo. 1915.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXII. Nos. 15, 16. 4to. 1915.
British Association for the Advancement of Science—Report of the Eighty-Fourth Meeting, July 28–August 31, 1914, Australia. 8vo. 1915.
British Astronomical Association—Journal, Vol. XXV. No. 7. 8vo. 1915.
Buenos Aires—Bulletin of Municipal Statistics for Jan.–Feb. 1915. 4to.
Cambridge Philosophical Society—Proceedings, Vol. XVIII. Part 3. 8vo. 1915.
Canada, Department of Mines—Bulletin, Nos. 9–14. 8vo. 1914.
  Geological Survey: Memoir, Nos. 30, 56, 57, 59. 8vo. 1914.
  Mines Branch: Summary Report of the Mines Branch for the Year 1914. 8vo. 1915.
    Annual Report on the Mineral Productions of Canada in 1913. 8vo. 1914.
  Preliminary Report on the Bituminous Sands of Northern Alberta. 8vo. 1914.
  Museum Bulletin, No. 10. 8vo. 1915.
Canada, Royal Society of—Geological Section: Transactions, Vol. VIII. Dec. 1914–March 1915. 8vo.

Mathematical, etc., Section: Transactions, Vol. VIII. Dec. 1914–March 1915. 8vo.
Literary and Archæological Section: Transactions, Vol. VIII. Dec. 1914–March 1915. 8vo.
Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, No. 96. 8vo. 1915.
Communications to the National Academy of Sciences, Nos. 6–8. 8vo. 1915.
Chemical Industry, Society of—Journal, Vol. XXXIV. No. 12, 1915. 8vo. 1915.
Chemical Society—Journal for June, 1915. 8vo.
Editors—Aeronautical Journal for April, 1915. 8vo.
Amateur Photographer for June, 1915.
American Journal of Science for June, 1915. 8vo.
Athenæum for June, 1915. 4to.
Chemical Engineering and the Works Chemist for May, 1915. 8vo.
Chemical News for June, 1915. 4to.
Chemist and Druggist for June, 1915. 8vo.
Dyer and Calico Printer for June, 1915. 4to.
Electrical Engineering for June, 1915. 4to.
Electrical Industries for June, 1915. 4to.
Electrical Review for June, 1915. 4to.
Electrical Times for June, 1915. 4to.
Electrical Vehicle for June, 1915. 8vo.
Electricity for June, 1915. 8vo.
Engineer for June, 1915. fol.
Engineering for June, 1915. fol.
Ferro-Concrete for June, 1915. 8vo.
Horological Journal for June, 1915. 8vo.
Illuminating Engineer for June, 1915. 8vo.
Imperial Review, Melbourne, No. 53. 8vo. 1915.
Journal of Physical Chemistry for June, 1915. 8vo.
Journal of the British Dental Association for June, 1915. 8vo.
Junior Mechanics for June, 1915. 8vo.
Law Journal for June, 1915. 8vo.
London University Gazette for June, 1915. 4to.
Marine Engineer for June, 1915. 8vo.
Model Engineer for June, 1915. 8vo.
Musical Times for June, 1915. 8vo.
Nature for June, 1915. 4to.
Nuovo Cimento for Jan.-Feb., March-April, 1915. 8vo.
Page's Weekly for June, 1915. 8vo.
Physical Review for June, 1915. 8vo.
Power for June, 1915. 8vo.
Power-User for June, 1915. 8vo.
Science Abstracts for June, 1915. 8vo.
War and Peace for June, 1915. 8vo.
Wireless World for June, 1915. 8vo.
Edinburgh, Royal College of Physicians—Reports from the Laboratory Vol. XIII. 8vo. 1915.
Electrical Engineers, Institution of—Private Branch Exchanges, by J. H. Kennedy and J. W. Turner. 8vo. 1915.
Faraday Society—Transactions, Vol. X. Parts 2, 3. May, 1915. 8vo.
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Geological Society—Abstracts of Proceedings, No. 977. 8vo. 1915.
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London County Council—Gazette for June, 1915. 4to.
Manchester Literary and Philosophical Society—Proceedings, Vol. LVIII. Part 3. 8vo. 1915.

Manchester Steam Users Association—Thirty-second Annual Report on the Working of the Boiler Explosions Act, 1882 (Amended, 1890). 1915
Meteorological Office—Monthly Weather Reports for April, 1915. 4to.
Weekly Weather Reports for June, 1915. 4to.
Daily Readings for March, 1914. 4to.
Mining and Mechanical Engineers, North of England Institute of—Transactions, Vol. XLV. Part 4. 8vo. 1915.
Mitchell, P. Chalmers, M.A., D.Sc., LL.D., F.R.S. (the Author)—Science and Life. 8vo. 1914.
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GENERAL MONTHLY MEETING,

Monday, November 1, 1915.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

The Honorary Secretary announced the decease of Sir Andrew Noble, Bart., K.C.B. D.C.L. D.Sc. F.R.S., on October 22, 1915, and of Mr. William Hugh Spottiswoode on August 20, 1915, Members of the Royal Institution; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Sir Andrew Noble, Bart., K.C.B. D.C.L. D.Sc. F.R.S., Hon.M.Inst.C.E., and recipient of a large number of Foreign Orders, late Member of various Government Select Committees on Explosives.

Sir Andrew Noble was a Member of the Royal Institution for twenty-eight years, and served as a Visitor, a Manager and Vice-President. He was a generous contributor towards the Royal Institution Fund for the Promotion of Experimental Research.

In association with the late Lord Armstrong, Sir Andrew Noble took the most prominent part in the development and management of the Elswick Works. In the midst of great business responsibilities, he found time to carry out extensive experimental and chemical investigations upon the properties of various powders employed in artillery. His classical investigations on the composition, volumes, pressures and temperatures of the gases developed by different explosives during the period of combustion have been universally recognized as researches of the highest importance in the Scientific World.

Sir Andrew Noble delivered three Friday Evening Discourses: "On the Tension of Fired Gunpowder," 1871; "Some Modern Explosives," 1900; and "Fifty Years of Explosives," 1907. His investigations are summarized in a work on "Artillery and Explosives," published in 1906.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Noble and the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire, at this their first Meeting since his death, to record their sense of the loss sustained by the Institution, by the decease of William Hugh Spottiswoode, eldest son of the late William Spottiswoode, for fourteen years Treasurer and Secretary of the Royal Institution.

Mr. William Hugh Spottiswoode was a Member of the Royal Institution for twenty-five years, and served as a Visitor, Manager and Vice-President. As a Manager he rendered valuable assistance in the transactions of the business affairs and concerns of the Institution, and was deeply interested in its welfare.

In the year 1899, Mr. Spottiswoode presented his father's collection of Optical Apparatus, which is unique in containing Polariscopic Prisms and Crystal Sections of quite exceptional value and interest, and which could not be replaced.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Mrs. Spottiswoode and the family in their bereavement.

Wilfrid Ward, F.S.A.,

was elected a Member of the Royal Institution.

The Special Thanks of the Members were returned to Richard Pearce, Esq., F.G.S., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

Also to Mrs. Hugo Müller for her Gift of a Microscope, which formerly belonged to and was used by the late Dr. Warren De La Rue, F.R.S., Secretary of the Royal Institution from 1879–1882.

The Chairman stated that, in view of the depletion of the Staff, owing to the exigencies of the War, the Managers had considered whether it would not be advisable to alter the hour of the Friday Discourse from 9 p.m. to 4 p.m. during the Session before Easter.

Lord Wrenbury made a few remarks on the subject, calling the attention of the Members to the matter for consideration.

The Resident Professor stated the difficulties in regard to continuing the Friday Discourses at the usual hour, and the necessity for making a change under present conditions.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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SPECIAL GENERAL MEETING,

Monday, December 6, 1915.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

HIS Grace the President stated that the objects of the Meeting were to consider Resolutions to be proposed in the following terms:—

(I.) That it is desirable to suspend Bye-Law Chap. XVIII, Art. 2, until such time as the Members in General Meeting shall resolve that the same be restored, and that during such suspension the Managers shall from time to time determine the day and hour of the Weekly Meetings of the Members, and the hour for delivery of the usual discourse.

(II.) That pending the passing and confirmation of the Resolution giving effect to such suspension, the Managers be requested to make such arrangements as to the holding of the Weekly Meetings and as to the delivery of the usual discourse as they think proper.

His Grace the President moved, and Lord Wrenbury seconded, the Resolutions, which were adopted.

The majority of the Members present were in favour of 5.30 as the hour for the delivery of the usual discourse.

GENERAL MONTHLY MEETING,

Monday, December 6, 1915.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

NOTICE is Given that it is intended to take the necessary steps to suspend Bye-Law Chapter XVIII, Art. 2, and that the necessary Resolution will be moved at the next General Monthly Meeting—

that is, at the General Monthly Meeting to be held on the first Monday in the month of February.

NORTHUMBERLAND.
JAMES CRICHTON-BROWNE.
WRENBURY.
CHAS. J. TABOR.
A. N. BUTT.
J. G. GORDON.
ALEX. SIEMENS.
ROTHSCHILD.
WALTER CHILD.
THOMAS BARLOW.
F. W. MANDER.
MAURES HORNER.
CHARLES HAWKSLEY.
W. B. GIBBS.
F. LYS SMITH.
EDWARD POLLOCK.

Royal Institution,
Albemarle Street, W.,
6*th December,* 1915.

The Honorary Secretary announced the decease of Sir Arthur Rücker, M.A. LL.D. D.Sc. F.R.S., on November 1, 1915, and of Professor Raphael Meldola, LL.D. D.Sc. F.R.S. F.C.S., on November 16, 1915, Members of the Royal Institution; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and by the World of Science, by the decease of Sir Arthur Rücker, M.A. LL.D. D.Sc. F.R.S. M.I.E.E., Principal of the University of London, Professor of Physics at the Royal College of Science, Past President of the British Association, a Member of Royal Commissions on various Universities; awarded a Royal Medal by the Royal Society of London.

The published papers of Sir Arthur Rücker cover a wide field of research. The work which he did in co-operation with other distinguished scientific men has been recognized as of world-wide importance, especially the "Magnetic Survey of the British Isles," and "Investigations on Physical Properties of Liquid Films."

Sir Arthur Rücker was a Member of the Royal Institution for twenty-nine years, and served as a Visitor and a Manager. He delivered five Friday Evening Discourses: "Liquid Films" (1885); "Electrical Stress" (1889); "Magnetic Rocks" (1891); "Physical Work of Von Helmholtz" (1895); and "Earth Currents and Electric Traction" (1899). He also delivered a Course of Lectures on "Molecular Forces" (1887), and a Christmas Course of Juvenile Lectures on "Electricity" (1889-90). Such a varied scientific output shows how much the progress of Science has benefited by Sir Arthur Rücker's labour, and how generously he supported the objects of the Royal Institution in the diffusion of knowledge.

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Professor Raphael Meldola, LL.D. D.Sc. F.R.S. F.R.A.S. F.C.S. F.I.C., Professor of Chemistry in the Finsbury Technical College, Past President of the Chemical Society, the Society of Chemical Industry, Society of Dyers and Colourists, Institute of Chemistry, and the Entomologists' Society, Chairman of the Advisory Council of the Board of British Dyers; awarded a Davy Medal by the Royal Society of London.

Professor Meldola was one of the most distinguished Chemists of the time. His researches in the Chemistry of Colouring Matters, Photographic Chemistry, his works on "Chemical Synthesis of Vital Products, "Coal, and what we get from it," and his many original papers on chemical subjects, have opened some rare fields of enquiry, some of which have led to great industrial success.

Professor Meldola was not only distinguised as a Chemist, but also as a Biologist, and a man of intellectual activity in all departments of knowledge.

He was a Member of the Royal Institution for thirty years, and served as a Visitor and a Manager. He delivered two Friday Evening Discourses: "The Photographic Image" (1890); "Mimetic Insects" (1891). He also delivered a Course of Lectures in 1905 on "Synthetic Chemistry."

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Mrs. Meldola and the family in their bereavement.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of the Department of Agriculture: Bacteriological Series, Vol. I. No. 6; Botanical Series, Vol. VII. Nos. 5 and 8; Chemical Series, Vol. IV. No. 3.
Kodaikanal Observatory, Bulletin No. 47. 4to. 1915.
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Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. 2o Semestre, Fasc. 8.
Admiralty, Lords Commissioners of the—Nautical Almanac for 1918. 8vo. 1915.
American Chemical Society—Journal for Nov. 1915. 8vo.
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American Geographical Society—Bulletin, Vol. XLVII. No. 11. 8vo. 1915.
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British Archæological Association—Journal, Vol. XXI. Part 3. 8vo. 1915.
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Kalendar, 1915-16.
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Buenos Aires—Monthly Bulletin of Municipal Statistics for July-Aug. 1915. 4to.
Canada, Department of Mines—Geological Survey: Memoirs, Nos. 67, 70, 71 and 78. 8vo. 1912-15.
Anthropological Series, No. 7.
Museum Bulletin, No. 19, 1915. 8vo.
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List of Officers and Members, 1915.
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Dewar, Sir James, M.A. LL.D. F.R.S.M.R.I.—Metals and Metal-Working in Old Japan. By W. Gowland.
Investigaciones Experimentales sobre Los Espectros de la Descarga Oscilante. By Adolfo T. Williams.
East India Association—Journal, Vol. VI. No. 4.
Editors—Amateur Photographer for Nov. 1915.
American Journal of Science for Nov. 1915. 8vo.
Athenæum for Nov. 1915. 4to.
Author for Nov. 1915. 8vo.
Chemical News for Nov. 1915. 4to.
Chemist and Druggist for Nov. 1915. 8vo.
Church Gazette for Nov. 1915. 8vo.
Concrete for Nov. 1915. 8vo.
Dyer and Calico Printer for Nov. 1915. 4to.
Electrical Engineering for Nov. 1915. 4to.
Electrical Industries for Nov. 1915. 4to.
Electrical Review for Nov. 1915. 4to.
Electrical Times for Nov. 1915. 4to.
Electricity for Nov. 1915. 8vo.
Engineer for Nov. 1915. fol.
Engineering for Nov. 1915. fol.
Ferro-Concrete for Nov. 1915. 8vo.
General Electric Review for Nov. 1915. 8vo.
Horological Journal for Nov. 1915. 8vo.
Illuminating Engineer for Nov. 1915. 8vo.
Journal of Physical Chemistry for Nov. 1915. 8vo.
Journal of the British Dental Association for Nov. 1915. 8vo.
Junior Mechanics for Nov. 1915. 8vo.
Law Journal for Nov. 1915. 8vo.
London University Gazette for Nov. 1915. 4to.
Marine Engineer for Nov. 1915. 8vo.
Model Engineer for Nov. 1915. 8vo.
Musical Times for Nov. 1915. 8vo.
Nature for Nov. 1915. 4to.
New Church Magazine for Nov. 1915. 8vo.
Page's Weekly for Nov. 1915. 8vo.
Physical Review for Nov. 1915. 8vo.
Power for Nov. 1915. 8vo.
Power-User for Nov. 1915. 8vo.
Science Abstracts for Nov. 1915. 8vo.
Tchèque, La Nation, for Nov. 1915. 8vo.
War and Peace for Aug.-Nov. 1915. 8vo.
Wireless World for Nov. 1915. 8vo.
Zoophilist for Nov. 1915. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIV. No. 25. 8vo. 1915.
List of Members, 1915. 8vo.
Franklin Institute—Journal, Vol. CLXXX. No. 5. 8vo. 1915.
Geographical Society, Royal—Journal, Vol. XLVI. No. 5. 8vo. 1915.
Hamlyn-Harris, Dr. R., D.Sc., F.L.S. F.R.M.S. F.Z.S.—Memoirs of the Queensland Museum, Vol. IV.
Horsley, Sir Victor, F.R.S. M.B. B.S.—"Alcohol and the Human Body."
Jefferson Physical Laboratory—Harvard University Contributions, Vol. XI. 1913-14. 8vo. 1915.
Kyoto Imperial University—Memoirs: College of Science, Vol. I. Nos. 3, 4, 5. 8vo. 1915.
Life-Boat Institution, Royal National—Journal for Nov. 8vo. 1915.
Linnean Society—Proceedings, 127th Session. 8vo. 1915.
List of Fellows, 1915-16. 8vo. 1915.

London County Council—Gazette for Nov. 1915. 4to.
Indication of Houses of Historical Interest in London, Part 44.
Meteorological Office—Monthly Weather Reports for Sept.-Oct. 1915. 4to.
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Daily Readings for Sept. 1915. 4to.
Milan R. Scuala di Agricultura—Annuario della Instituzione Agraria. Vol. XII. 8vo. 1915.
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Parke, Davis & Co.—Collected Papers from the Research Laboratory, Detroit, Mich., Reprints, Vol. III. 1915. 8vo.
Photographic Society, Royal—Journal, Vol. LV. No. 7. 8vo. 1915.
Post Office Electrical Engineers, Institution of—Wireless Call Devices. By L. B. Turner, A.M.I.E.E.
An Introduction to the Theory of Illumination. By L. B. W. Jolley, B.A., A.M.Inst.C.E.
Pusa, Agricultural Research Institute—Journal of India (Quarterly), Vol. X. Part 4, Oct. 1915.
Ralli, Mrs. Stephen, M.R.I.—The Political Life of M. Tricoupi, 1894-5.
Royal Colonial Institute—United Empire, Vol. IV. No. 11. 8vo. 1915.
Royal Society of Arts—Journal for Nov. 1915. 8vo.
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Sanitary Institute, Royal—Journal, Vol. XXXVI. No. 11. 8vo. 1915.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXI. Nos. 9-11. 8vo. 1915.
Societa degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. IV. Disp. 9*a*. 4to. 1915.
Society for Promoting Christian Knowledge—A Voyage in Space. By Professor H. H. Turner, D.Sc. D.C.L. F.R.S.
South Australian School of Mines and Industries—Annual Report for 1914, and Prospectus for 1915.
Squier, Lieut.-Col. George O., Ph.D. M.R.I.—On an Unbroken Alternating Current for Cable Telegraphy.
United Service Institution, Royal—Journal for Nov. 1915. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. V. Nos. 5-7, 1915. 8vo.
Experiment Station Record, Vol. XXXIII. No. 5, Abstract No. 6. 8vo. 1915.
Victoria Institute—Journal, Vol. XLVII. 8vo. 1915.
Washington, National Academy of Sciences—Proceedings, Vol. I. No. 11. 8vo. 1915.

WEEKLY EVENING MEETING,

Friday, April 30, 1915.

His Grace the Duke of Northumberland, P.C. K.G. D.C.L. F.R.S., President, in the Chair.

Professor F. G. Donnan, F.R.S. F.G.S. *M.R.I.*

Emulsions and Emulsification.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, January 22, 1915.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR SIR JAMES DEWAR, LL.D. D.Sc. F.R.S. *M.R.I.*,
Fullerian Professor of Chemistry, R.I.

Problems of Hydrogen and the Rare Gases.

IN the course of the Friday Evening Discourse of last year on "The Coming of Age of the Vacuum Flask" attention was drawn to some low temperature determinations of the amount and distribution of hydrogen, helium and neon in gases from various sources. This work has now been extended by the use of improved apparatus and methods of manipulation. To measure with accuracy the presence of a few parts per million of one or other gases, either by volume or weight, diluted to such a large proportion in another, is not an easy problem. What a millionth part represents is shown in the model on the table. The framework encloses a cubicle space one metre in the side, in the centre of which a cube of one centimetre in the side is suspended. The relative volumes of these cubes are as 1 to 1,000,000. When it is stated that the air contains less than one part in a million of hydrogen, this means less than one cubic centimetre per cubic metre. The values given last year were derived from measurements of the partial pressure observed in a McLeod gauge, after the gases had been cooled to 20° Abs. by the use of liquid hydrogen from an initial pressure of 760 mm., or one atmosphere. The unit of pressure adopted to express the recorded results was 10^{-4} ($\frac{1}{10000}$) mm. mercury pressure, and that mode of expressing the experimental results will be continued in this lecture.

COMPARATIVE CONDENSABILITY OF GASES BY CHARCOAL AT LIQUID AIR AND LIQUID HYDROGEN TEMPERATURES.

The relative condensability by charcoal of the gases, air, hydrogen, and helium, can be shown by filling three similar sets of double tubes with samples of these gases. The two tubes of each set are filled with the particular gas at atmospheric pressure and the ordinary temperature, and dip into a little bottle of mercury. The tubes are bent twice at right angles and sealed up. Thus the closed end of either tube of each set can be cooled by immersion in a vacuum vessel containing liquid air or hydrogen. A gramme of charcoal is placed in the sealed end of one tube in each pair. For convenience of pressure observations, a barometric tube is placed alongside each pair of tubes in the three separate little bottles of mercury.

When the closed end of the air tube is cooled in liquid air, only a small contraction is observed, as shown by a slight rise of the mercury in the tube. If, however, the air tube which contains the charcoal is cooled all the air is thereby condensed, and the mercury quickly rises to the barometric height. Now take the pair of hydrogen tubes. As before, on cooling the tube without charcoal, only a slight contraction is shown, but when the charcoal tube is immersed in the liquid air, a quantity of the hydrogen is condensed in the charcoal, and the mercury rises in the tube. The height the mercury attains, however, is noticeably less than in the air-charcoal tube, showing the smaller condensability of hydrogen in the charcoal at liquid air temperature. When either of the helium tubes are cooled with liquid air, practically no condensation is shown.

Now instead of liquid air, let us use liquid hydrogen, and cool first the air tube. Immediately the mercury rushes up to the barometric height, all the air being condensed into a solid of inappreciable tension of vapour, and the charcoal tube behaves in the same way. Now pass to the hydrogen tubes. When the plain gas tube is cooled, quite a noticeable contraction is visible, because the temperature of liquid hydrogen is so low compared to hydrogen gas at the room temperature. Complete absorption is produced when the charcoal-hydrogen tube is cooled and the mercury rises to the barometric height. The hydrogen, at the temperature of its own boiling point, is completely absorbed in the charcoal.

Now compare this with the set containing helium. The helium tube when cooled behaves similarly to the plain hydrogen tube, but on cooling the charcoal tube, quite a large diminution of pressure is produced, showing that even helium is condensed to a considerable extent by charcoal at 20° absolute. As the charcoals warm up, the condensed gases are again expelled; the helium very rapidly, the hydrogen somewhat more slowly, and the air after some few minutes.

APPARATUS EMPLOYED FOR MEASUREMENTS OF HYDROGEN AND THE RARE GASES UNCONDENSED AT THE BOILING POINT OF HYDROGEN. (Plate 1.)

In order to avoid indiarubber connections, vitiating the accuracy of the values of minute quantities of gas, the gas to be examined was contained in a flask A of suitable size, attached by a coned ground joint T. The gas was allowed to enter the exhausted apparatus by opening the cock B, which also served to connect to the pump for the preliminary exhausting of the whole apparatus. To remove hydrogen and hydrocarbons the part between B and the measuring burette C contained a roll of oxidised copper gauze tightly filling about 8 cm. of the centre of the tube. For safely heating this a thin tube of iron about 5 mm. greater in diameter was secured in position by asbestos wool packed in at the ends. A bent sheet of asbestos served

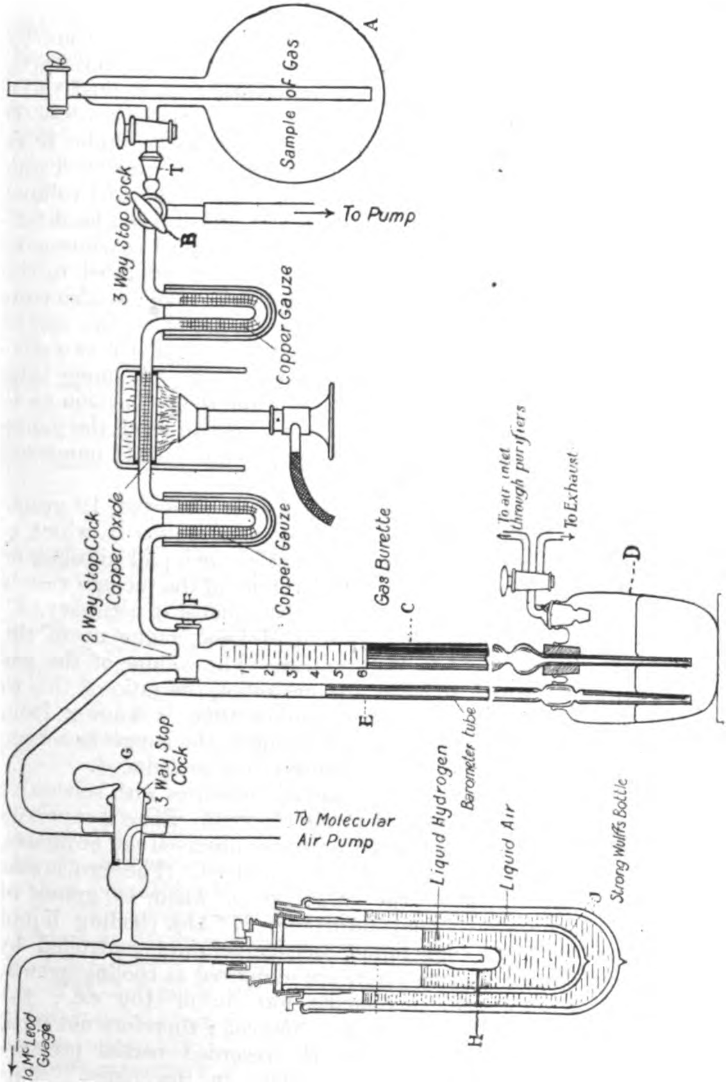
A
Sample of Gas
T
3 Way Stop Cock
B
To Pump
Copper Gauze
Copper Gauze
Copper Oxide
2 Way Stop Cock
F
Gas Burette
C
1
2
3
4
5
6
E
Barometer tube
To air inlet through purifiers
To Exhaust
D
G
3 Way Stop Cock
To Molecular Air Pump
Liquid Hydrogen
Liquid Air
Strong Wulff's Bottle
J
H
To McLeod Guage

PLATE 1.

to further unify the heating, which was supplied by a small fan-shaped bunsen flame, just not luminous. On either side of the oxidised copper the tube is bent into a **U** shape for cooling in liquid air, to condense any volatile hydrocarbons from the stopcock and the oxidised products from the action of the hot CuO on any combustible material in the gas sample.

The exact measurement of samples small in volume is made by lowering the pressure to any convenient amount in the burette C, in which the gas collects during the purification by heated CuO. The burette opens below under the surface of mercury in a vessel D with a connection to the exhaust pump; an exhausted tube E as barometer also opens into the mercury in the same vessel. From the difference of levels of the mercury in C and E the real volume of the gas sample at normal temperature and pressure can be determined. After measuring the gas, the cock F is turned to connect to the main stopcock G, by which the gas is cautiously admitted to the cooling bulb H, and through this to the McLeod gauge; G also connects by another way to the molecular pump, by which the cooled products can be highly exhausted. The bulb H is formed of two concentric tubes $1\frac{1}{2}$ cm. and $\frac{1}{2}$ cm. diameter respectively. The inner tube is continued up to connect to the gauge, while the connection to G is on the annular space; by this means no gas can reach the gauge until it has passed through the cooled part of the bulb H immersed in the liquid hydrogen.

When the charcoal and liquid air method is employed, 10 grams of charcoal are placed in an enlarged part of the tube H, which is immersed in the liquid hydrogen. Liquid air or liquid nitrogen or oxygen replaces the liquid hydrogen in the inner of the vacuum vessels J, the outer still containing liquid air, under exhaust if necessary.

The volume of the cooling bulb and McLeod gauge up to the main stopcock G was carefully determined. The volume of the gas sample admitted through G being also measured, the ratio of this to the total volume of the gauge and cooling tube is known, from which the partial pressure which would result if the gauge had been filled with the gas at atmospheric pressure could be deduced.

This is given by observing the partial pressures with successive increasing volumes of gas occluded in the charcoal. Plate 2 expresses the results graphically. The partial pressures observed are ordinates, while the abscissæ are the gas volumes admitted. The two lowest graphs, Nos. 1 and 2, show the values given when 10 grams of charcoal at the respective temperatures of 77° Abs. (boiling liquid nitrogen) and 86° Abs. (old liquid air, temperature controlled by oxygen vapour tension thermometer) are employed as cooling agents. The volume of successive increments was 50 or 100 c.c.; the volume of the apparatus was just over 350 c.c.; therefore not until this volume had been admitted does the recorded partial pressure correspond to 760 mm., or one atmosphere, in the gauge. This is therefore so marked on the diagram, and the ordinate at this point can be scaled in parts per million volumes, the unit for this scale being equal to $7 \cdot 6 \times 10^{-4}$ mm. mercury pressure, which is one millionth of an atmosphere. The rise of pressure is at first linear in both cases, but after the admission of 300 c.c. in the second case the graph begins to bend upwards, and the rate of this bending becomes greater with further increments, showing that the partial pressure of those constituents condensed by the charcoal, which up to now had been low, here begins to be appreciable as the concentration in the charcoal increases. The place at which this bending begins depends on the weight of charcoal employed and the mode in which it has been prepared. The two upper graphs, Nos. 3 and 4, are perfectly linear, which is characteristic of the results obtained when liquid hydrogen is employed as condensing agent alone. No. 3 deals with ordinary air, and is thus comparable to the charcoal and liquid air results just discussed. No. 4 shows the values obtained when the oxygen is removed from the air, leaving only "air nitrogen." The uncondensable portion is now relatively higher in the same volume, which should be the case if it remains unaffected by the oxygen absorption.

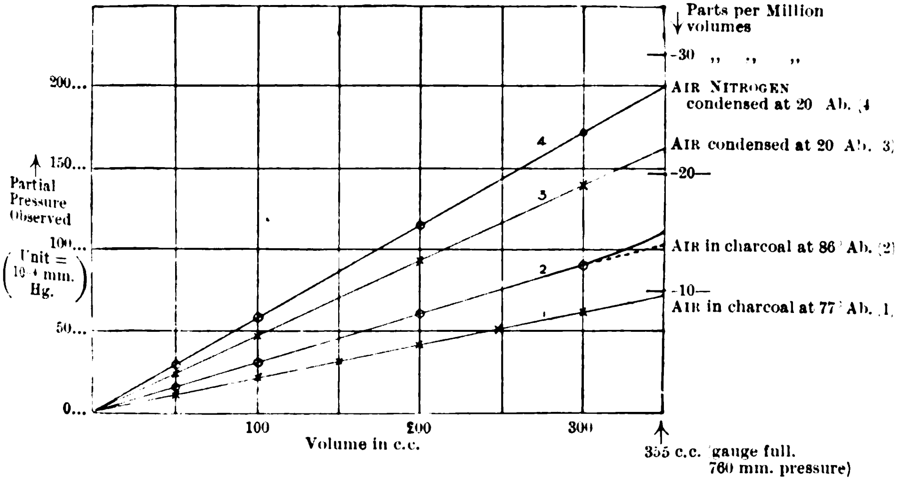

PLATE 2.—PROPORTIONS OF HELIUM AND HYDROGEN IN AIR. Comparison of the Use of Charcoal at Liquid Air Temperature with that of Liquid Hydrogen.

The difference in the values recorded in No. 3 from those in Nos. 1 and 2 represents the amount of condensation of hydrogen and neon in the charcoal. This is confirmed by its being greater at the lower temperature of boiling nitrogen, No. 1, which lies below No. 2

obtained with liquid air, some 9 degrees higher. The approximation of No. 2 to No. 3 as the higher concentrations are reached is due to the rapidly increasing tension of neon and hydrogen in charcoal containing relatively large amounts of occluded nitrogen and oxygen. The disturbance in the equilibrium of the condensed constituents under these conditions is beginning to be appreciable. To illustrate the problems resulting from such a condition the following experiment is instructive: Two similar U-shaped bulbs charged with 10 grms. of charcoal and provided with inlet and outlet stopcocks are cooled in liquid air and opened to the atmosphere of the room. One is simply allowed to saturate itself from the air, while the second has a slow current of dry air passed through it for some time after saturation. This having been done, both bulbs are warmed up by immersion in tepid water, and the gases so expelled are roughly tested by inserting a glowing splint of cedar-wood; the gas issuing from the first bulb at once extinguishes the splint, whereas that coming from the second excites a brilliant combustion. Nitrogen is given off from the bulb which has been saturated by atmospheric air; whereas oxygen principally results after the air current to which the second charcoal was subjected. In the latter case the less volatile and more condensable oxygen has accumulated in the charcoal to a greater extent to that of nitrogen. The slow current of dry air has provided the opportunity for the equilibrium of the two gases occluded in the charcoal to be established; but when air rushes in at atmospheric pressure, filling the pores of the charcoal in a small limited space, the proportions of each gas present in the charcoal must have substantially the composition of the ordinary air. Thus, on warming up each of these samples of charcoal which have been treated as above described, it is to be anticipated that the one sample of charcoal would give gas containing excessive nitrogen and the other excessive oxygen.

The alteration of the proportions of helium, neon and hydrogen in air when it has been equilibriated in charcoal at liquid air temperature is given in the following table:—

Alteration of the Proportions of Helium, Neon and Hydrogen in Occluded Air in Charcoal at Liquid Air Temperature. (Plate 3.)

Material	Parts per Million Volumes	
	Helium and Neon	Hydrogen
City air	22·5	10·6
Country air	22·8	0·5
Gas occluded in charcoal after air current at 85° Abs.	1·64	6·1
Above repeated with compressed air from steel bottles	1·53	9·8

GAUTIER'S OBSERVATIONS OF 1901.

	Hydrogen
City air	200
Mountain air	170
Sea air	190

Charcoal has less condensing power for the rare gases than for nitrogen ; so that if the proportion of nitrogen in the charcoal gases after a long air current at 85° Abs. be lower than in ordinary air, we should expect the proportion of the rare gases to be still more greatly diminished. The values observed prove that this is so ; only 1·64 parts per million of the rare gases helium and neon being present, instead of the 22·5 parts in ordinary air, and the total of the helium, neon and hydrogen is only 7·7, instead of 25·1. Although the proportion of hydrogen is exceedingly small in air, yet it is quite appreciably condensed by charcoal at liquid air temperature, having been increased from 1·6 to 6·1 by such treatment. Its proportion has, in fact, been increased ten-fold, while that of the helium and neon has been reduced to one-fourteenth, due to the charcoal having practically little power of absorption for these gases at liquid air temperatures.

In a second experiment the air with which the charcoal was treated was taken from a steel bottle in which it had been first compressed : the amount of hydrogen in the evaporated product was then higher by one-half—9·8 instead of 6·1—although the neon and helium value was not sensibly different. The higher proportion of hydrogen is explicable by the chemical action of water on the iron storage receptacle. Gautier, in 1901, employed metallic tubing in his apparatus for measuring the amount of hydrogen present in air, with the result that he obtained a value as high as 200 parts per million with city air, the lowest amount being 170 in mountain air. This must have arisen from the chemical action on the metallic tubing producing hydrogen. On the subject of the relative distribution of the atmospheric rare gases in thermal springs and air, Moureu, in 1913, found that the ratio of any two of the gases present in air to the same pair in the gas from thermal springs is roughly constant except where helium is concerned. The table on the following page epitomises his results. Krypton and xenon, as well as the more common argon, are less volatile than the ordinary atmospheric gases oxygen and nitrogen, whereas helium is the most volatile known gas.

MOUREU'S EXPERIMENTS (1913).

COMPARISON OF DISTRIBUTION OF RARE GASES IN AIR AND IN EARTH GASES.

Gases from Thermal Springs and Fire Damp		Air	Ratio
$\frac{\text{Krypton}}{\text{Argon}}$	divided by	$\frac{\text{Krypton}}{\text{Argon}}$	Roughly constant
$\frac{\text{Xenon}}{\text{Argon}}$	,,	$\frac{\text{Xenon}}{\text{Argon}}$	,,
$\frac{\text{Xenon}}{\text{Krypton}}$	,,	$\frac{\text{Xenon}}{\text{Krypton}}$	,,
$\frac{\text{Helium}}{\text{Argon}}$	,,	$\frac{\text{Helium}}{\text{Argon}}$	Varies enormously

The measures of such small quantities of gas, as are here being discussed, will, of course, be liable to errors from many causes, and the conditions of experimenting must therefore be modified in the search for possible variations. For this purpose the application of a high exhaust, such as that obtained by the use of the molecular pump, in conjunction with the cooling methods already described, has yielded some suggestive information.

The manner in which such experiments can be carried out can be seen by referring back to Plate 1. The main stopcock B on the cooling bulb H there shown, besides the connection to the burette, can be turned on to the high exhaust of the molecular pump. By this means both the condensed products in the bulb H and the uncondensable residue in the gauge are subjected to an exhaust of below 0·00001 mm.; which is one-tenth of the unit of pressure employed to express the results; and approximately one two-thousandth part of the partial pressure in the gauge of the uncondensable fraction in air. The readings then obtained are shown in the next table:—

PLATE 4.—OCCLUSION AT 20° ABS. OF THE UNCONDENSABLE FROM AIR IN THE CONDENSED SOLID NITROGEN AND OXYGEN.

Material used	Partial Pressures (in 10^{-4} mm.) left from 760 mm.		
	Cooling to 20° Abs.	Exhaust of Molecular Pump for 10 min. at 20° Abs.	Raised to 15° C.: Recondensed at 20° Abs.
			(Amounts occluded)
Air	181	0·11	4·6
Air nitrogen . . .	218	0·075 (1)	1·5 (1)
		0·08 (2)	0·31 (2)
		0·075 (3)	0·10 (3)
		0·07 (4)	0·11 (4)

Air and air nitrogen were the materials examined, as stated in the first column. As oxygen is absent in the second case, some idea of the effect of its presence can thus be obtained. The second column gives the partial pressure of the residue of rare gases uncondensed at 20° Abs. The next column shows the reduced pressure in the apparatus when the exhaust pump was connected, and continued in action for over 10 minutes, the uncondensable residue being thereby removed down to the limit which the pump can maintain under the circumstances. The exhaust was then closed off, and the condensed material allowed to evaporate off into the apparatus, by lowering the liquid hydrogen vessel away from the bulb H until everything was at room temperature. The bulb was then again cooled by cautiously and slowly raising the vessel of liquid hydrogen to its former position; the steady pressure then obtained is recorded in the last column. This value is seen to be higher than that left by the pump exhaust. By repeating the operation a state can be reached when only a very small increase is registered, as shown when the air nitrogen was examined.

This means that even at the low pressure maintained by the pump some helium or neon or hydrogen has been held bound, or dissolved in the solid condensed nitrogen and oxygen; so that the value obtained by simple condensation to 20° Abs. is low by at least this amount, though no doubt the amount of occlusion at the higher initial observed pressure of the uncondensable is even greater than that at the lower pressure of the pump; the nature of this occluded material remains to be seen. Having regard to the relative volatility of hydrogen, neon and helium, one might expect that hydrogen would most likely be present in any gas remaining occluded under high exhaust in solid nitrogen and oxygen at 20° Abs. Experiment, however, seems to suggest that the very opposite is the case. To examine this question, mixtures of hydrogen with nitrogen and carbonic oxide respectively were employed; the table on the following page gives the results.

The first column shows the gases used in the experiments. In the second column is given the partial pressure of each constituent after admission to bulb H and the McLeod gauge of Plate 1. Following this is recorded the actual pressure registered on cooling the bulb H in liquid hydrogen; and in the next column the apparent occlusion before exhausting, being the difference between the partial pressure of the hydrogen in the case of the mixtures shown in column 2 and the reading of column 3, which is the pressure observed. At this point, following the former procedure, the molecular pump exhaust was connected for 15 minutes; then, as before, the condensed material remaining was evaporated off into the gauge by removing the liquid hydrogen. But before re-cooling to determine any uncondensable residue due to occlusion at the high exhaust, which would be a completion of the previous experiment, a

PLATE 5.—EFFECT OF PRESENCE OF HYDROGEN ON THE VOLATILITY AT 20° ABS. OF NITROGEN AND CARBONIC OXIDE.

Gas and Gas Mixtures Examined	Pressure (in 10^{-4} mm.)			
	Gas at 15° C.	Cooling to 20° Abs.	Apparent Occlusion (before Exhaust)	Raised to 15° C. after 10 min. High Exhaust at 20° Abs.
Air nitrogen .	16920 (0·82 c.c.)	0·47	—	16920 (all left)
Carbonic oxide .	28800 (1·39 c.c.)	40 (0·14% hydrogen)	—	28570 (99% left)
Nitrogen, 20% Hydrogen, 80%	5190 (0·25 c.c.) 20760 (1·00 c.c.)	16440	4320 (16·7%)	0·44 (i.e. none left)
Carbonic Oxide, 20% Hydrogen, 80%	2390 (0·116 c.c.) 9560 (0·464 c.c.)	7600	1960 (9·7%)	0·12 (i.e. none left)

measure of the pressure was taken to determine the amount of the evaporated material obtained, with the surprising result, in the case of the hydrogen mixtures, that there was nothing practically to measure. Under the high exhaust of the pump, therefore, not only had the uncondensable hydrogen been removed, but also the condensable constituent; nitrogen, or carbonic oxide, had suffered a forced volatility in presence of the hydrogen, although naturally without admixture of hydrogen, the nitrogen or carbonic oxide was regained unaltered. In view of this, it seems difficult to resist the conclusion that some sort of molecular aggregation had taken place at the very low temperature, between hydrogen and the carbonic oxide and nitrogen respectively.

The composition of the gases in this experiment is of course very different from that dealt with in determining the small proportion of air uncondensable. There is, therefore, only a limited application to that case. Thus the amount of condensed nitrogen employed is only one-fourth of the total amount of hydrogen, whereas in the experiment with ordinary air there is five hundred thousand times more nitrogen than hydrogen; the amount of "forced volatility" will therefore probably be so small as to escape measurement, as the difference to be detected would have to be measured on a total of one atmosphere in the gauge. Further, it is possible to make a correct estimate of the amount of occlusion occurring on condensation to 20° Abs. in the case shown in the last table, because the actual composition of the mixture is known; whereas in the former case of examination of air uncondensable, this is uncertain, the only data being the pressure remaining after exhaust and recondensation. But there is evidently ample need for enquiry under different intermediate conditions in order to throw more light on what appear to be new conditions of relative volatility at low temperatures.

Diffusion through Platinum and other Metals at High Temperatures.

The diffusion of hydrogen through a heated platinum tube can easily be demonstrated. For this purpose a platinum tube A (Plate 6) of ½ mm. bore and ½ mm. thickness of wall is fused up at one end, while the open end is sealed into one limb of a glass manometer B charged with sulphuric acid; the platinum tube is reduced to about 0·2 mm. thickness of wall for sealing into the glass. It is further supported by an external glass tube (not shown in the figure) fitting tight on to the horizontal extension of the manometer, and drawn out to afford a good sliding fit on about 5 cm. of the platinum tube. The left-hand limb of the manometer connects to a stopcock and charcoal bulb for exhausting between successive measurements; to facilitate this, the two limbs of the manometer are connected by another stopcock C, which is closed when a measure is to be made. For providing atmospheres of different gases round the platinum tube, a quartz tube D of special

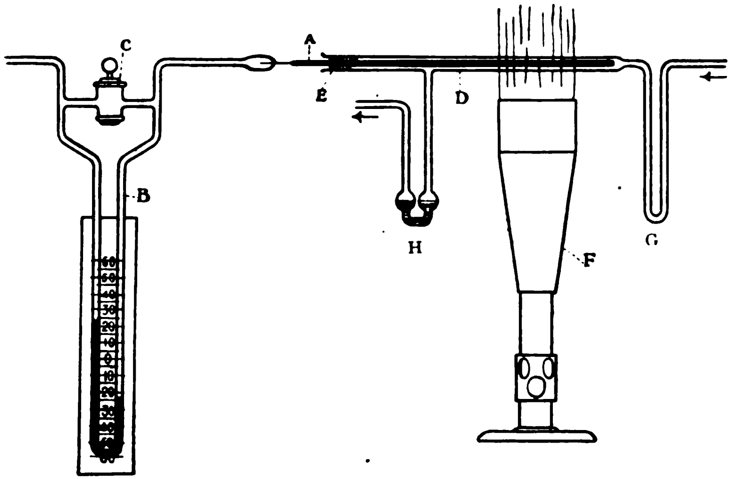

Plate 6.

shape is slipped over the platinum tube, which it loosely fits, and to which it is luted at the plain open end nearest the sealed joint by a short plug of asbestos wool tightly packed when wet. The purification of the gas entering the quartz tube, and thus passing through the annular space round the platinum tube, is provided for by a plain **U**-bend G, which is cooled in a vessel of liquid air or solid carbonic acid. A second bulbed **U**-tube containing a little mercury or sulphuric acid is sealed in near the open end of the quartz cover tube, and this acts as a seal and bubbler H for controlling the gas rate. The tubes are conveniently heated by a Méker burner F.

Plate 7 shows the observed increase in the pressure when the platinum tube is heated directly in the flame in three different positions. The rate of growth of the pressure and the limiting pressure reached are seen to be dependent on the region of the flame to which the platinum tube is adjusted; when near the bottom, where the amount of unburnt hydrogen is greatest, a rapid rise is shown and the equilibrium pressure obtained is high, both values becoming less as the platinum tube is brought higher in the flame. Finally, at the tip of the flame where combustion is complete, reversal can be demonstrated, and the manometer again falls in the manner shown in the diagram; for the hydrogen will diffuse out as well as in, the direction depending on whether the region of lowest pressure is outside or inside. The position of equilibrium of the

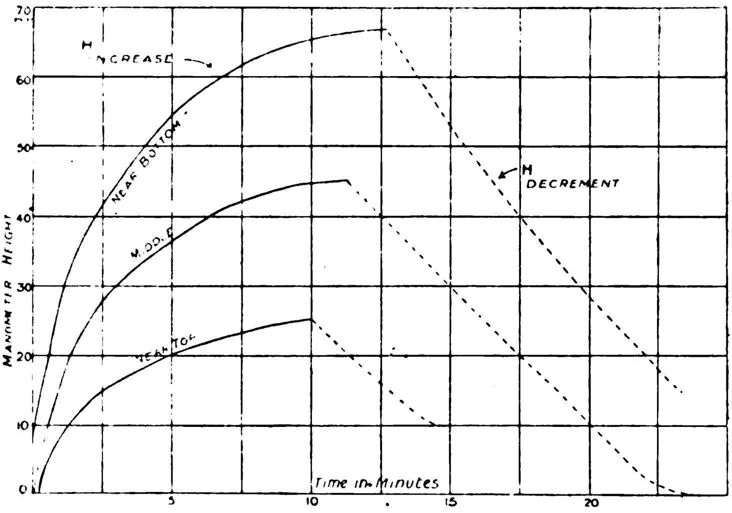

PLATE 7.—GROWTH OF PRESSURES IN PLATINUM TUBE HEATED BY MÉKER GAS BURNER.

manometer, given by any part of the flame, records the partial pressure of the diffusing hydrogen for that region; this is easily shown in the middle of the flame. In the same way a pure hydrogen flame can be employed. The temperature of the flame is now not so high, but can be increased by the introduction of a little oxygen, using a simple form of quartz blowpipe. The oxygen introduced passes through an annular tube round the hydrogen jet, whereby the temperature of the tube is sufficiently raised by conduction from the outside intense part of the flame, while leaving plenty of available free hydrogen to diffuse from the central part. The manometer rises then to almost any amount we please. An actual rate of this diffusion has been measured by observing the growth of pressure in an exhausted McLeod gauge of known volume to which the platinum tube and attached U-tube was sealed. With the platinum near a white heat the rate was found to be of the order of 3 c.c. an hour per square centimetre through a mean thickness of half a mm. When thin platinum foil was employed in a different arrangement about twenty times this rate was observed at a rather lower temperature.

If instead of the flame impinging directly on the platinum tube we introduce the quartz tube cover shown in Plate 6, allowing dry air or oxygen to pass through the annular space, the manometer remains steady, whereas, however, on passing a current of hydrogen through the annular space the pressure instantly increases and quickly reaches a limit.

On replacing the platinum tube by a palladium one, similar effects result, but at a greater rate. Now, however, the pressure disappears when the flame is removed, as the palladium on cooling absorbs the hydrogen; but not so fast as when the tip of the flame is used for heating to show the diffusion of the hydrogen out through the hot tube. An iron tube similarly employed shows much smaller effects; but on the other hand this is the only tube of the three to give any indications of gas diffusion when heated by a flame of carbonic oxide raised in temperature by the addition of oxygen.

An even more effective and ready way to demonstrate a succession of these effects is by using an electric discharge tube to replace the manometer; the arrangement of the apparatus is seen in the next figure (Plate 8).

The platinum tube in this case is also provided when necessary with an external quartz tube for leading different gases over the heated platinum. For examining the electrical discharge and spectrum of the diffusing gases, the discharge tube B is provided with external electrodes, formed by silvering 3 cm. length near each end, protected by thin copper foil bound on with wire. Exhaust is provided as usual by a charcoal bulb C with a stopcock F for putting the exhaust off and on as required. B is protected at each end with U-tubes D and E, as liquid air traps for volatile materials, either diffusing in or coming from the stopcock. The volume is kept small by using tubing of about 2 mm. bore. By this means the diffused pressure attains in a very short time a sufficient value to allow a discharge to pass.

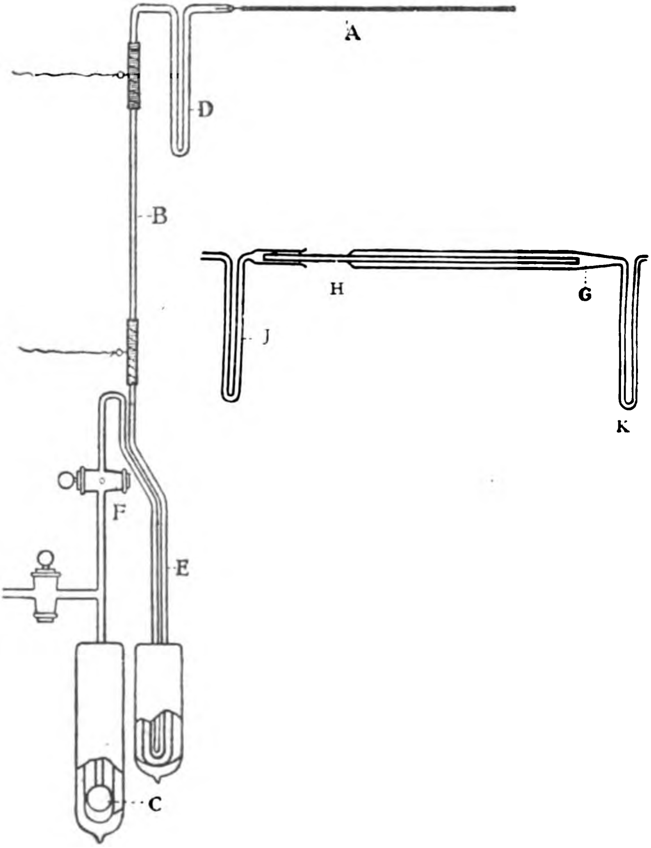

PLATE 8.

The apparatus being highly exhausted, as shown by the absence of any discharge when the induction coil is put on, the naked flame of the Méker burner is applied to the platinum tube. Within a very few seconds of closing off the charcoal stopcock a lilac-coloured discharge begins in the tube and rapidly intensifies and becomes deep pink, characteristic of hydrogen, and on examining with the spectroscope a well-developed spectrum of this gas is seen. Now the heating is repeated, after exhausting by the charcoal, but with the quartz tube containing air covering the platinum tube. This time no discharge will pass even after several minutes of heating as before. Other gases can then be passed through the annular space between the quartz tube and the platinum. Neither oxygen nor carbonic acid will show any diffusion under such circumstances, although at a somewhat higher temperature, obtained in some cases by passing a strong electric current through the platinum tube instead of using flame temperatures, there are indications that both carbonic acid and water vapour will pass through, and possibly also carbonic oxide, a gas which, as we have seen, readily passes through hot iron. Finally, when hydrogen is passed through the quartz tube a strong discharge is rapidly obtained.

If a thoroughly cleaned quartz tube is subjected to direct heating by the Méker flame, there is no certain indication of any gas transference. If the quartz tube, however, is attached to a McLeod gauge, instead of to the electric discharge tube, and the heating continued for a considerable time, then a definite increase of pressure can be detected. On the addition of the external quartz tube, as was done with the platinum tube, the arrangement can be made more convenient, as the two quartz tubes can be sealed together to form a double tube in one piece, as shown in Fig. 5. This arrangement further enables the annular space between the tubes to be well exhausted or filled to any required pressure with different gases. If hydrogen be admitted and the tubes strongly heated, a discharge can only be obtained after a long time, the rate of diffusion being very slow.

When helium and neon replace the hydrogen in the annular space, then a discharge is readily obtained. The helium appears more quickly than the neon, the discharge being sky-blue at first and steadily modifying to the well-known peach-blossom tint of helium and neon mixtures, and becoming a brilliant orange-pink as the neon intensifies. This affords an effective and ready manner of demonstrating the transference of these gases through hot quartz. The annular space was filled to about four-fifths of an atmosphere at ordinary temperature ; a good Méker flame is a sufficient source of heat ; the inner quartz tube is approximately ½ mm. thick in the walls and about 1½ mm. bore. To prevent much loss by diffusion into the air through the outer tube the quartz tube ought to be thick in the wall. After charging the annular space the helium and neon gasholder is sealed off. The measure of the rate of transference of helium and neon by the use of a McLeod gauge has not yet been made.

DIFFUSION THROUGH INDIARUBBER.

A widely different example of a diffusing medium is afforded by indiarubber stretched into thin membranes. To enable them to stand the necessary difference of pressure on the two sides they are supported on discs of copper gauze mounted in a shallow metal drum of about 4 cm. diameter; the drum A, Plate 9, is closed at one end by a brass plate pierced to connect to a lead tube E, of about 2 mm. bore and 8 mm. diameter. Connection is made to the manometer by a ground coned joint B; a brass cone fitting this joint is soldered on the end of the lead tube. The indiarubber membrane is stretched over the open end of the drum and rests on the copper gauze disc inside, which is raised a few millimetres above the brass plate forming the closed end of the drum. Tightly wound thread is used to bind on the membrane, a little melted indiarubber as luting being first smeared thinly on the brass. To enable different gases to be used, and their temperature and pressure to be varied, as applied to the indiarubber membrane, a gas-tight metal box is fitted on (this is not shown in the diagram).

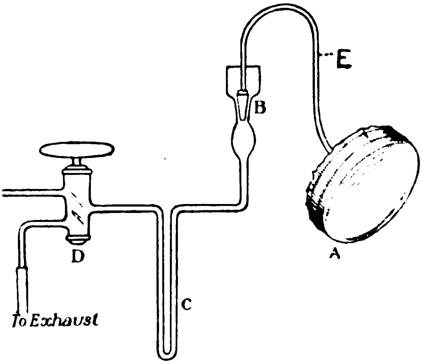

PLATE 9.

For demonstrating diffusion through the membrane a manometer is employed of the same pattern as that shown in Plate 6; the index tube is charged with very high boiling-point mineral oil instead of sulphuric acid. In order to dry or purify the diffusing gas a plain U-tube C (Plate 9) is sealed to the coned joint connecting to the mounted membrane. Beyond the U-tube is a two-way stopcock D, whose upper limb is sealed to the manometer, while the lower connects to an exhaust pump for the preliminary exhaust of the space between D and the indiarubber.

Everything being exhausted, the **U**-tube C is cooled in liquid air, while the ordinary air of the room is present in the gas-box round the indiarubber. In half-a-minute a difference of pressure of over 10 mm. is registered on the manometer. This, then, represents the rate due to pure dry air diffusing in through the membrane and through the cooled **U**-tube to the manometer.

If C is not cooled, and A is simply surrounded with a beaker lined with wet paper, then a pressure of 30–40 mm. in half-a-minute is seen, the difference being due to the added rate of the aqueous vapour Other vapours can be similarly tried by dropping a small quantity of different liquids into beakers placed in turn round the mounted membrane. Thus with ether, chloroform and ammonia the manometer gives exceedingly rapid indications, about 5 cm. or more being registered in a few seconds. Less active vapours are benzene, alcohol, pyridine and acetonitrile. These are a few illustrations of what is no doubt a large field for future enquiry. A viscous liquid like glycerin stops all diffusion when painted on a dry film. The solution, however, in glycerin of substances such as chloroform, etc., causes the manometer to act immediately.

An interesting variation applicable to the subject of the discourse is the passage over the membrane of respired air dried through a tube in boiling solid carbonic acid (– 80° C.). The 5 per cent or so of carbonic acid therein contained gives an appreciable addition to the rise of the manometer shown by dry air alone. If the breath be "held" for a half-minute or so before expiring, the difference is accentuated. When dry oxygen, hydrogen, and carbonic acid respectively are passed through the box over the membrane, the corresponding pressures recorded are 10, 40, and over 100 mm. respectively in the half-minute. Hydrogen therefore goes through, roughly, at four times the rate of air, and carbonic acid at least ten times as fast.

More careful measures made with a McLeod gauge showed that for the same membrane the rate expressed in c.c. per day through each square centimetre at the ordinary temperature was 2·12 for air, 8·4 for hydrogen, and 30·0 for carbonic acid. Different films give different values within certain limits, according to their condition and thickness, which last is of the order of 0·01 mm. The rate of diffusion at different temperatures is readily determined by immersing the gas-box containing the indiarubber membrane in a fluid the temperature of which can be properly regulated. The variation of rate with temperature is found to be logarithmic, with a definite break in the curve at 0° C., and also at other points below this in the case of hydrogen and carbonic oxide; as far as experiments have been carried out. Helium has a slightly greater rate than air, while carbonic oxide and nitrogen are not far removed. Some of these curves intersect at lower temperatures. When the indiarubber membrane is immersed in liquid air, the diffusion is too small to be seen on the oil manometer; the actual rate as determined on the McLeod gauge being only about 0·002 or 0·003 c.c. per day through each square centimetre. These observations all refer to a difference of pressure of one atmosphere on the two sides of the membrane. The discussion of the results got by the action of varying pressures and temperatures must be deferred to a future occasion.]

[J. D.]

Mr. W. J. Green, B.Sc., of the Davy Faraday Laboratory, has given valuable assistance in the conduct of the Investigation; and Mr. J. W. Heath, F.C.S., in the Lecture arrangements.

WEEKLY EVENING MEETING,

Friday, January 28, 1916.

HIS GRACE THE DUKE OF NORTHUMBERLAND, P.C. K.G. D.C.L. F.R.S., President, in the Chair.

LEONARD HILL, ESQ. M.B. F.R.S.

The Science of Clothing and the Prevention of Trench Feet.

THE lecturer began by contrasting our well clad and housed people with the Terra del Fuegians described by Darwin as enduring naked a climate worse than ours. "Nature, by making habit omnipotent and its effects hereditary," has fitted the Fuegians to the climate. Our people nowadays generally over-clothe themselves, and weaken the defensive mechanism of the body against cold by over-coddling. The Channel swimmer, robust and well lined with the natural coat of cutaneous fat, and thickly smeared with grease to prevent maceration of the skin, has endured the sea-water as long as 23 hours, and yet water has 240 times the conductivity of air and over 3000 times its capacity for taking up heat. The adaptability of body habit, on the one hand to exposure, on the other to over-clothing and hot atmospheres, is enormous. The doctrine so universally believed that cold is the cause of all our ills is erroneous and pernicious. The vastly improved health, vigour and manhood of our conscripts, taken from desk, shop or factory and put to hard exercise in the open air, shows how much ill-health and deficient vigour arises from sedentary confined occupations. No less striking is the effect on children of open-air schools, which should be made universal. Cerebro-spinal fever and similar respiratory infections (common colds, etc.) are spread by close contact of the carriers of disease with others in confined places. Those living an open-air life with great exposure to cold do not suffer from these epidemics. Exposure to cool moving air tones up the body through the cutaneous nerves and leads to the taking of muscular exercise to keep up the body-heat: thus metabolism is maintained at a high level, appetite and digestion kept vigorous, the breathing deepened, the circulation of the blood invigorated, and the abdominal organs massaged by the deep breathing and the muscular exercise—this is most essential to the circulation and well-being of their function. The sedentary confined worker, housed in tenements, suffers the atrophy of disuse, and misses the enjoyment of life that comes from perfect physical health—the wild animal's heritage. With appetite impaired, he is a prey to minor alimentary disturbances.

Dealing with the principles of ventilation, the lecturer pointed out that attention had been wrongly focused on the chemical purity of the air. The air in our rooms was never vitiated by the chemical products of respiration to such an extent as to have the least physiological effect. The two things that mattered most were : (1) bacterial purity—prevention of spread of infection by saliva spray from carriers of disease, most difficult to prevent when people crowd so closely together in trains, lifts, meeting-houses, etc., and talk, cough, and sneeze at each other ; (2) the maintenance of a proper and not monotonous rate of cooling of the skin. Of importance, too, was the freedom of the air from dust and irritating and unpleasant products of combustion, smoke, petrol fumes, and smells that depress our feelings. The methods in vogue of warming and ventilation too often gave us monotonous windless atmospheres which did not cool and stimulate the skin. The ideal conditions out of doors were radiant heat of the sun and a cooling variable breeze. Indoors we wanted a source of radiant heat and cool air, gently wafted first this way and then that, to prevent monotony or unpleasant draught. The open fire, or the modern gas fire, gave us radiant heat, and reversing fans could be used to give changing wafts of air. Hot-air plenum systems gave entirely the wrong conditions. In the House of Commons the members' feet were made cold and their heads too warm by the air impelled up through the floor gratings, a condition which leads to congestion and swelling of the mucous membrane of the nose. The lecturer demonstrated the surface temperatures of the skin and clothes, and how these were affected by radiant heat and wind, and pointed out the protection afforded the negro by the black pigment in his skin to the tropical sun. The black man's pigment absorbs the sunlight on the surface of the skin, converts it into heat which stimulates the transudation of water, so the heat is sweated off. In the white man the sunlight penetrates and warms the deeper layers of the skin and the blood therein. Thus the white man has to shield himself with clothes. These surround him with stationary humid warm air, preventing the cooling effect of wind. Thus the white man cannot do field labour in the tropics, and must be a burden on the black man's labour. White clothes reflect and scatter the sun's rays, while black and dark colours absorb them and convert them to heat rays ; therefore we need to wear white in the sun's heat, and black or dark colours in cold climates. The lecturer next demonstrated his Kata-thermometer, an instrument devised for measuring the cooling power of the skin surface at body temperature. This, a large-bulbed spirit thermometer (of standard shape and size), is heated above 110° F. and then allowed to cool, and the rate of cooling from 100° to 95° F. measured with a stop-watch.*

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* A factor is determined for each instrument which allows the readings to be converted into mille-calories of heat loss per sq. cm. per sec.

The instrument is used (1) dry = cooling by radiation and convection; (2) wet (surrounded with a glove of damp muslin) = cooling by evaporation, radiation and convection. The difference between the two readings gives the evaporative cooling power. The dry instrument can also be clothed in a woollen glove, and readings taken representing the cooling power of the clothed portions of the body.

The ordinary dry-bulb thermometer is a static instrument, representing average temperature of surroundings and giving no account of the cooling effect of wind. The Kata-thermometer, on the other hand, is a dynamic instrument like the human body, and represents the rate of heat loss and the enormously important influence of wind. It can be used as a most delicate annemometer.* The wet Kata-thermometer shows the great effect of wind in promoting evaporation of sweat from the skin.† It is an admirable measurer of evaporative power, and should be of great use in determining this in the drying chambers used in so many commercial processes. The Kata-thermometer readings show that the cooling power of the atmosphere in confined rooms is like or even worse than that of tropical regions out of doors. The difference in cooling power is enormous between still air and full exposure to wind, such as an outdoor worker endures. Movement of air is of vastly greater importance to our comfort and health than temperature measured by the ordinary thermometer. To test if with *natural* ventilation there is sufficient *movement* of the air in rooms the volume of carbon dioxide in the air may be measured, *but it should not exceed that in the outside air by even* 1 *part in* 10,000. There should be no permissible increase.

Kata-thermometer readings taken with various clothing materials show that the essential heat-retaining power depends on the retention of a layer of still warm air in contact with the body. To keep cool in hot days the one thing necessary is to allow the wind to blow through flimsy, loose, open garments. To keep warm in arctic conditions *wind-proof* clothing is designed, such as that used in Labrador. Whether the clothing is woollen or cotton in this respect is immaterial; its thickness and permeability to wind are the essential properties. A woollen stuff when wet does not cling close to the skin, as cotton does, owing to the elasticity of the hair fibres in the woollen garment. The curving hairs keep it off from the skin and so allow a layer of warm air to protect the skin from the cooling effect of wet. Thus a wet woollen is warmer than a wet cotton garment. In no other respect do the hygroscopic properties of clothing materials seem to matter. A shipwrecked man immersed in the sea should not strip, but keep on his woollen clothes, so that the entangled air may keep him warm. A great deal of nonsense is talked about the relative merits of cotton and woollen stuffs. Cotton flannelette made to entangle air like flannel is as warm as flannel.

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* It measures the effect of eddies together with the direct current.

† See Trans. Roy. Soc. B., 1916, p. 183.

The lecturer demonstrated with the Kata-thermometer the enormously greater rate of cooling in cold water compared to air of the same temperature, and showed the influence of a wet woollen glove with entangled air compared to that of a glove wet and containing no air, and that of a water-proof glove.

In the causation of trench feet, the soldiers are immersed in cold water for many hours. Bacterial decomposition of the skin secretions takes place if boots and stockings are worn continually for days, and the skin is macerated by the ammoniacal products. Water soaks into the macerated skin and causes local destruction of the tissues. Prolonged immersion in sea-water is endured by fishermen with impunity because of the salt, which prevents maceration of the skin—a question of difference of osmotic pressure. It is not, then, merely the prolonged chilling, but the soaking of water into the skin which helps to do the damage. The continuous pressure of the boot on certain parts helps to produce the injury. Ointments made with antiseptics, such as salicylic acids and essential oils, have been used by the Italian soldiers, spread on cotton cloths and wrapped round the feet. Prophylactic discipline, directed towards changing the stockings, drying and cleaning the feet, is of the greatest importance. This and the provision of waterproof waders, and trenches properly built with planked pathways and water-channels beneath, have prevented the suffering which our troops endured in the first winter of the war.

[L. H.]

WEEKLY EVENING MEETING,

Friday, February 4, 1916.

THE RIGHT HON. LORD WRENBURY, P.C., Vice-President, in the Chair.

PROFESSOR WILLIAM BATESON, D.Sc. F.R.S.

Fifteen Years of Mendelism.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1916.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Honorary Secretary announced the decease of the Right Hon. Viscount Alverstone, G.C.M.G. P.C. M.A. D.C.L. LL.D. F.R.S. Hon.M.Inst.C.E., late Lord Chief Justice of England, on December 15, 1915, a Member of the Royal Institution; and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, by the decease of Lord Alverstone, G.C.M.G. P.C. M.A. D.C.L. LL.D. F.R.S. Hon.M.Inst.C.E., late Lord Chief Justice of England, Member of the Royal Commission of Historical Manuscripts.

Lord Alverstone was a Member of the Royal Institution for thirty years. For many years, as Manager and Vice-President, he took an active interest in the welfare of the Institution, and in its administrative and legal affairs. He rendered valuable advice in connection with the reconstruction of the mode of presentation of the Accounts, by arranging for the introduction of a Chartered Accountant to assist the Visitors in their Annual Audit, which in many ways has been operative for the benefit of the working of the Institution. The charm of the late Lord Chief Justice's personality will long be remembered by the Members.

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with the family in their bereavement.

Professor William Arthur Bone, D.Sc. Ph.D. F.R.S.,

was elected a Member of the Royal Institution.

Pursuant to Notice given at the last Meeting,

It was RESOLVED, That Bye-Law Chapter XVIII., Article 2, be suspended.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Kodaikanal Observatory, Bulletin, No. 48. 4to. 1915.

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Accountants, Association of—Journal, Vol. VIII. Dec. 1915; Vol. IX. Jan. 1916.
Allegheny Observatory—Miscellaneous Scientific Papers, Vol. III. Nos. 19-21. 4to. 1915.
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American Geographical Society—Bulletin, Vol. XLVII. No. 12. 8vo. 1915.
Geographical Review, Jan. 1916.
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British Museum Trustees (Natural History)—Catalogue of Ungulate Mammals, Vol. IV. 8vo. 1915.
Catalogue of Fresh-Water Fishes of Africa, Vol. III. 4to. 1915.
Catalogue of Moths, Supplement, Vol. I. Text and Plates. 8vo. 1915.
Revision of the Ichneumonidæ, Part 4. 8vo. 1915.
Guide to Fossil Remains of Man. 8vo. 1915.
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Museum Bulletin, Nos. 29-30, 1915. 8vo.
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Canada, Royal Society of—Transactions, Third Series, Vol. IX. Sept. 1915. 8vo.
Carnegie Institute—Contributions from Mount Wilson Solar Observatory, Nos. 104-107. 8vo. 1915.
Chemical Industry, Society of—Journal, Vol. XXXIV. No. 22, 1915; Vol. XXXV. No. 1. 8vo. 1915.
Chemical Society—Journal for Dec. 1915-Jan. 1916. 8vo.
Chicago, Field Museum of Natural History—Publications: Zoological Series, Vol. X. No. 13; Anthropological Series, Vol. XV. No. 1.
Civil Engineers, Institution of—Proceedings, Vol. CC. Part 2, 1914-15. 8vo. 1915.
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Dewar, Sir James, M.A. LL.D. F.R.S. M.R.I.—Collected Papers on Spectroscopy. By G. D. Liveing and Sir James Dewar. 8vo. 1915.
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WEEKLY EVENING MEETING,

Friday, February 11, 1916.

EDWARD POLLOCK, F.R.C.S., Vice-President, in the Chair.

PROFESSOR W. M. FLINDERS PETRIE, D.C.L. Litt.D. LL.D. F.R.S. F.B.A.

Egyptian Jewellery.

FAR back in the prehistoric civilisation of Egypt began the search for beautiful materials. The choicest qualities of flint were selected as subjects for the most delicate flaking, more refined than any such work since then. The desert was ransacked for carnelian, garnet, noble serpentine and turquoise. Lazuli was imported from Persia, and emery-rock to cut the stones from Smyrna. The most beautiful rocks, porphyry, diorite, blue and green volcanic ash, coloured marbles, were sought for the dishes and cups of the table service. Never in later times was there a more general use of fine materials than at the dawn of history. Nor is Egypt alone in this, the earliest stone vases of Crete are also the most beautiful.

Fortunately there remains from that age a set of bracelets which show us the earliest jewellery. The arm of a queen of the 1st Dynasty, which I found at Abydos, had four bracelets of gold, turquoise, lazuli and amethyst. The soldering of the gold was perfect, without a trace of excess or difference of colour.

After that there is nothing known of the Great Pyramid age, when the noblest work might be expected, but on coming to the XIIth Dynasty an abundance is known to us. Strings of splendid amethyst or carnelian beads were commonly worn. Gold work was decorated with patterns of fine globules of gold like the later Etruscan jewellery. Naturalistic figures of fishes show the most careful modelling. The great development of this age was the inlaid designs, *not* enamels, but *champlevé* inlays of hard stone.

The crowns are, perhaps, the most striking pieces of the Dahshur jewellery. Every minute piece of the little florets was cut and polished separately, and they were then attached to the maze of fine gold threads which enclosed the hair.

The pectorals were the greatest triumphs of skill. A bold design of openwork in gold was inlaid with hundreds of pieces of coloured stones, too minute to be handled except with a wet needle. The gold engraved side is shown here for the minute engraving.

The design deteriorated reign after reign—more confused and crowded. The splendid groups of the Dahshur jewellery have been celebrated for the last twenty years.

Two years ago the work of the British School in Egypt brought to light the Treasure of Lahun, a group by the same school, or perhaps the same artists, as the Dahshur riches, but in point of design perhaps even finer. The pyramid I had partly explored twenty-five years before, and we went there to exhaust the quest, completely baring every inch of rock around it far and wide. The sarcophagus of red granite in the pyramid must be accounted in the jewellery. If the whole of the errors of work in that were concentrated in a little brooch they would not be perceived. The sides are straight to $\frac{1}{200}$ inch, they are parallel to $\frac{1}{50}$ or $\frac{1}{100}$ inch, they are flat all over to $\frac{1}{300}$ inch, and in the exact proportions of all the dimensions to each other, there is only $\frac{1}{30}$ inch variation.

Around the pyramid was a great brick enclosure, on which patrolled the guards to see that no attempt was made on the royal tombs. Large stairways or shafts descended to the various tombs of the princesses. At the bottom of one shaft was a granite sarcophagus, the lid pushed back, the edges broken to admit a boy, and not a fragment of the burial left inside it. Yet in a recess within reach of it stood the two caskets of jewellery and the crown perfectly undisturbed. The name is given by the alabaster jars which contained the viscera—the Princess Sat-hathor. There were some large necklaces, one of cowry shells of gold, the others of lion's heads. Bracelets with gold lions, and beads of gold and carnelian. Armlets of mottoes inlaid with carnelian. A mirror of silver, with a handle of black obsidian and gold inlaid with carnelian. Bracelets of gold, turquoise, and carnelian beads, spaced by gold bars covered with minute beads. They were fastened by sliders of gold inlaid with carnelian and blue faience; the perfect work of fitting undercut sliders of four inches long to run without a hitch is exquisite. Originally there were smaller bracelets made with plain sliders for the princess, under Senusert III.; when she became queen of Amenemhat III., later on, the larger bracelets were made up, with inlaid sliders. A necklace of the deepest amethyst has gold claw pendants. The large outermost necklace of pendants is of the most brilliant specimens of the stones that I have seen—intense red carnelian, light blue-green amazonite, and strong blue lazuli. The crown is of a much older pattern, the band around the head with inlaid rosettes upon it. Two pectorals were found, one for the princess with her father's name; the other with her husband's name. Probably the pectoral was hung by a necklace of the deep amethyst beads. The later pectoral of Amenemhat III. is inlaid with lazuli, green faience and carnelian. The backs of the two pectorals show how superior was the design and detail of the earlier one. It was inlaid with lazuli, turquoise, and carnelian. The free wavy outline of the falcons and centrepiece is far better than the stiff frame border of the Dahshur pectorals. In this respect this work surpasses all that was known, and is apparently the finest piece of minute colour inlay that has been preserved. None of the greatest modern jewellers will attempt to execute such work, on any terms.

The later Egyptian jewellery, skilful as it is, never reached such perfection again. The bracelets of King Aahmes are coarse by comparison. The dagger of King Aahmes is of much interest for the inlaid design of gold lines in dark metal—a fashion also well known at Mykenæ. The royal axes were also in relief-designs with coloured inlays.

Under Rameses II. some skill in design is still seen, but the work is much less delicate. Still later, about 600 B.C., inlaying of glass set in gold was practised; and a little vulture, no larger than the little finger, has over two hundred separately cut and mounted feathers on it. A favourite form of fine metal work in the later ages was inlay of gold in bronze, as in the large figure of the Athens Museum.

Thus we may say that as the Egyptian excelled in the grandeur of his architecture and the marvellous precision of his stone work, so also in the most delicate detail he exceeded all that has been done in other lands, supreme in his greatness as well as in his minuteness, beyond the scale of all other men.

[W. M. F. P.]

WEEKLY EVENING MEETING,

Friday, February 18, 1916.

Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

Professor Ernest G. Coker, M.A. D.Sc. M.Inst.C.E. M.R.I.

Polarized Light and its Applications to Engineering.

One of the fundamental questions which arises in the majority of engineering problems is the design of a structure or machine which will carry out some pre-determined work in an efficient and economical manner, and whatever the problem may be, it is almost invariably bound up with the arrangement of a number of connected parts designed to resist loads which are imposed upon them.

The machines and structures which the engineer has to construct are almost infinite in their variety, and each one usually presents a new and a difficult problem, especially as regards the stresses which may be imposed upon its parts, and the way in which these stresses are distributed.

The application of science to the determination of the internal stresses in materials loaded in any given manner, offers, in fact, a great field of inquiry which has resulted on the one hand in a highly developed mathematical theory of the strength of materials, and on the other, in a vast number of experimental investigations upon the physical properties of engineering materials; and it is upon the data which mathematical and physical researches provide, that the engineer must depend for guidance in designing safe and economical structures and machines.

It is, I think, a common experience among engineers to find themselves confronted with a stress problem in their designs which, even with all their accumulated wealth of knowledge, presents almost insoluble difficulties; it often defies mathematical processes, and is beyond the scope of any previous physical investigation. But it must be solved, if only approximately, and the imperative need of an answer, renders it advisable to make experimental investigations before proceeding with an important work of construction.

It is perhaps somewhat severe, but not untrue, to say that engineers have not always made the fullest use of the discoveries of pure science in their practice; and it is remarkable that a discovery of Sir David Brewster, in 1816, that transparent materials when stressed become doubly refractive, should not have been more frequently pressed into service, for its use was immediately obvious to the discoverer, who pointed out that the stresses in the arched rings of bridges, could be rendered visible in a glass model by aid of the doubly refractive effect produced by a beam of polarized light.

Here and there one finds accounts of applications of this property for engineering work, but usually with little success, mainly no doubt, to the difficulties experienced in shaping glass models to the required form; but when these are overcome the value of the information gained is very great, as, for instance, in the recent valuable investigations of the stresses made upon a glass model of a reinforced concrete arch by M. Mesnager, of Paris, who used the results so obtained for the design of an arch of about 310 feet span, with a most gratifying agreement between the stresses in the actual bridge and its model. The expense and difficulty of constructing glass models is a bar to their general use, and other transparent materials are now available which offer many advantages, in that they are strongly doubly refracting under stress, are easily fashioned with engineering tools, and are not readily broken or damaged, while the cost is insignificant.

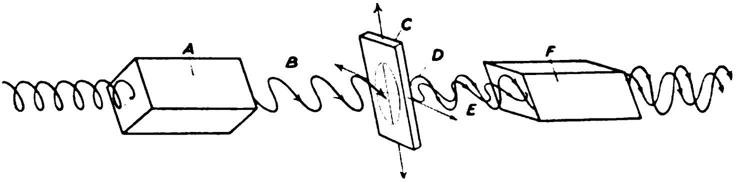


Fig. 1.

Here, for example, is a rough model of an arch-ring, and you observe that when loads are applied, it glows with colour in the polariscope, and a picture of the state of internal stress is obtained which can be readily interpreted.

It is useful to form an idea of the behaviour of a beam of polarized light passing through a plate of stressed material, and although modern research has shown that the light is a complex electro-magnetic phenomenon, I will content myself with an elementary explanation based upon the wave theory, in which we suppose that a Nicol's prism (A, Fig. 1), or a sheet of black glass, at a suitable angle reduces an ordinary beam of light to a transverse vibration *B* of the ether in a plane perpendicular to the path of the ray, which later passes without material change through unstressed material *C*, but is broken up into two waves D, E, vibrating at right angles and in the directions of principal stresses if the material is loaded in any way. Both waves are retarded somewhat by the transparent material, but one more so than the other, and if a second Nicol's *F* prism is interposed, it selects the components of both waves which are parallel to its principal plane, and their interference causes the glow of colour which you have already observed.

MEASUREMENT BY COLOUR EFFECT.

We can estimate simple stresses by the colours observed.

If, for example, we take a strip of transparent material, and arrange the optical apparatus so that when the strip is unloaded no light is transmitted, the effect of a moderate tension causes the specimen to appear a greyish white, and as the stress increases, the colour changes by insensible gradations to a lemon-yellow, then to a reddish purple, and, with a very little increase of stress, to a well-defined blue. With a further increase of stress, the scale of colours is approximately repeated for twice the intensity of stress, and the relation of colour to stress is shown to be that given in the table below. We therefore have a recognizable and easily understood colour scale to mark the intensity of stress in a material.

For simple tension and compression, the relative retardation of the rays which produces the colour effect, is proportional to the stress and to the thickness of the material. In such cases therefore, the stress intensity may easily be determined by observing the colour bands, bearing in mind that both tension and compression produce similar effects, if changes in the thickness of the material are allowed for. Thus, if we take the case of a transparent beam subjected to a uniform bending moment, a system of colour bands is obtained, distributed as shown in the accompanying experiment, and, by inspection with the aid of Table I, which can be calibrated in terms of stress, the distribution across the section can be determined

TABLE I.—COMPARATIVE TABLE OF TENSION AND COMPRESSION STRESSES CORRESPONDING TO A GIVEN COLOUR.

Order	Colour	Stress
I.	Black	0
	Grey	3·5
	White	5·5
	Straw	8·0
	Orange	10·0
	Brick-Red	10·5
	Purple	11·0
II.	Blue	13·0
	Yellow	18·0
	Red	21·0
	Purple	22·0

approximately, as shown in the diagram. It is seen that the stress varies uniformly from a maximum compression at the under side to nothing near the centre, and it then changes sign and increases uniformly to a maximum tension stress at the upper side of the beam, in exactly the same manner as occurs in a metal beam subjected to the same kind of loading.

This case and others which have been examined afford instances where the results of optical experiments can be compared, not only with mechanical measurements of strain, but also with the theory of the distribution of stress in materials; and the experimental determinations for a transparent material show a very good agreement with strain measurements and with the precise theory. We can, therefore, feel very confident that in more complicated cases the stresses in a transparent model are similar to those in a metal. For example, a beam with a notch cut in it may be taken (as shown), and, as might be expected, the effect of the notch is seen to increase the stress in the material very considerably. The distribution is now much more complicated than it is in a simple beam; the neutral axis has moved towards the notch, while the colour effects show that the maximum stress is at least twice as great as that in a beam without a notch.

To apply this kind of experimental work to cases of practical importance, it is evidently desirable that the material used should possess similar physical characteristics to those possessed by steel, iron, and other materials in general use by engineers, and in this respect glass has very many desirable qualities. Unfortunately, however, in order to produce colour effects in glass specimens, it is necessary that the material should be very thick, and the forces to which the glass is subjected very great—in fact, very near the breaking-point of the material. Glass is, moreover, an extremely difficult material to shape, and specimens can only be produced by laborious processes.

The extensive commercial value of nitro-cellulose preparations in recent years, however, affords a material which has nearly all the desirable features of glass, and, in addition, some other characteristics which make it superior in many respects for experimental work. This material may be obtained in large sheets of moderate thickness, entirely free from stress except at the edges. It is not so transparent as glass, but it has the very important advantage that specimens may be cut from the sheet quite readily with ordinary wood and metal-working tools, and with reasonable care the contours of the specimen show no signs of residual stress after shaping.

An example of its value is afforded by the accompanying model of a roof-truss in which the riveted joints havs been accurately constructed from the drawings supplied, while another is supplied by the cog-wheels in gear, which have been machined in exactly the same manner as their metal counterparts.

LAWS OF OPTICAL EFFECT.

We must now inquire what are the simplest means of measuring the stresses produced in a plate of transparent material caused by any given system of loading in its own plane, and it will be apparent that if the stress is merely simple tension or compression, its intensity can be read off at once by reference to the colour scale already established. Many cases of this kind occur in practice. If, for example, a simple ring is cut through one side and loaded after the fashion of a hook it is very severely stressed, especially at its principal horizontal section, and the distribution across this section consists of tension and compression stress only.

We may therefore read off the stress at any point of the section and obtain curves of distribution, such as those now shown, in which the stress intensities for several loads have been determined and plotted.

In this case the experiments agree fairly well with the calculations of Andrews and Pearson on the stress distribution of a hook of this cross-section, and they also show the interesting result that as the load increases the neutral line moves away from the tension side.

In most cases, however, the stress distribution is more complicated to deal with, but it is known that any case of stress in the plane of a plate, can always be represented by two principal stresses at right angles, and if the magnitude and direction of these are determined for all points the stress distribution is solved.

In order to obtain an experimental solution of this problem, it is necessary to inquire into the relation of the optical effect to the principal stress intensities at a point, and it is easy to show this by simple experiments. If, for example, we take two tension members and subject them to the same uniform stress intensity, the colour effects produced by interference will be precisely the same for each, while if they are superposed, or if we interpose a double thickness, the colour effect is that produced on a single member under twice the stress. Experiments on three or more members readily verify for simple tension and compression that the optical effect is simply proportional to the stress intensity, and also to the thickness of the plate. If, however, two equally stressed tension members of the same thickness are crossed, the common area gives a dark field, showing that the stress effect of one neutralizes that of the other. The same dark field is produced if an equally stressed compression member is placed with the direction of stress parallel to that of the tension member, and we may readily verify in all cases that tension and compression stresses in the same direction add their effects, while stresses in directions at right angles subtract them.

The latter result is of chief importance, since the stress at any point of a plate can always be represented by the stresses p and q

at right angles, and their optical effect is therefore proportional $p-q$. The value of the stress difference may therefore be determined by matching the optical effect produced at the given point with that produced on a simple tension or compression member. The eye is, however, not a very reliable instrument, especially after the fatigue of a few minutes' exposure to a brilliant light; and it is better still to reduce the optical effect to zero by a simple tension or compression member set along one of the directions of principal stress, and stressed until a dark field is produced.

The laws which the optical effects obey may be at once utilized for a variety of cases of practical interest.

THICK CYLINDER.

An example is furnished by piping for transmitting fluids under pressure. The action of water, or other fluid, in a pipe, can be imitated by applying a uniformly distributed pressure to the interior of a ring, such as is now shown, where the application of a uniformly applied pressure produces a stress distribution in the circular ring of a perfectly symmetrical character. The arrangement of the colour bands shows that there is a very large stress at the interior surface, diminishing rapidly at first, and afterwards more gradually as the

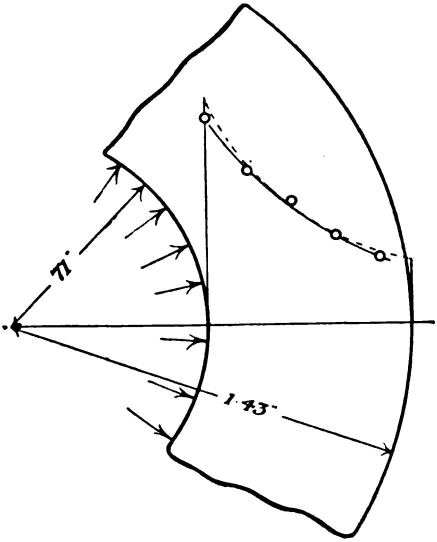


Fig. 2.

outer surface of the pipe is approached. In this case there is known to be a radial compression stress accompanied by a circumferential tension stress, and the optical effects produced at any point are proportional to the algebraic difference of their intensities—in this case their numerical sum.

In a thick cylinder of these proportions the radial stress is not large, and its intensity can be determined independently, but the combined effects of both stresses have been measured here, and are plotted in Fig. 2, in which the firm line gives the experimental values obtained, while the dotted curve represents the results of calculation.

If p is the circumferential stress and q the radial stress at any radius r of a cylinder with free ends, calculations show that their values are given by—

$$p = \frac{p_1 r_1^2}{r_2^2 - r_1^2}\left(\frac{r_2^2}{r^2} + 1\right)$$

$$q = \frac{-p_1 r_1^2}{r_2^2 - r_1^2}\left(\frac{r_2^2}{r^2} - 1\right)$$

so that

$$p - q = \frac{2p_1 r_1^2 r_2^2}{r_2^2 - r_1^2} \cdot \frac{1}{r^2} = \frac{C}{r^2}$$

where p_1 is the applied pressure, and r_1 and r_2 the inner and outer radii of the cylinder.

In an experiment carried out on a ring in which the inner radius $r_1 = 0{\cdot}71$ inches, and the outer radius $r_2 = 1{\cdot}43$ inches, a pressure of 900 pounds per square inch was applied to the interior of the ring, and the experimental values compared with those obtained by calculation. The following results were obtained :—

TABLE II.

Radius r inches	$(p-q)$ in lbs. per sq. inch		
	Experimental Value		Calculated Value
	Observed	Corrected for Cup Leather	
·71	2100	2185	2400
·85	1560	1625	1660
1·00	1185	1230	1205
1·15	870	905	910
1·30	670	700	715
1·43	—	—	588

Showing a fair agreement with the theory, which is really closer than the numbers indicate, owing to the full pressure recorded on the gauge not being effective, as will now be described.

As is evident, this stress problem requires the application of a measurable fluid pressure to the cylindrical surface of a ring, in such a way that no essential part is obscured from view, and a simple and effective means has been devised for this purpose by my assistant, Mr. F. H. Withycombe.

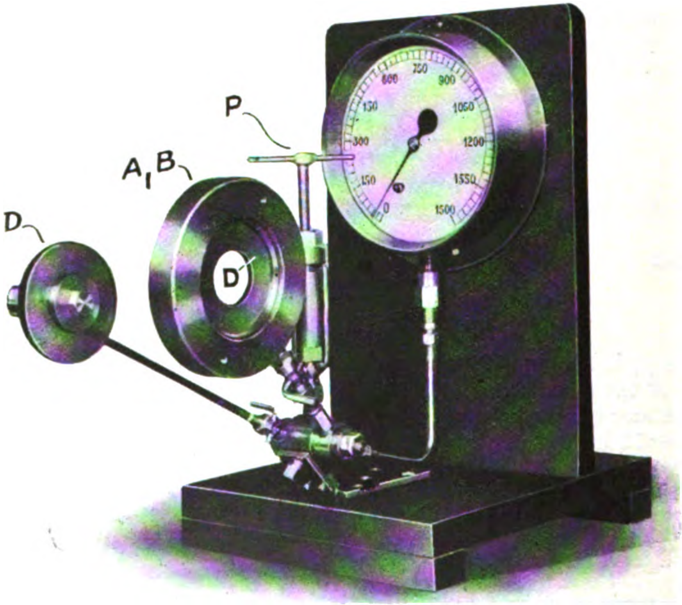
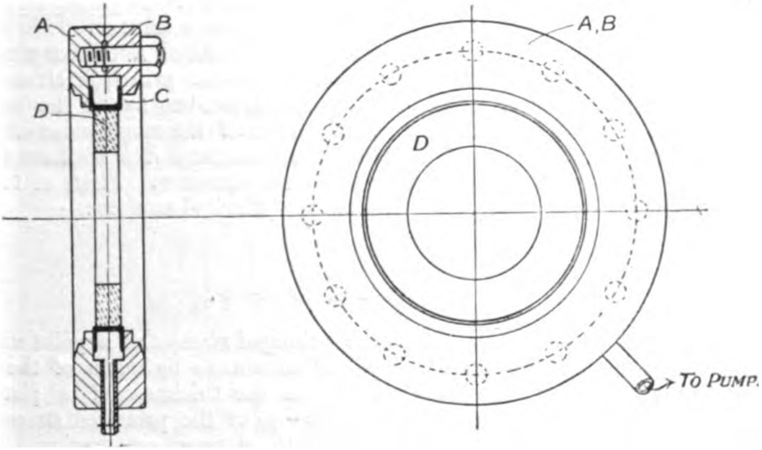


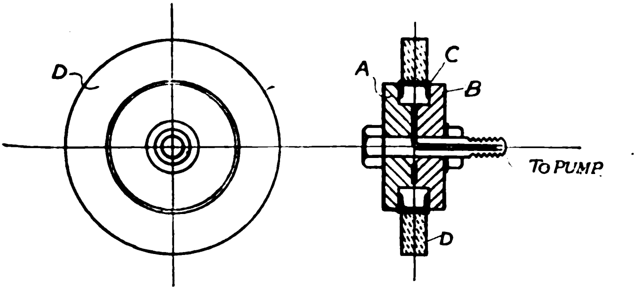
FIG. 3.

Fig. 3 is a photograph of the complete apparatus for applying internal and external fluid pressure to rings, and drawings of the essential details are shown in side elevation and cross section in Fig. 4.

Fluid pressure of water or other liquid is applied by the action of a small hand-pump P, the piston of which is actuated by a screw to force oil at any desired gauge-pressure into the annular space between two metal discs A, B, bolted together to hold a retaining ring C shaped like a Bramah cup-leather to prevent leakage of the fluid. This retaining ring projects slightly beyond the periphery of the discs, and carries the transparent ring to be stressed. The cup-leather is itself so thin that a pressure of a few pounds per square inch will burst it, but when the ring D is mounted upon it, even a pressure of 2,000 pounds per square inch may be applied with safety.



Cross-Section.   Side Elevation.



Side Elevation.   Cross-Section.

Fig. 4.—Pressure Chambers for Applying Fluid Pressure to the Internal and External Boundaries of Rings.

In the experiments described above, the ring had a thickness equal to the interior breadth of the cup-leather, but a small percentage of the total pressure is absorbed by this leather, and is not exerted upon the ring. The experimental results must therefore be slightly lower than the calculated values, and, if the correction is made, there is a very good agreement, as the table shows. In more complicated cases the stresses are less amenable to calculation, as in cylinders provided with ribs for aeroplane engines, or tubes constructed in sectors bolted together, as in underground railways, but it would not be difficult to determine experimentally the stresses produced under working conditions, although not in so simple a manner.

So far our measurements have only utilized a property which enables us to measure the difference of the principal stresses at a point in a plate subjected to plane stress, but by far the greater number of problems require a knowledge of the magnitudes of each stress separately, as well as their directions, and it is to these determinations which we must now address ourselves. First, as to the determination of the magnitude of the principal stresses :—

PRINCIPAL STRESSES.

A measure of the sum of the principal stresses at a point can be obtained, as Mesnager suggested, if advantage be taken of the fact that the stress causes a change in the thickness of a plate of material proportional to the sum $(p-q)$ of the principal stresses in its own plane. If, for example, both stresses are tensions, there will be a lateral contraction of $(p+q)/m\,E$, where E is the modulus of direct elasticity, and m is Poisson's ratio. Both these latter quantities can be determined, and the sum of the stresses can be measured, if an extensometer is used of sufficient accuracy to measure the lateral contraction. The values of the physical quantities E and m differ very much for different materials, but for the artificial transparent material used here they are much smaller than for a metal, and the difficulty of this kind of measurement is therefore much lessened. A fair valve of E for xylonite is 300,000 in pound and inch units, while m has a value of about 2·5, so that for each 1000 lb. of stress intensity, the corresponding lateral contraction for plates of the usual thickness of $\frac{1}{4}$ inch is $\frac{1}{3000}$ of an inch. To measure such a quantity to an accuracy of within 1 or 2 per cent it is advisable to use an instrument capable of indicating a change of at least one-hundredth of this quantity; such changes have been measured with fair accuracy by using a lateral extensometer capable of detecting a change of about half-a-millionth of an inch. An instrument of this kind has been employed by Mr. Scoble and myself in several investigations, and an elevation of one form of this apparatus is shown in Fig. 5 attached to a specimen A.

In this arrangement, a frame B carries a calibrating screw C, the point of which bears against the plate of transparent material, and is immediately opposite to a second piece D, the inner end of which is lightly pressed against the plate by a spring E, while the outer end presses against the short arm of a lever F pivoted at G, and controlling the angular position of the lever H of a mirror J pivoted at K. Any change which takes place in the thickness of the specimen between the measuring points causes a rotation of the mirror, and this change can be measured by observing the movement of a spot of light which is reflected from the mirror in the usual manner. The observations can be checked by the calibrating screw, which is provided with a graduated head V for this purpose. It will be noticed that the measuring points simply bear against the face of the bar and do not penetrate it, so that the length over which a measurement is made can be accurately determined. The whole of the measuring apparatus is, moreover, supported on a pair of light steel springs M, attached to a clip N, secured to the specimen by a pair of screws O, so that indentations are avoided at the points of measurement, while errors caused by the weight of the instrument on the measuring points are negligible. Further, it may be noted that if measurements of the lateral contraction are compared with those from a similar plate of material when subjected to simple tension or compression stress, it becomes unnecessary to find the values of the physical constants *m* and E of the material, and a possible source of error is thereby avoided. For investigating cases of plane stress a combination of the two methods described here is chosen, in which the sum of the two principal stresses at a point is found by a lateral extensometer, and the difference by an optical measurement, since it involves

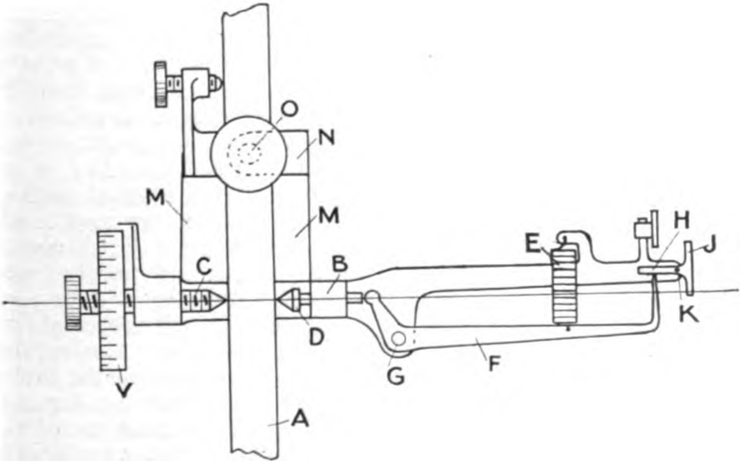


FIG. 5.

mechanical measurements only, and is therefore particularly adapted for engineering work. In some cases it requires considerable care to obtain accurate values of each quantity separately, especially if one stress is very much smaller than the other, as then minute errors of observation become a large percentage of the value of the lesser stress; but possibly this difficulty would be met with in any other method.

LINES OF PRINCIPAL STRESS.

Reference has already been made to the fact that any state of stress at a point in a plane may be represented by a pair of stresses at right angles through the point. If a simple case is taken of a plate subjected to tension by forces uniformly applied at the ends, the state of stress in the body may be imagined as being represented by equally spaced stress lines continued throughout the plate, and the intensity may be indicated by the spacing, and the kind of stress by the character of the line drawn. If another stress system is applied perpendicularly to the first, it may be indicated in the same manner, and the condition of the body is then shown by a network of lines, which may be looked upon as pulled or pushed according to the circumstances existing in the body due to the external loading. Such simple cases do not often occur, but whatever may be the character of the stress distribution in a plane, it can be represented conventionally by two systems of orthogonal curves spaced in some manner which the external loads and boundaries of the plate dictate. If, for example, two symmetrically disposed notches are cut in a tension member, it is clear that equally spaced tension lines above and below must be crowded together as they pass the narrow neck, and those at the sides will probably come closer together than do those in the centre, thereby indicating a high stress intensity at the middle points of the notches. Lines of principal stress may be drawn from the data provided by experiment, if advantage be taken of the property possessed by stressed material of causing the two systems of retarded rays to vibrate, one in the direction of the major principal stress, and the other in the direction of the minor principal stress.

Between crossed Nicol's prisms a loaded plate shows, in general, dark bands, which mark the positions of all points where the directions of principal stress correspond to the axes of the polarizer and analyser, and by varying the angular positions of these latter a series of bands is obtained, each corresponding to definite directions of the axes of stress.

If, for example, the case of a simple tension member is taken, with notches in it on each side as shown, dark bands are observed like those shown in the accompanying figure, and these change their positions as the axes of the optical apparatus are rotated. A diagram may be constructed which shows the centre lines of a number of these curves, with the directions of the axes of stress marked on them. If lines of stress at some distance away from the notches are now considered, they will clearly be parallel and perpendicular to the sides of the plate, but as they approach the discontinuity they must bend towards the centre line in order to pass through the narrow neck, since they cannot maintain their continuity in any other way. From these observations one can see how they are actually guided, and that they come close together at the extremities of the notches and produce an intense stress at these points. Other lines of principal stress at right angles to the first set are also indicated by the measurements, and the two systems give a kind of framework diagram which shows the direction of the principal stresses at any point, and therefore completes the experimental solution of the problem. The stress distribution in a plate cut to a required form, and stressed in an arbitrary manner by forces in its own plane, is therefore capable of solution experimentally.

COMPLETE SOLUTION.

The complete experimental solution of the stress distribution in a plate stressed by forces in its own plane, may be illustrated by an investigation by Mr. Scoble and myself of the case of the action of a rivet near the edge of a riveted joint, since we can determine the sum $(p+q)$ of the principal stresses, their difference $(p-q)$, and their directions. In this problem we can no longer neglect either principal stress, and it is in general necessary to determine both their directions and magnitudes. If the uniform tension stress in the full section of a plate is represented by equally spaced lines in the direction of stress, we may expect to find alterations in their directions and distances apart as they draw near to the discontinuity produced by the rivet, and an optical examination shows that the lines of stress approach one another very closely as they pass around the rivet, and afterwards diverge again if the overlap of the plate is sufficient to permit this. It is not difficult to explore the whole of a plate stressed in this way, by determining both the sum and difference of the stresses at a sufficient number of points on the lines of stress so found, and some of the measurements for the cross section passing through the centre of a rivet in a plate are shown in Table III., for the case of a plate in which both the overlap and the widths of metal on each side of the rivet are equal to the diameter of the rivet.

In these determinations the distance r of the point examined is measured from the centre of the rivet in terms of its radius a, while the stress p_r across the section and the stress p_t in the section are given in terms of the mean stress.

The experimental values of the sums and differences are plotted

TABLE III.

Stress Ratios	Stresses across the Section						
r/a . . .	1·20	1·40	1·60	1·80	2·00	2·50	3·00
$(p_t + p_r)/p$.	2·55	2·20	1·84	1·47	1·18	0·70	0·22
$(p_t - p_r)/p$.	2·80	1·60	1·00	0·76	0·56	0·10	−0·17
p_t/p . . .	2·68	1·90	1·42	1·11	0·87	0·40	0·025
p_r/p . . .	−0·125	0·30	0·42	0·35	0·31	0·30	0·195
	Stresses on Centre Line Below Rivet						
r/a . . .	1·40	1·50	1·70	1·90	2·20	2·50	2·80
$(p_t + p_r)/p$.	−4·31	−2·32	−0·90	−0·37	0·43	1·27	2·05
$(p_t - p_r)/p$.	−2·56	−2·32	−1·87	−1·48	−1·25	−1·32	−1·60
p_t/p . . .	−3·44	−2·32	−1·39	−0·93	−0·41	−0·025	0·225
p_r/p . . .	−0·88	—	0·49	0·55	0·84	1·30	1·82

in Fig. 6, and the principal stresses deduced from them are given in Fig. 7. They show that the tensile stress at the cross section reaches a high value, while below the rivet an even greater compression stress is produced. The measurements of radial stress along the sections chosen give marked compression close to the rivet, and it is worthy of note that they are very nearly zero at the outer boundaries of the plate, results which confirm the general accuracy of the measurements. Other measurements of a similar kind show that the action of a rivet produces an intense stress at the hole, sometimes reaching five times the stress in a full plate. In a transparent model this is often accompanied by permanent overstress and local yielding, which latter tends to equalize the stress in the material.

BLOCKS IN COMPRESSION.

A problem of considerable importance in practical engineering, is the distribution of stress in a rectangular block subjected to pure compression. This case occurs in the testing of materials when equal and oppositely directed loads are applied to the parallel faces of a short rectangular block, of a material like stone, brick, or concrete, to give a compressive stress as uniformly applied as is possible.

The manner in which the load is applied to the end faces of such a rectangular block, is known to exert an influence upon the distribution of stress and strain, and to obtain consistent results the end faces of small specimens are sometimes faced by aid of a surface plate in order to give a uniform bearing. In a large specimen, where this process becomes very laborious, it is often faced with plaster-of-Paris, and plates of millboard are sometimes interposed in addition, to give a uniform bearing area between the pressure plates of a testing machine and the specimen.

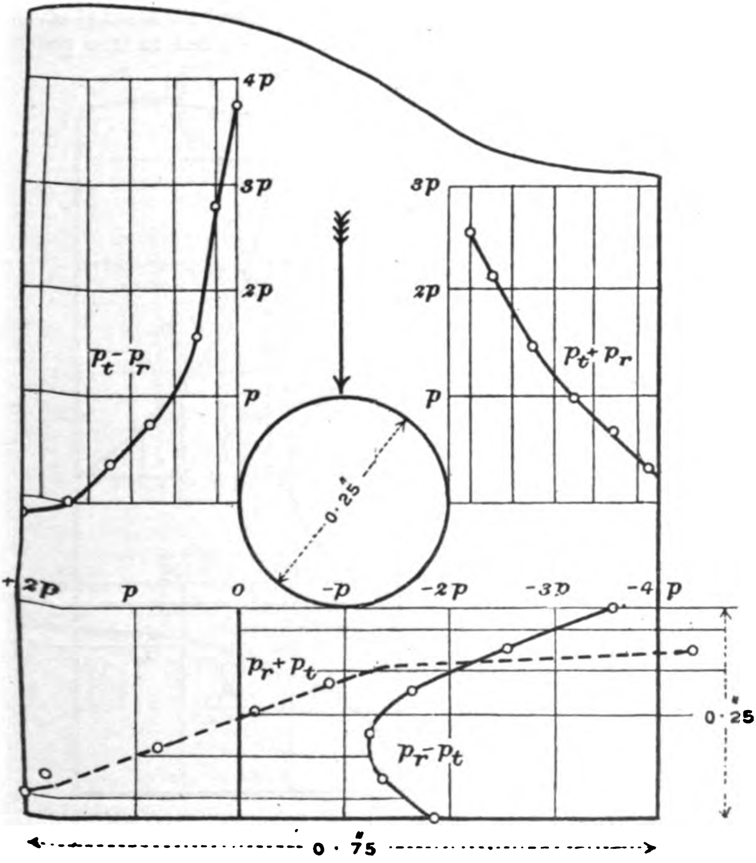


FIG. 6.—LOAD APPLIED BY RIVET. EXPERIMENTAL RESULTS.

It is difficult to experimentally determine the distribution of stress in a short block of an engineering material, but if a transparent model is used the problem is comparatively simple; and it may therefore be of interest to indicate what progress has been made in the solution of this practical problem by Professor Filon and myself.

In the majority of testing machines the mechanical arrangements for applying a pure compression load are defective, and for investigations of this kind a special form of testing-machine was devised and constructed to give a very accurately applied load. The general arrangement of this machine is shown in sectional elevation (Fig. 8), in which A and B are two substantial blocks worked to true planes at their opposing faces. One of these, A, may be fixed or capable of a small movement along the normal to the working face by means of the screw C. The other, B, is secured to a block carried by two flexible diaphragms DD, spaced at a considerable interval, and clamped at their outer edges in the framing of the machine in such a manner that the motion of the pressure-plate has only one degree of freedom, and is limited by the elastic deformations of the diaphragms. The applied load is weighed by a lever system E, and, as the work done

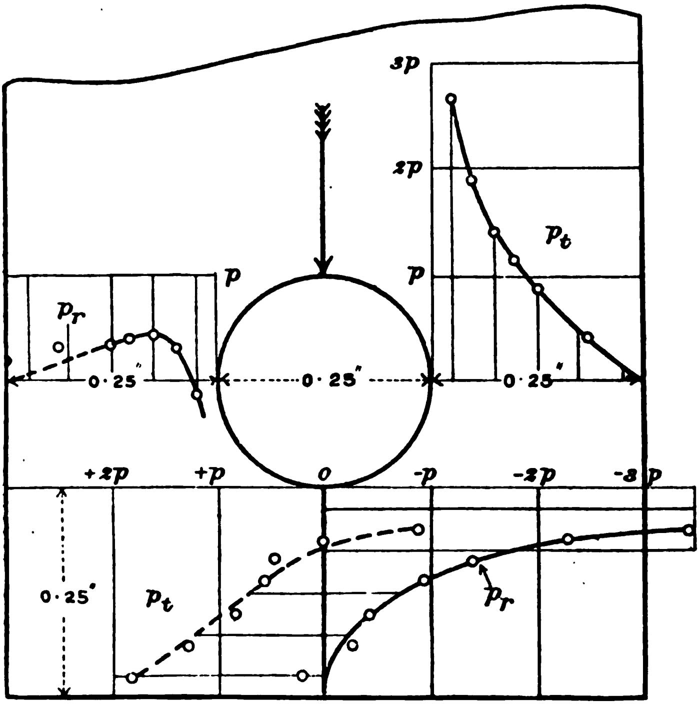


Fig. 7.—Load applied by Rivet. Principal Stresses.

in deforming the diaphragms is stored therein, and tends to bring the moving system back to its initial position, errors are avoided, and a very accurate measure of the applied load is obtained.

The methods adopted for obtaining the stress distribution were similar to those described above, and experiment showed that the lines of stress formed an orthogonal system very nearly directed in the line of load and perpendicularly thereto.

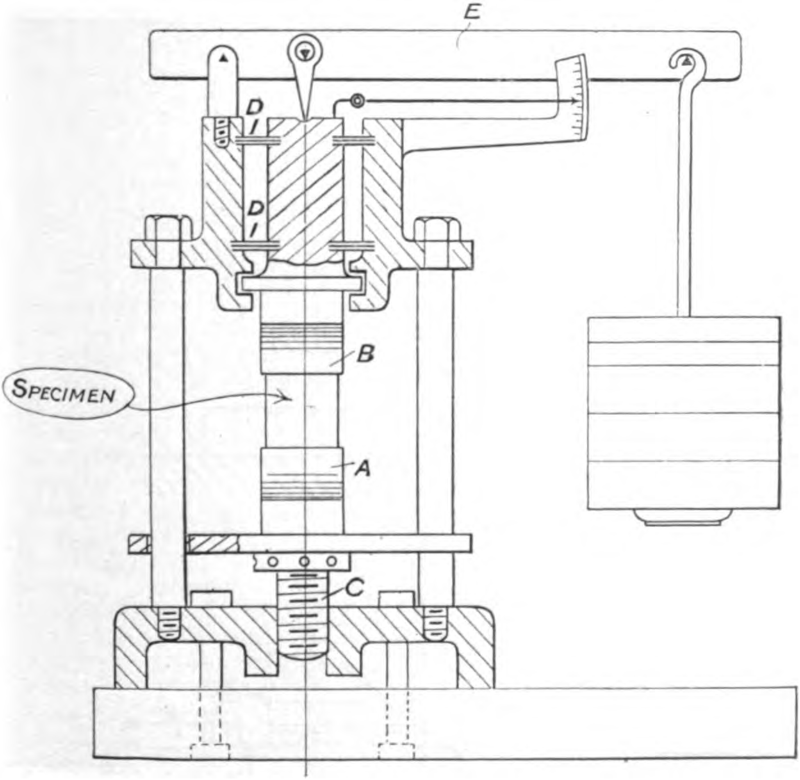


FIG. 8.

In order to facilitate the measurements it was found convenient to suspend the lateral extensometer X, from a three-point support (Fig. 9), borne by a block sliding upon the vertical pillars P_1, P_2 of the testing machine, and having two cross horizontal slides B and

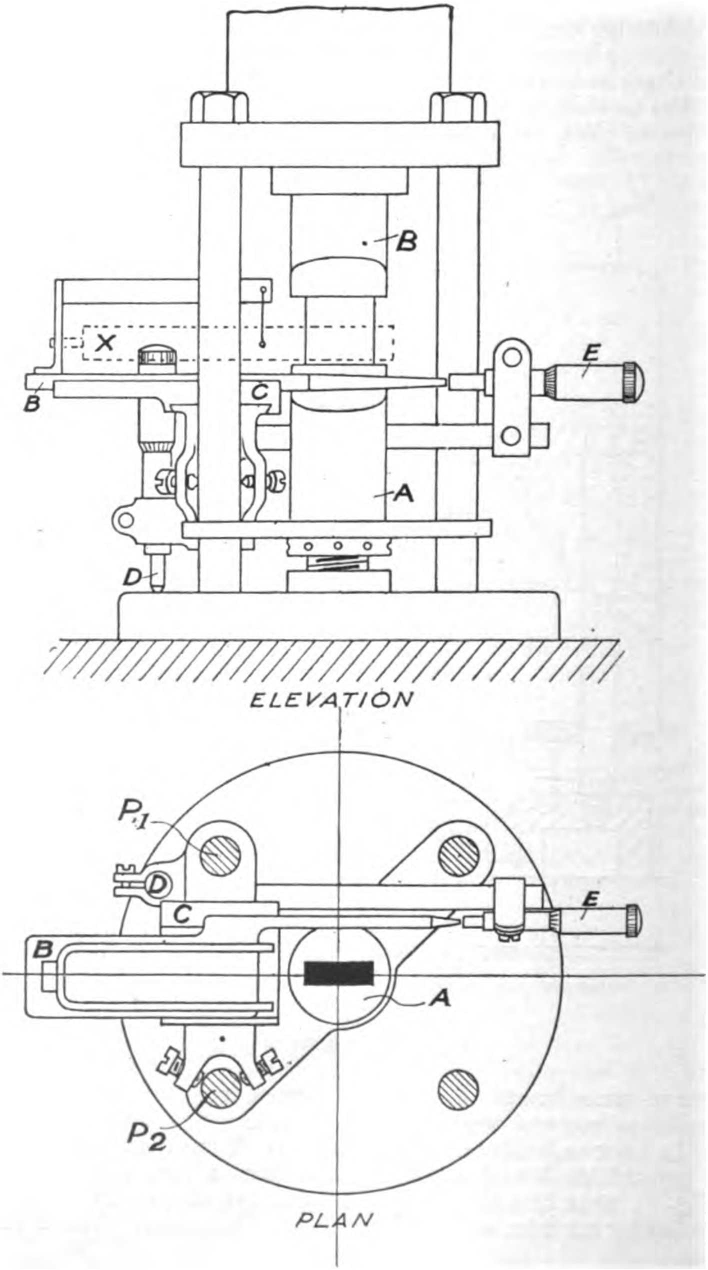

B
X
E
B
C
A
D
ELEVATION
P1
D
C
E
B
A
P2
PLAN

Fig. 9.

C at right angles, these latter being capable of adjustment by micrometer screws D and E, in such a manner that the measuring instrument could be moved to any position with respect to the specimen, and the horizontal and vertical co-ordinates determined to an accuracy of $\frac{1}{1000}$ inches.

In the photograph now shown, a block of square section is subjected to compression stress, and the optical effects show that the stress is far from uniform, although all possible precautions were taken to ensure a perfectly uniform bearing. It appears to be of much less intensity at the end faces than at the centre of the block. The measurements confirm this, and show that there is a very considerable end effect, tending to reduce the compression stress at the centres of the end faces, and only disappearing at a moderate distance away. In work of this kind where it is necessary to examine the effect of the pressure of an opaque body upon a transparent one, there is considerable difficulty in accurately measuring the stresses very close to the boundary between the surfaces.

The accompanying diagram (Fig. 10) of some preliminary measurements indicates the general character of the distribution, in which the curves denote the vertical compression stresses at various distances from the end faces.

The non-uniform character of the stress distribution, is evidently due to the way in which the pressure is applied to the material under test, and in this example the brass pressure plates prevent free lateral displacement of the material under load. If, therefore, a more extensible material is interposed for transmitting the load to the block, we may expect to obtain greatest stress at the centre, and this is actually what occurs.

A convenient material to interpose is a thin sheet of india-rubber, and the accompanying photograph of the colour effects clearly shows the widely different character of the stress distribution. The stress at the centre line is now the greatest, and it is, moreover, artificially increased 20 per cent or thereabouts, for the same total load owing to the action of the interposed film of india-rubber, as the measurements of the accompanying Fig. 11 show. It is, moreover, not a local effect confined to a small area at the ends, but exerts an effect over the greater part of the block.

The injurious effect of a lead plate, when applied to give a bearing over a block of material under test, is confirmed by this experiment, and the measurements give a quantitative estimate of the increase in stress intensity.

To ensure uniform stress conditions, the experimental results appear to point to the conclusion that the load ought to be applied through an intermediate layer of the same material, and when this is carried out, the block shows a very nearly uniform optical effect, and the measurements prove that the stress intensity is nearly uniform throughout.

Compression tests may therefore be expected to show more consistent results when the load is applied through an intermediate layer of the same material, since it is apparent that the practice of testing every kind of material in compression between hard steel face plates with quite arbitrary facing material, causes an error which varies widely in amount with different bodies, and even with different specimens of the same kind of substance.

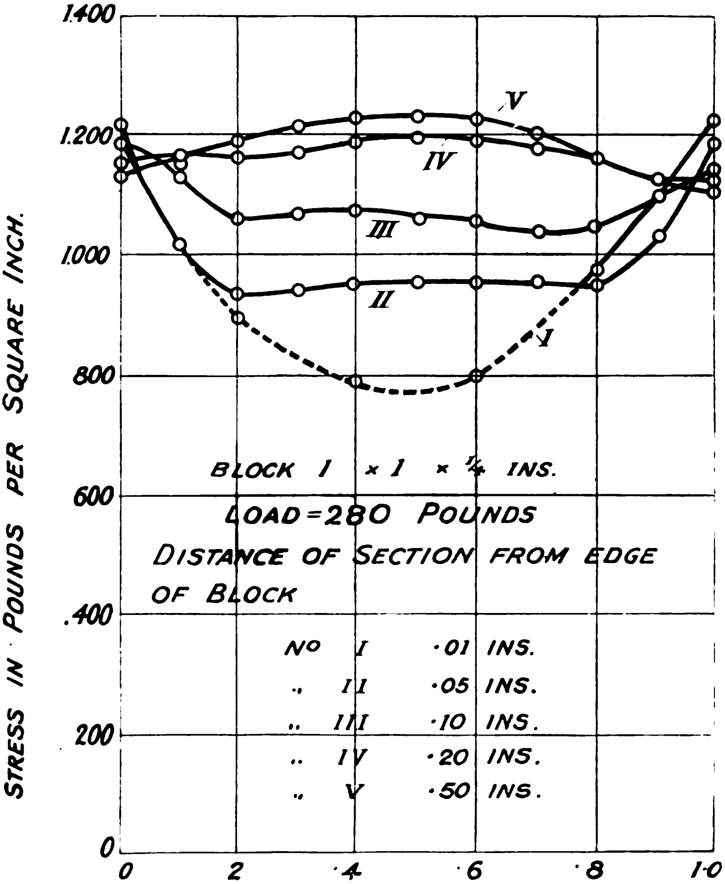


Fig. 10.

The application of optical methods of investigation to other problems which arise in the testing of materials, shows that something may possibly be gained by an examination of familiar methods of testing.

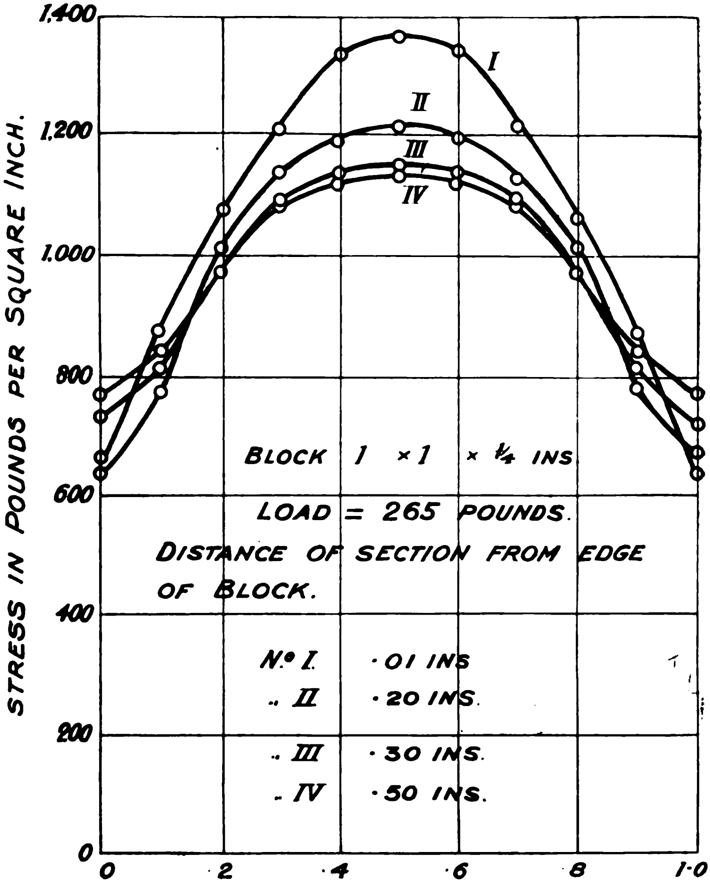


FIG. 11.

Among the cases which have been examined, is that of the usual shear test of materials, and it may be of interest to give an indication of some of the immediately obvious features.

SHEAR TESTS.

Shear tests are usually made upon bars of rectangular or circular cross sections, gripped in such a manner that shear stress is applied to two similar and equal sections at a finite distance apart, and load is applied until the material is sheared across these sections. In this way a mean value of the shear stress at rupture is obtained which is of some value, but it affords little, or no indication at all of the behaviour of the material up to the elastic limit. It seems probable that the assumption of uniform shear stress across the

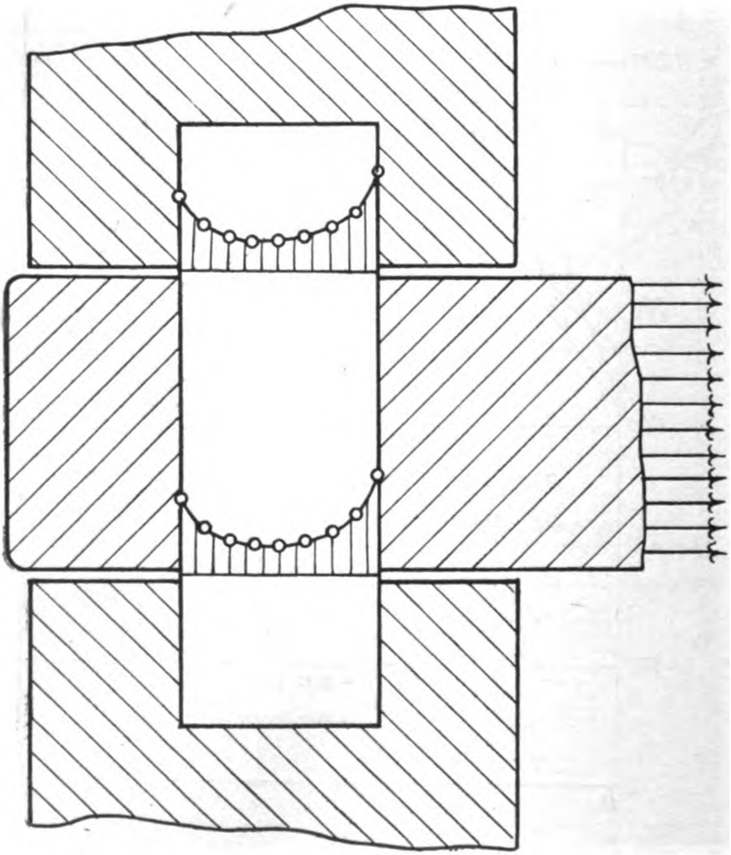


FIG. 12.

section is not justifiable, and within the elastic limit it is clearly not so, as is immediately obvious from the colour effects in the model, where stress distribution due to a shearing force applied to a rectangular section, is shown at one of the vertical cross sections.

Commencing from the left-hand edge, Fig. 12, we observe that the stress is a maximum here, and diminishes rapidly as we proceed along the section until it becomes sensibly uniform, and then as the right-hand edge is approached, the stress again rises rapidly, and reaches its greatest intensity at the lower edge. This increase of stress beyond the left-hand edge is due to the bending of the specimen in the grips, and it indicates that failure will occur here.

Measurements such as those given in Fig. 12 show that this is substantially the distribution which occurs, in a specimen of rectangular section, and they give a measure of the stress at all points of the section.

EYE-BARS.

A promising development of experimental work with polarized light, relates to the design of machines and structures, especially the detailed parts or components. It has already been pointed out that in the majority of cases the stresses in even the simplest members are so complicated as to defy exact calculation, and with the simplifying assumptions usually adopted, it is necessary to allow for faulty methods by using a large factor of safety.

We may take a very simple example in the case of a member which is merely required to transmit a pull in the direction of its length by means of pins, as shown in the diagram.

If we take a member of rectangular form bored out at each end to receive the pins, then it is at once apparent that the material around the pin is very highly stressed compared with the body of the member, and, as we have already seen in the case of the rivet, there is a compression stress of very large intensity in one region, and a tension stress of considerable magnitude in another, but in the main body of the member the section is excessive for the load applied. We have here, in fact, the problem which confronted the Deacon in Oliver Wendell Holmes' story of the wonderful "One Hoss Shay" which "ran a hundred years to a day."

> "Fur," said the Deacon, "'t's mighty plain
> Thut the weakes' place mus' stan' the strain;
> 'n' the way t' fix it, uz I maintain,
> Is only jest
> To make that place uz strong uz the rest."

In practice, therefore, the superfluous material of the main body is cut away, and a link is obtained with swelled ends, shaped to resist in the best possible way the stresses which come upon them.

The form which these ends shall take to ensure the maximum possible strength is a problem which has exercised the minds of many engineers, particularly those engaged in the construction of large span-bridges of the pin-connected type, where such members occur in considerable numbers and of great size. In fact, the main tension members of the majority of the great bridges in the United States, Canada, and Australia are simple links of this kind.

Some of the forms of ends which have been very generally used are shown on the diagram, and their diversity indicates the uncertainty which is felt as to the best possible shape; nor is it easy to devise a method of selection unless the stresses in these forms can be measured.

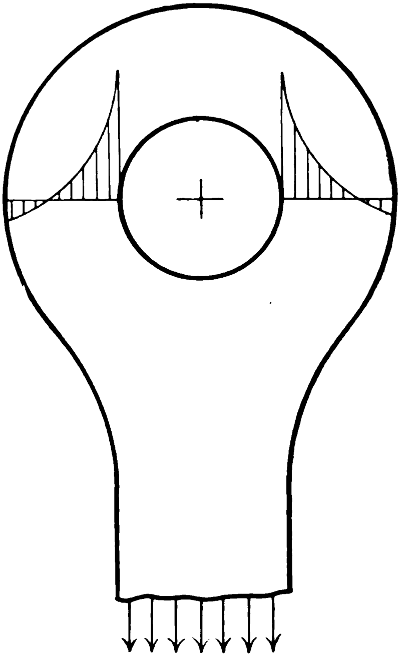


Fig. 13.

An optical investigation of a model does this effectively and quickly. A very common form is that in which the swelled end has a contour bounded by a circle concentric with the pin, and if we use proportions very widely adopted, it is easy to see from the colour effects on the loaded model now shown, that the stress distribution is unsatisfactory; in fact at the principal transverse section (Fig. 13) the normal stress is that of compression at the extreme edge, and undue stress is therefore thrown upon the remainder of the section, to balance the total pull on the member. This is also indicated by the lines of stress which have been plotted from the observations.

A much better shape is one devised by Berkeley, and the reason for its special excellence is, I think, clear from the picture of its stressed condition which is before you, from which you will see that the head is so proportioned that the principal transverse section is wholly in tension (Fig. 14) and is much more evenly distributed than before, while the elongated head allows a

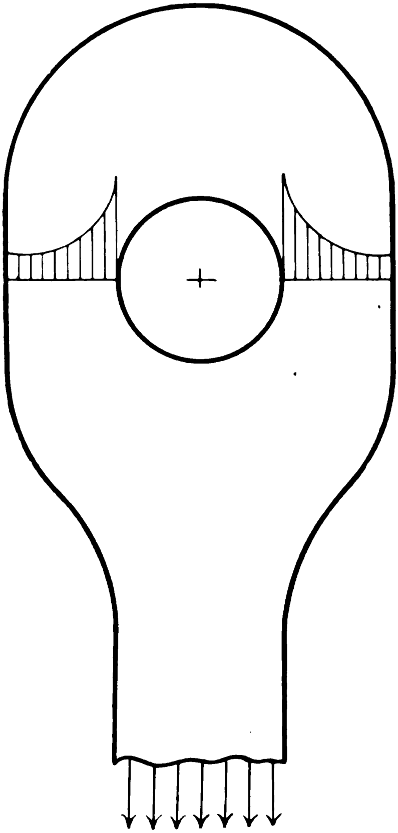


Fig. 14.

more even distribution in the longitudinal section. The contour is apparently not entirely satisfactory, as the head merges into the main body in a somewhat abrupt manner, and suggests that a more satisfactory solution would be obtained by more gradual transition curves following one of the systems of curves of principal stress in a member of rectangular form, and of considerable width. If this is carried out as shown in the next model, you will, I think, observe that the effect of this change is a beneficial one, and the lines of stress are less curved, while there are no portions of the head which may be looked upon as of doubtful utility.

Economy in the use and distribution of material to resist stresses in a structure, is clearly a most desirable end, and in perhaps no case is this more necessary than in some of the latest developments of modern engineering—the airship and the aeroplane—where weight is a most important factor; and tentative experiments upon models of links used in these structures show that some help in the solution of these new problems may possibly be afforded by optical investigations.

OVERSTRESSED MATERIALS.

The behaviour of materials which are not stressed beyond the elastic limit, suggests inquiry as to the nature of the stress distribution in a body which is overstressed at certain points. When this occurs in a transparent material, the permanent strain produces such a change in the structure that we can no longer depend upon the laws of the optical behaviour of elastic material, but some further knowledge may be gained if we take into account the stress strain relations of overstressed material. In this respect we are fortunate in having compounds of nitro-cellulose which show a marked similarity to ductile materials like wrought-iron and structural steel; in fact, the stress strain properties are very similar to those of ductile metals, and it seems a natural inference that the stress distributions under similar conditions will correspond, although the metal is a crystalline solid while the optical material is of a colloidal character.

If, therefore, the stress distribution is determined so far as the optical behaviour can be traced, the remaining effect may in many cases be determined approximately from the stress strain relations.

Usually the most convenient method of examining this latter is by the measurement of the lateral changes, and the case of a tension member with a central hole has been examined by Mr. Y. Satake and myself in the laboratory at University College. At the central minimum section, for example, the sum of all the stresses across it must equal the applied load both within and beyond the yield point of the material, and when these stresses are determined, it is found that the distribution is such that when the yield point is reached at the periphery of the hole, the stresses begin to equalize across the section as more and more load is applied. This tendency to equalization of stress beyond the yield point has also been observed in other cases of a simple character, notably in beams and eccentrically loaded tension members, and the value of ductility in preventing excessive stresses by local yielding is demonstrated.

The effects of overstress in brittle materials like cast-iron may also be studied with glass models, and the behaviour of the latter material may possibly be more representative, since its stress strain properties are very like cast-iron, and, in addition, it has also a crystalline structure.

The failure of ductile materials under stress is also interesting from another point of view, since the results of several investigators in recent years appear to demonstrate that such materials fail by shear.

As we have seen, the colours produced are proportional to the difference of the principal stresses, and, since the shear stress at the same point is one-half this difference, the images projected on a screen show the distribution of shear stress in a stressed model, and we can determine the maximum values by inspection. Further, the points where failure takes place are indicated by the transparent material becoming opaque owing to the breakdown of its structure, and these can be readily ascertained. A transparent model can be tested to failure, and, if it is weak at any point, experiment indicates the place, and in general affords some indication of the alteration needed in the design to cope with the imposed load.

It would not be difficult to supply other examples of the use of optical investigation, but the cases already described are possibly sufficient to show the use of polarized light in engineering problems of stress and strain, and to indicate the possible utility of stress pictures in other fields of applied science and industrial research.

[E. G. C.]

WEEKLY EVENING MEETING,

Friday, February 25, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Professor Sir Arthur Quiller-Couch, J.P. M.A. Litt.D.

The Commerce of Thought.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, March 3, 1916.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

PROFESSOR SILVANUS P. THOMPSON, D.Sc. LL.D. F.R.S. *M.R.I.*

Corona and other Forms of Electric Discharge.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1916.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Frederick Harrison Glew,
James Keith, Assoc.M.Inst.C.E.,
Hon. Mrs. R. C. Parsons,
Mrs. R. J. Symonds,

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti, Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXV. 1° Semestre, Fasc. 1 & 2.

Accountants, Association of—Journal, Vol. IX. Feb. 1916.

American Chemical Society—Journal for Jan.-Feb. 1916. 1916. 8vo.
Journal of Industrial and Engineering Chemistry for Feb. 1916. 8vo.

American Geographical Society—Geographical Review, Feb. 1916.

Amsterdam, Royal Academy of Sciences—Verslag: Deel XXIII. Parts 1-2, 1914-15. 8vo.
Proceedings, Vol. XVII. Parts 1-2, 1914-15. 8vo.
Verhandelingen: 2e Sectie, Deel XVIII. Parts 4-5, 1915. 8vo.
Jarboek, 1914. 8vo.

Asiatic Society, Royal—Journal for Feb. 1916. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVI. No. 3. 8vo. 1916.
Bankers, Institute of—Journal, Vol. XXXVII. Part 2, 1916. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIII. Nos. 7 & 8. 4to. 1916.
British Astronomical Association—Journal, Vol. XXVI. No. 4. 8vo. 1916.
Buenos Ayres—Bulletin of Municipal Statistics for Sept.-Oct. 1915. 4to.
Chemistry, Institute of—Proceedings, 1916, Part 1. 8vo.
Chile, Central Meteorological Institute—Publications, Nos. 10-12, 1911-13. 4to.
Editors—Aeronautical Journal for Oct.-Dec. 1915. 8vo.
  Amateur Photographer for Feb. 1916.
  American Journal of Science for Feb. 1916. 8vo.
  Athenæum for Feb. 1916. 4to.
  Author for Feb. 1916. 8vo.
  Chemical News for Feb. 1916. 4to.
  Chemist and Druggist for Feb. 1916. 8vo.
  Church Gazette for Feb. 1916. 8vo.
  Concrete for Feb. 1916. 8vo.
  Dyer and Calico Printer for Feb. 1916. 4to.
  Electrical Engineering for Feb. 1916. 4to.
  Electrical Industries for Feb. 1916. 4to.
  Electrical Review for Feb. 1916. 4to.
  Electrical Times for Feb. 1916. 4to.
  Electricity for Feb. 1916. 8vo.
  Engineer for Feb. 1916. fol.
  Engineering for Feb. 1916. fol.
  Ferro-Concrete for Feb. 1916. 8vo.
  General Electric Review for Feb. 1916. 8vo.
  Horological Journal for Feb. 1916. 8vo.
  Illuminating Engineer for Feb. 1916. 8vo.
  Journal of Physical Chemistry for Feb. 1916. 8vo.
  Journal of the British Dental Association for Feb. 1916. 8vo.
  Junior Mechanics for Feb. 1916. 8vo.
  Law Journal for Feb. 1916. 8vo.
  London University Gazette for Feb. 1916. 4to.
  Marine Engineer for Feb. 1916. 8vo.
  Model Engineer for Feb. 1916. 8vo.
  Musical Times for Feb. 1916. 8vo.
  Nature for Feb. 1916. 4to.
  New Church Magazine for Feb. 1916. 8vo.
  Nuovo Cimento for Nov.-Dec. 1915. 8vo.
  Page's Weekly for Feb. 1916. 8vo.
  Physical Review for Feb. 1916. 8vo.
  Power for Feb. 1916. 8vo.
  Power-User for Feb. 1916. 8vo.
  Science Abstracts for Feb. 1916. 8vo.
  Tchèque, La Nation, for Feb. 1916. 8vo.
  War and Peace for Feb. 1916. 8vo.
  Wireless World for Feb. 1916. 8vo.
  Zoophilist for Feb. 1916. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIV. No. 256. 8vo. 1916.
Franklin Institute—Journal, Vol. CLXXXI. No. 2. 8vo. 1916.
Geographical Society, Royal—Journal, Vol. XLVII. No. 2. 8vo. 1916.
Geological Society—Abstracts of Proceedings, No. 985. 8vo. 1916.
Imperial Institute—Bulletin, Vol. XIII, No. 4. 8vo. 1915.
Inwards, Richard, F.R.A.S. F.R.Met.Soc.—Weather Lore: A Collection of Proverbs and Sayings. 8vo. 1898.
Legge, Francis, F.S.A. M.R.I.—Forerunners and Rivals of Christianity, Vols. I. & II. 8vo. 1915.

Life-Boat Institution, Royal National—Journal for Feb. 1916. 8vo. 1916.
London County Council—Gazette for Feb. 1916. 4to.
Meteorological Office—Monthly Weather Reports for Jan. 1916. 4to.
Weekly Weather Reports for Feb. 1916. 4to.
Daily Readings for Dec. 1915. 4to.
Geophysical Journal—Daily Values for Nov. 1914.
Microscopical Society, Royal—Journal for Feb. 1916. 8vo.
Musical Association—Proceedings, Forty-First Session, 1914-15. 8vo. 1915.
New York, Society for Experimental Biology—Proceedings, Vol. XIII. No. 3. 8vo. 1915.
New Zealand, High Commissioner for—Patent Office Journal, Jan. 1916. 8vo.
Nova Scotian Institute of Science—Proceedings and Transactions, Vol. XIII. Parts 3 & 4; Vol. XIV. Part 1. 8vo. 1915.
Pharmaceutical Society of Great Britain—Journal for Feb. 1916. 8vo.
Photographic Society, Royal—Journal, Vol. LVI. No. 2. 8vo. 1916.
Royal College of Physicians—List of Fellows, etc. 1916. 8vo.
Royal Colonial Institute—United Empire, Vol. VII. No. 2. 8vo. 1916.
Royal Engineers' Institute—Journal, Vol. XXIII. No. 2, 1916. 8vo.
Royal Society of Arts—Journal for Feb. 1916. 8vo.
Royal Society of London—Philosophical Transactions, B, Vol. CCVII. No. 339. 4to. 1916.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXII. No. 2. 8vo. 1916.
Selborne Society—Selborne Magazine for Feb. 1916. 8vo. 1916.
Smithsonian Institution—Report of Secretary for Year ending June 30, 1915.
Report on the Astrophysical Observatory for Year ending June 30, 1915. 8vo. 1916.
United Service Institution, Royal—Journal for Feb. 1916. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. V. Nos. 17-19, 1916. 8vo.
Experiment Station Record, Vol. XXXIV. Nos. 1 & 2. 8vo. 1916.
United States Patent Office—Official Gazette, Jan.-Feb. 1916. 8vo.
Washington, National Academy of Sciences—Proceedings, Vol. II. No. 2, 1916. 8vo.
Western Australia, Agent-General for—Monthly Statistical Abstract for Sept. 1915. 4to. 1915.
Statistical Register for 1913 and previous years. 4to. 1915.
Yorkshire Archæological Society—Journal, Vol. XXIII. Part 92. 8vo. 1915.
Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, 1915, Heft 3 & 4. 8vo. 1915.

WEEKLY EVENING MEETING,

Friday, March 10, 1916.

The Right Hon. Lord Rayleigh, O.M. P.C. LL.D. D.Sc. F.R.S.,
in the Chair.

Sir Napier Shaw, M.A. LL.D. Sc.D. F.R S. *M.R.I.*,
Director of the Meteorological Office.

Illusions of the Upper Air.

When the year of grace 1916 comes to an end half a century will have elapsed since a daughter was born to Neptune in the temple of Athene. Her name was the Meteorological Committee of the Royal Society. In more conventional language, the Board of Trade, acting on behalf of the Government, with the concurrence of the Admiralty, came to an understanding with the Royal Society for the scientific study of the weather in the interest of shipping for the benefit of the public and at its expense. I propose to review the progress of meteorological theory in England in the fifty years.

The Formation of the Meteorological Committee of the Royal Society.

By that understanding two lines of investigation, the study of weather-maps on the one hand, which was a public duty, and, on the other, the study of automatic meteorological records, which was a private enterprise, were brought together.

The public duty belonged to the Meteorological Department of the Board of Trade, which had been created in 1854, on the recommendation of an international maritime conference, initiated by Lieutenant Maury, of the United States Navy, for the study of the Meteorology of the Sea. It was dependent for its funds partly upon the Admiralty in the interest of the Navy, and partly upon the Board of Trade in the interest of the Mercantile Marine. To its original programme of compiling statistics of all oceans for the use of sailors had been added, through the instrumentality of the British Association, with the Prince Consort as President, the collection of daily reports by telegraph from stations on the British and French coasts for the study of weather, particularly of cyclonic storms, which caused great damage to shipping.

Thus, while the public meteorological services of most countries have their roots in the study of rainfall, and the interests of agriculture or water-supply, our own is to be referred to the study of winds, and the interests of the seafaring community—to Neptune, in fact. In this country the study of meteorology in relation to rainfall and climate was left to private enterprise. While Admiral FitzRoy, who was in charge of the Department of the Board of Trade, was busy with meteorology for sailors, Mr. G. J. Symons, a member of his staff, set up an independent organisation for the study of British rainfall, which has proved itself a practical necessity for engineers and others interested in water-supply; and the British Association had made an effective physical observatory out of the derelict Observatory at Richmond, which George III. had built to replace the Royal Observatory at Kew, and which had been lent by the Crown to the British Association in 1842.

The men who were prominently associated with the work of the Observatory fifty years ago were Sabine, De la Rue, Gassiot, Galton and Balfour Stewart, and the most noteworthy contribution of the Observatory to meteorology was the perfecting of the automatic registration of the meteorological elements pressure, temperature, wind and rainfall. FitzRoy felt impelled to make public the interesting things that came under his notice when he plotted telegraphic reports on a map, and in like manner the meteorological enthusiasts, who were at one time of the year the British Association, and during the rest of it the Royal Society, were impelled to maintain that the proper way of arriving at a comprehension of the weather-map was by a study of such records as those of Kew Observatory.

Rival Weather Maps.

There were, in fact, in the sixties three meteorological parties in this country, the Official party, if an official can be said to be or to have a party, the Rainfall party and the Kew Observatory or British Association-Royal Society party. All three showed a lively interest in the weather map, and were actively engaged in devising means for representing the geographical distribution of weather.

The evidence of their activity survives in the proposals for weather maps of various kinds which appeared in the year 1861. The first is FitzRoy's representation of the weather of the Royal Charter storm of 1859, in which the variations of pressure and temperature are shown by ordinates set up either along parallels of latitude or along radial lines on the map; the second is the prospectus of "The Daily Weather Map Company, Limited," which is curiously enough without any date, but which is generally associated with Symons and Glaisher; and the third, Galton's method of representation propounded in a work called "Meteorographica," in which the word "anticyclone" was used for the first time.

The differences between these three attempts to represent the weather have really more to do with typography than with meteorology. Gradually the use of isobars introduced by Leverrier came in; they and their reproduction by lithography have now become universal, and the weather maps of all countries use a common form with only slight variations in detail.

The Study of Cyclones and Anticyclones.

In 1866, a year after FitzRoy's death, when my present story begins, the Kew party had made good its claim, and the Royal Society had undertaken by means of the new Meteorological Office to establish seven other Observatories in various parts of the country just like the one at Richmond, and to use the automatic records in explanation of the weather as set out in the daily maps. The explanation of the winds and the interest of the sailor were still the justification of the public expenditure.

Meteorologists knew about cyclones from Piddington in 1848, and about anticyclones from Galton in 1863; and from that time onwards until the end of the century the study of cyclones and anticyclones was the dominant idea of dynamical meteorology.

It was mainly conducted by observations at the earth's surface, and necessarily so. In 1852 Welsh, the superintendent of Kew Observatory, had made four sets of excellent observations of the upper air in balloons, and Glaisher had followed them up by a large number of ascents for the British Association, which reached their climax in the famous ascent with Coxwell in 1862. They added a good deal to our knowledge but very little to our ideas. They told us that the atmosphere showed continual decrease of temperature with height, and that surprised nobody; it was a natural incident in the gradual transition from the temperature of the surface of the earth to the absolute zero of space. "The nicely calculated less or more" was not of vital importance. Cyclones and anticyclones obviously belonged to the upper air, the regions where clouds are formed and dissipated, where rain and snow and hail are produced, but balloon ascents told us little about them beyond confirming the surmise that there are great ascending currents associated with certain forms of cloud.

The only real information to be got about the atmosphere in upper regions was that contained in observations of pressure at the surface, which is the cumulative result of the whole thickness of the atmosphere, and the amount of rain, hail or snow which was noted as falling from above. There were also observations of the forms of cloud and their motion, and, if we please, of their position. The rest is necessarily speculation, so that out of these observations meteorologists were obliged to imagine for themselves what cyclones and anticyclones are, how far up they extend, how they are produced and maintained, what kind of air they are made of, and so on.

Observations of the Upper Air.

Speculation can do a good deal with the atmosphere. It goes beyond the reach of our balloons, and tells us of the substitution of hydrogen and the rarer gases for oxygen and nitrogen in the region of the meteor and the solar electron. But from the year 1896 onwards there has been a systematic collection of facts about the upper air by using kites to carry instruments up to heights of 3 kilometres or occasionally more, *ballons sondes* which carry instruments up to heights of 35 kilometres—20 miles or more—and pilot balloons which give the direction and velocity of the wind at various levels up to 10 kilometres, or sometimes more.

Comparison of Fact with Speculation.

This investigation has given us a wealth of information about the upper air. The principal result is the division of the atmosphere into two layers, a lower layer about 10 kilometres thick, the troposphere, the region of convection; and an upper layer, the stratosphere, where there is no convection. We can use the information to test some of the generally accepted ideas about cyclones and anticyclones, and to compare the results of speculation with the new facts. Many of the pictures which we imagined now appear to have been illusions. Those of us, for example, who thought that because the air was warmed from the bottom the upper part would be free from sudden changes of temperature such as we get at the surface were rapidly and rudely disappointed. Fig. 1 illustrates the disillusionment. Simplicity is not apparently the characteristic of the upper air.

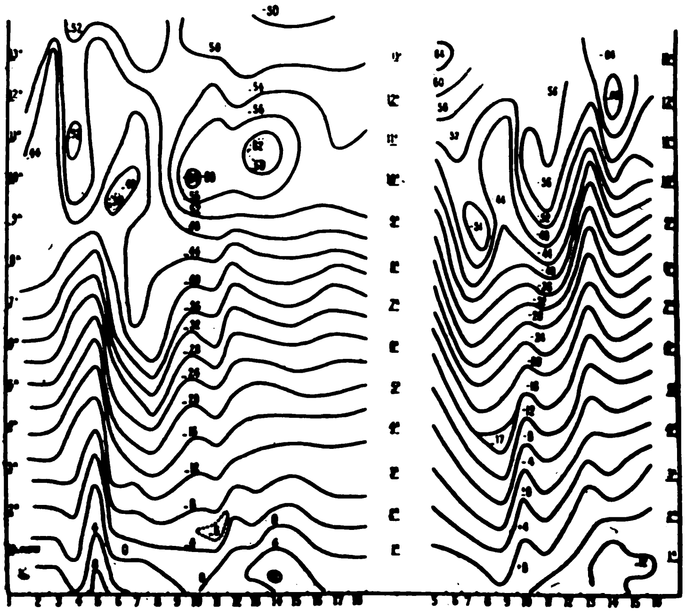
The Convection Theory of Cyclones and Anticyclones.

Before giving you other examples let me quote the description by which Galton introduced the name "anticyclone," because the mental picture of the structure of cyclones and anticyclones, which has guided the thoughts of the majority of meteorologists, has been formed by the gradual elaboration of the ideas contained in that description :—

"Most meteorologists are agreed that a circumscribed area of barometric depression is usually a locus of light ascending currents, and therefore of an in-draught of surface winds which create a retrograde whirl (in our hemisphere).

"Consequently, we ought to admit that a similar area of barometric elevation is usually a locus of dense descending currents, and therefore of a dispersion of a cold dry atmosphere, plunging from the higher regions upon the surface of the earth, which, flowing away radially on all sides, becomes at length imbued with a lateral motion due to the above-mentioned cause, though acting in a different manner and in opposite directions." *

Out of that there gradually grew the conception, on the one hand, of the central area of a cyclone on the map as a centre of centripetal motion, a focus of attraction for the surrounding air, and of the general area of the cyclone as a region of ascending warm air producing rain or snow; round the central region the air moves inward



ISOTHERMES DE L'ATMOSPHÈRE LIBRE
AU DESSUS DE LA RÉGION DE PARIS
PAR M L TEISSERENC DE BORT

DU 3 AU 18 MARS 1903 DU 5 AU 16 MAI 1904

FIG. 1.—ISOPLETHS OF HEIGHT (in kilometres) AND TEMPERATURES (in degrees centigrade) for sixteen days in March 1903 and twelve days in May 1904. The sequence of temperatures in the upper regions is not quite the same as at the surface, but there is no suggestion of changes becoming less pronounced aloft.

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* Proceedings of the Royal Society, 1862–1863, vol. xii, p. 385.

with a counter-clockwise motion in spiral curves. On the other hand, the conception of the central area of an anticyclone is of a centre of centrifugal motion, a region of repulsion; the general area of an anticyclone as a region of descending cold air which moves with a clockwise motion spirally outwards. The fundamental dynamical idea is that of air driven, like gas along a pipe, from high pressure to low pressure, retarded by the friction of the surface, and diverted from its direct object by the rotation of the earth.

For future reference let us separate the three elements of this picture, and keep them distinct.

First, the *circulation*—counter-clockwise in a cyclone, clockwise in an anticyclone.

Second, the *convergence* across the circulation from high to low.

Third, the *convection* or vertical motion which appears as ascending air in the cyclone, and descending air in the anticyclone.

According to the conception which developed on the lines of Galton's words, and which found ready acceptance, the circulation is incidental to the convergence, the convergence is universal, the convection general.

It is another example of the *facilis descensus Averni.* The very simple piecing together of the three parts makes it almost obvious that the third element, the convection, is the effective cause of the whole dynamical process; it is natural to regard convection as the ascent of warm air in a relatively cold environment causing low pressure on account of the relatively high temperature of the ascending air; and high pressure as the natural corollary of cold descending air. The convergence or motion across the isobars is the primary result of the motion due to the distribution of pressure, and the circulation is merely the deviation from the straight path caused by the rotation of the earth. The theory is quite simple and quite self-contained, and it has this great advantage, that the cause which it assigns for the cyclone, namely, the convection of warmed air, has always been regarded as the cause of winds—it has been accepted as explaining the trade winds and the monsoons, and if it is also accepted as explaining the cyclone and anticyclone, which are the modern meteorological names for the diverse winds of the temperate latitudes, we can see in the idea a beautiful unity in meteorological theory. The origin of all the winds is thereby assigned directly to what we know must be their ultimate cause, namely, the warming of the lowest layers of the air by the warmed surface of sea or land. If we doubt its efficiency in one case there seems no good reason for holding to it in the others.

It seems a pity that an illusion which apparently does such good service should be shattered, but it cannot face the facts of the upper air.

You will notice that the whole matter depends upon the idea of the low pressure in the warm ascending air of the cyclone as the driving force, whatever be the area covered by the circulation. The observations of the upper air have made us familiar with certain facts about the height of the atmosphere that make such an idea too improbable. The convective atmosphere is only about 10 kilometres thick. The region in which convection can operate is therefore a thin skin represented by a centimetre in the case of a map on the millionth scale, on which 1000 miles is about 6 feet in length. A cyclone is often regarded as a towering structure which may produce curious effects by tilting its axis, but that is clearly illusory; the idea that descending air over Northern France is operating in conjunction with rising air over Iceland to produce a flow of air along the line joining them is an unproductive way of representing the facts.

The idea of the ordinary cyclones and anticyclones in our latitudes as foci of centripetal and centrifugal motion is an illusion. In all ordinary cases of cyclone the convergence of the paths of air towards the centre is itself an illusion, because the motion of the cyclone makes them miss their apparent aim, and we get in actual fact paradoxical cases of air which, always seeking a place of lower pressure, yet makes its way to a place of higher pressure, because the pressure has been raised over its path; and though it always seeks the centre, in reality it goes further away from it. If it wanted to reach it, it was a mistake to aim at it; if it wanted to get near, it should have aimed to get away. There certainly is convergence and convection, but it is local and not general over the cyclone. The idea which is conveyed by convergence in spiral paths to the centre of a moving cyclone is an illusion. It did not even require observations of the upper air to tell us that.*

Take the time required for the operating forces to produce any such wind velocities as we find in actual experience. In one hour an ordinary pressure-difference would produce a velocity of 1000 metres per second, if it were free to act. The time required to generate a velocity of, say, 10 metres per second, is infinitesimal compared with the time during which we see the forces in operation: these last for hours, or even days, while a minute would suffice for the production of all the velocities exhibited; the motion of the air which we register on anemometers is not accelerating motion but uniform motion, except for the effect of turbulence and local convection; so we must picture to ourselves the air of cyclones as being under the operation of balanced forces, not unbalanced forces. I wish to suggest that the idea of air being accelerated by the forces we see on the map is another illusion, so far as the upper air is concerned.

The ostensible reason for supposing that the distribution of pressure created by convection is pushing air from high to low

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* See "Life-History of Surface Air-currents," by W. N. Shaw and R. G. K. Lempfert, M.O. publication, No. 174.

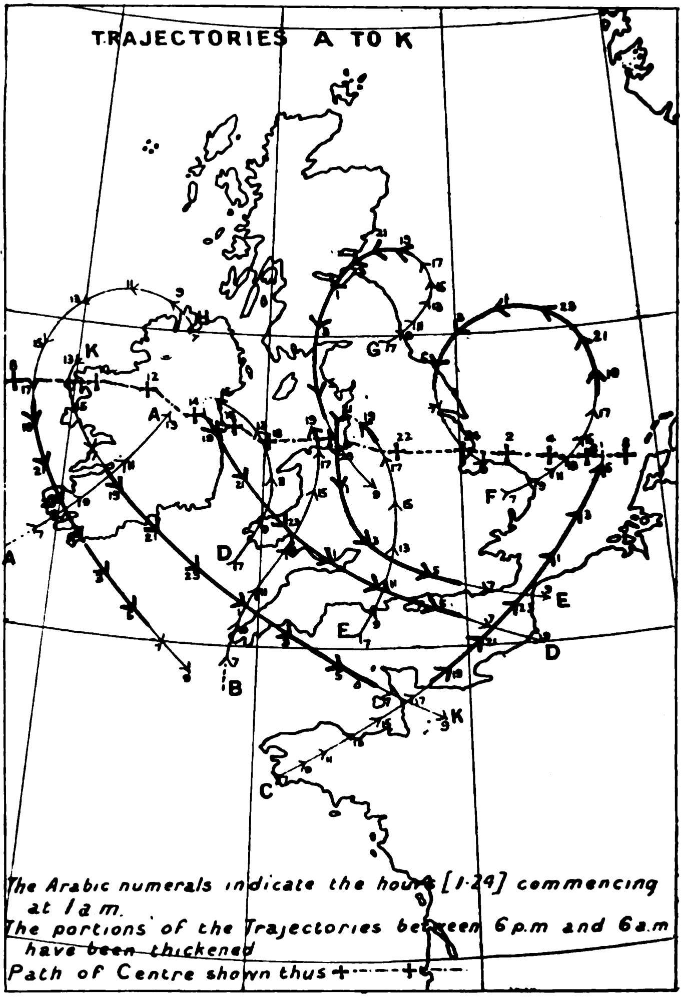
TRAJECTORIES A TO K
A
B
C
D
E
F
G
K
The Arabic numerals indicate the hours [1-29] commencing at 1 a.m.
The portions of the Trajectories between 6 p.m and 6 a.m have been thickened
Path of Centre shown thus +-----+-----

Fig. 2.—Trajectories showing the paths of air over the surface in the travelling depression of March 24-25, 1902.*

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* Figs. 2 and 3 are to illustrate the progress of air over the surface, and the changes in the velocity, pressure and temperature of the air in its motion during the passage of a well-formed cyclonic depression on March 24-25, 1902. From Fig. 2 we see that only one path, that marked C, led actually to the centre; and from Fig. 3 we note that in eight hours the wind velocity increases from ten miles an hour to twenty-seven miles an hour with diminishing pressure; in four hours more of still falling pressure the velocity falls to fourteen miles an hour, then in six hours it rises with rising pressure to thirty-six miles per hour and then slackens to thirty-one.

is due to the fact that the charted winds show the air at the surface crossing the isobars from high to low; the observations with pilot-balloons suggest that the effect is peculiar to the surface. If the driving force from high to low were the operative force which produces the wind of a cyclonic depression we should expect to find its operation more strongly marked as we get higher up, because the friction of the surface would not interfere with it, but the fact is quite otherwise. The movement across isobars becomes less and less marked as we ascend: it is relatively much less at Pendennis Castle than it is at Falmouth Observatory a mile away. We cannot be sure that it exists at all at 1500 feet, because we cannot draw the isobars at that level with the necessary accuracy; the consensus of our observations goes to show that there is no real evidence of convergence at that level. There the centrifugal force of the air travelling over the moving earth, combined with the centrifugal force due to the curvature of the air's path, are sufficient to balance the force due to pressure, and there is no component of motion towards the centre.*

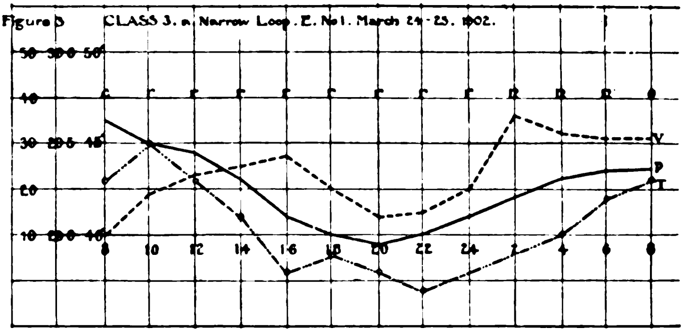

Fig. 3.—Changes in Velocity (V), Pressure (P) and Temperature (T) of the air moving along the path marked E, of Fig. 2.

What happens nearer the surface is that the friction of the surface converts part of the energy of the motion of the wind into eddy motion, and the air does not move fast enough on the right path to keep up the balance. Consequently it drifts inwards as a pendulum does when its motion is retarded, but the lower air cannot hold back the air far above it; the effect of viscosity in that direction was shown by Helmholtz to be negligible. The effect of eddy-motion is very limited in height.

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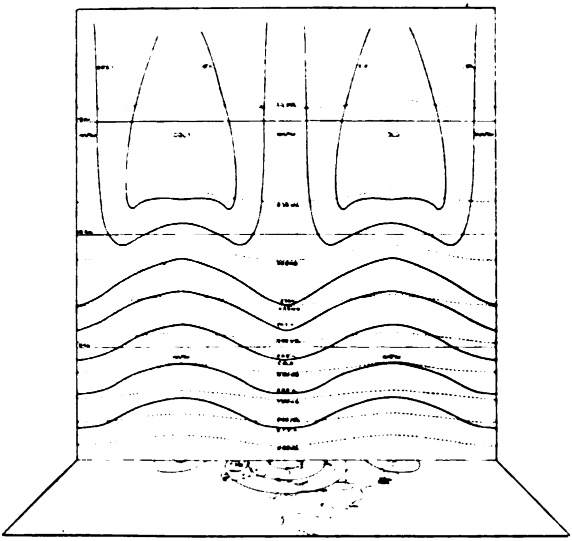
* See the four reports on wind structure to the Advisory Committee for Aeronautics by W. N. Shaw and J. S. Dines.

Observations in the Upper Air in Relation to the Convection Theory.

But the greatest blow to the illusion that I have portrayed comes directly from the observations of the upper air; the convection theory requires that the air of the cyclone should be warmer than that of the anticyclone, but, as a matter of fact, the new observations show that the opposite is the case.

DISTRIBUTION OF TEMPERATURE AND PRESSURE IN CYCLONES AND ANTICYCLONES
MEAN VALUES AFTER W. H. DINES.



SOLID CURVES INDICATE TEMPERATURE IN DEGREES ABSOLUTE [273° A. BEING THE FREEZING POINT OF WATER]
BROKEN CURVES INDICATE PRESSURE IN MILLIBARS [1000 BEING THE C. G. S. ATMOSPHERE]

FIG. 4.—DIAGRAM showing the normal distribution of temperature and pressure in the upper air in relation to the distribution of pressure at the surface, which is indicated by isobars on a foreshortened map at the foot of the diagram.

In a paper published by the Royal Society Mr. W. H. Dines* gave the mean values of the observations of temperature in the upper air of this country arranged according to the pressure at the ground. From his results the following table has been compiled. The figures show that a pressure-difference of 26mb. exists at the level of 10 kilometres where convection has ceased to exist. The difference is accentuated to the extent of 21mb. as the surface is reached, by the existence of the high pressure transmitted from above in spite of the relative coldness of the air at the lower pressure. The diagram included in Mr. Dines's paper, which is reproduced with some modifications in Fig. 4, showed that there is a remarkable change at the top of the troposphere. Above the level for which values are given in the table *the high is colder than the low*, *reversing the state of things in the troposphere.*

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* See M.O. publications 210b, Geophysical Memoirs, No. 2.

TABLE of Average Values of the Pressure, Temperature and Density of Air in a Region of High and of Low Pressure.

Height.		High Pressure.			Low Pressure.		
		Pressure.	Temp.	Density.	Density.	Temp.	Pressure.
1000-ft.	k.	mb.	A.	g/m³	g/m³	A.	mb.
32·809	10	273	226	421	382	225	247
29·528	9	317	233	474	444	226	288
26·247	8	366	240	531	514	227	335
22·966	7	422	247	595	583	232	388
19·685	6	483	254	662	652	240	449
16·406	5	552	261	736	724	248	516
13·124	4	628	267	818	807	255	591
9·843	3	713	272	911	893	263	675
6·562	2	807	277	1012	992	269	767
3·291	1	913	279	1137	1100	275	870
0	0	1031	282	1270	1220	279	984

We cannot resist the conclusion that the pressure-differences of cyclone and anticyclone are not local surface effects at all; we must seek their origin in the upper air, where there is no convection. They are little affected by the lower stratum of 9 kilometres, which roughly marks the range of the effect of heating at the surface.

The idea of warm air in the lower layer causing the low pressures which are recorded on our barometers is therefore an illusion.

Thus it will be seen that the observations of the upper air have proved that all the vital parts of the facile description which was the accepted theory of cyclones and anticyclones are quite illusory. What it took for guidance in forming a picture of the structure was the accidental character of motion near the ground. We now feel that the motion of air in the lowest kilometre had better be disregarded, or, better still, be handed over to students of turbulent motion, while we, as meteorologists, consider the normal state of the atmosphere as motion under balanced forces. Instead of a natural flow from high pressure to low pressure we have a natural flow without any change of pressure; the motion of a heavenly body round its sun is taken as a type for the air instead of the motion of a falling stone.

While we are considering illusions, let me add another example depending upon what was at one time, and possibly is still, a commonplace of physical teaching in regard to the relation of barometric changes to weather.

It is this: moist air is lighter, bulk for bulk, than dry air, and consequently pressure is low where the air is moist. That is why a low barometer is indicative of rain; the moist air causes the low pressure. This is not true to fact. Mr. Dines has recently examined the correlation between the humidity of the troposphere and the pressure at the surface—the coefficient is quite insignificant; there is no relation between moist air and low pressure on the map.

Structure of the Atmosphere according to the Observations of the Upper Air.

But if the ideas which were common in meteorological practice fifty years ago are now to be regarded as illusory, let us consider what we have in their place. We go back to the three elements, the circulation, the convergence and the convection. As to the circulation, we now think of it as it is exhibited in the upper air, and instead of regarding it as an incidental disturbance of the motion from high to low, we regard it as the foundation of atmospheric structure; as the motion of air which is persistent because the pressure-gradient is balanced by the centrifugal action of the earth's rotation, which we may call the geostrophic component, and of the curvature of the path over the earth's surface, which we may call the cyclostrophic component. If the balance between velocity and pressure is not perfect the difference from perfection can be only infinitesimal, because in the free atmosphere the air must always begin to adjust itself to the strophic balance from the moment that any infinitesimal change becomes operative and the power of adjustment arising from the extreme mobility of the air prevents any finite perturbation being set up, except temporarily in those regions where violent convection is operative. It is only through the mobile air that perturbation can be transmitted. We no longer picture to ourselves the air as being somehow held firm without moving until a pressure distribution is set up and then let go; the first symptom of pressure-difference will be the occasion of motion, the distribution and velocity grow together; they adjust themselves automatically. The whole history of the general motion of the atmosphere is the story of the constant pursuit of the strophic balance, the adjustment of velocity to pressure, constantly disturbed by infinitesimal changes.

Near the surface things are much more complicated, because there is turbulence due to the interference of the surface, and the obstacle which it offers to the steady progress of air. The air loses some of its motion, and is exposed to the pressure without the velocity that is required to balance it. It must, therefore, fall away towards the low pressure, taking out of the pressure the energy necessary to provide for the loss by friction. Thus the convergence which we have to account for is only that shown near the surface within half a kilometre. We need not trouble ourselves about a supposed convergence and convection over the whole area in the upper air. The second element of our specification disappears. After years of contemplation of the motion of the air from high to low as produced in a quiescent atmosphere by the operation of pressure-difference and kept within bounds by friction, we now regard the motion from high to low as actually caused by the friction which retards the velocity required to maintain the strophic balance. To base the theory of motion of the upper air upon the idea of a given distribution of pressure setting a quiescent atmosphere in motion is as great an error as to begin the lunar theory by supposing the moon to start from rest under the force of the earth's attraction, and only to find out after it had started that the earth was moving.

As to convection, there is certainly convection wherever there is the juxtaposition of air of different densities; it takes a great variety of forms, it is very common in cyclones, but it is not a necessary attribute of them. Possibly it is set up there more easily because the air travels so much faster in cyclonic areas than it does in anticyclones, and adjoining localities are fed from different sources of supply. The convection is probably the one disturbing cause of the strophic balance of velocity and pressure. So we regard the troposphere as a layer of about 9 kilometres thick, always striving to arrange its motion according to the pressure, and perpetually baffled in its endeavours by the ubiquity of convection. Since all the changes proceed by infinitesimal steps, there is never a time when we can identify a state of finite divergence from the balance between velocity and pressure. From this point of view the centre of a cyclonic or anticyclonic system has no special dynamical importance. It becomes a notable feature on the map, when for any reason the cyclostrophic component is the chief element in balancing the pressure. That is seldom the case in our maps, which more often consist of isobars of complicated shapes.

The Dominance of the Stratosphere.

Further than this, Mr. Dines has thrown a new light upon the origin of differences of pressure at the surface by obtaining the correlation co-efficient between corresponding deviations of pressure from the normal at the level of 9 kilometres and at the ground, and has obtained results ranging from 0.67, for the last available set of a hundred soundings on the Continent, to 0.88 for soundings in England grouped for the winter season. Moreover, the standard deviations are of the same order of magnitude at both levels, that is to say, both levels are subject to similar changes. At the same time, the correlation co-efficient between the pressure at the surface and the mean temperature of the 9-kilometre column is small; in other words, the temperature of the lower strata of the atmosphere has on the whole little to do with the general distribution of surface pressure in this country. Its effects are local.

We must, therefore, regard the general flow of air, except in so far as it is disturbed by convection, as governed not by what happens at the surface but by what is imposed upon it from the stratosphere above. It is from there that the general control of the distribution of our pressure comes. It is only modified by what happens below. The upper air, the stratosphere, is the operator, and the lower air the subject operated on. After fifty years of strenuous endeavours to regard the surface as the operator and the upper air as the subject, the exchange of rôle is very disturbing, but it has its compensations. There are many things which can easily be explained by operations from above, but only with the greatest difficulty by operations from below. Let us indulge in some speculations which follow from supposing that the stratosphere operates upon the troposphere. It makes the troposphere as tuneful as an organ under the alternating rarefaction and compression caused by the changes in the stratosphere. Every cloud is an expression of its action. One can imagine them being developed, like photographic plates, showing first the region of greatest humidity, and then developing further into loci of instability, and remembering each several cloud means the disturbance of the normal circulation, the condensation will alter locally the horizontal distribution of temperature and therefore that of the pressure and wind. On the table are two autochrome photographs of the Western sky at Ditcham Park with a quarter of an hour's interval, taken on a September evening in 1911, with gradually reddening clouds that gradually vanished as they approached from the West. Nothing could be more attractive than to speculate upon such changes in relation to the changes of pressure in the stratosphere.

The Régime of the Stratosphere.

But our new point of view only shows our problem removed one step further; we have now to begin again and imagine for ourselves what is the régime of pressure and winds in the stratosphere until the enterprise of meteorologists completes our knowledge of what it actually is. The problem is at any rate much simplified because convection is avoided; we deal with an atmosphere which, being nearly isothermal, is inherently stable, density goes directly with pressure, layer lies on layer like a light liquid on a heavy one—temperatures are uniform, or very nearly so, in the vertical direction, and therefore isotherms are also isobars, and winds are proportional everywhere to pressure-differences, that is, to temperature-differences. The rotation of the earth secures that air always moves along the lines of pressure, keeping high pressure or low temperature on the right. So the general idea is simple, but whether the streams of air are long straight currents or local whirls we do not yet know.

Numerical Calculations.

Speculations of a qualitative character are apt to lead the speculator into serious error; the real test of any physical theory is its quantitative application.

It will be of great advantage to the further development of our ideas if we can trust implicitly to the hypothesis of pressure balanced by motion (let us call it the principle of strophic balance) as the foundation of the structure of the atmosphere, and that hypothesis will be confirmed in the orthodox scientific manner if the quantitative conclusions to be drawn from it are verified by observation. I propose to ask your attention to some applications which can be tested numerically.

From this point of view the theory of strophic balance has the great advantage of giving a definite relation between wind velocity, pressure and temperature, and therefore brings the relations between all these quantities within the region of arithmetical computation.

Let us consider some of these relations. We require a number of symbols for the meteorological quantities.

p	represents	the atmospheric pressure.
θ	„	the atmospheric temperature.
ρ	„	the atmospheric density.
l	„	horizontal distance.
h	„	vertical height.
$s \left(= \frac{dp}{dl}\right)$	„	the horizontal pressure gradient.
$q \left(= \frac{d\theta}{dl}\right)$	„	the horizontal temperature gradient.
v	„	the velocity of the wind.
$R = p / (\rho\theta)$	„	the constant of the gas equation.

Certain geodesic quantities also come in, viz :—
E the radius of the earth.
g the acceleration of gravity.

r the angular radius of a small circle on the earth's surface which indicates the path of air in a cyclone.

λ the latitude of the place of observation.

ω the angular velocity of the earth's rotation.

We require also some convention as to the positive and negative of v:—

v positive represents the wind when the pressure difference Δp represents higher pressure on the right of the path.

The fundamental relation between the velocity of the wind at any level and the pressure-gradient there is

$$s = \frac{d\,p}{d\,l} = 2\,\omega\,v\,\rho \sin\lambda \pm \frac{v^2}{\mathrm{E}}\,\rho \cot r \quad . \quad . \quad (\mathrm{F})$$

The two terms which make up the right hand side of this equation are of different importance in different places and circumstances; for example, if the air is moving in a great circle r is 90° and cot r is zero; the first term alone remains. On the other hand, at the equator the latitude $\lambda = 0$; sin λ is zero and the second term alone remains. Away from the equatorial region the second term is also relatively unimportant unless the velocity v is great. In temperate and polar latitudes the path of the air differs little from a great circle, except in rare cases, near the centres of deep depressions; consequently the first term may be regarded as the dominant term in these regions.

We call the wind, computed according to the first term, the geostrophic wind, and regard it as generally representing the actual wind of temperate and polar regions.

We call the wind, computed according to the second term, the cyclostrophic wind, and regard it as representing the actual wind (in so far as there is any regular or persistent wind at all) in the equatorial regions. It represents the wind of tropical hurricanes, and winds of the same character may also occur locally in temperate regions as tornados and other revolving storms.

Thus we have the following auxiliary equations :—

Horizontal gradient of pressure

$$s = \frac{d\,p}{d\,l}.$$

Horizontal gradient of temperature

$$q = \frac{d\,\theta}{d\,l}.$$

Winds of temperate and polar regions, geostrophic winds

$$s = 2\,\omega\,v\,\rho \sin\lambda \quad . \quad . \quad . \quad (1)$$

Winds of equatorial regions, cyclostrophic winds

$$s = \rho \frac{v^2}{\mathrm{E}} \cot r \quad . \quad . \quad . \quad . \quad (2)$$

The measurement of pressure

$$\frac{dp}{dh} = -g\rho \quad . \quad . \quad . \quad . \quad (3)$$

The gaseous laws (assumed for dry air)

$$p = \mathrm{R}\rho\theta \quad . \quad . \quad . \quad . \quad . \quad (4)$$

From these, by simple manipulation, I have deduced the following :—

For change of pressure-gradient with height

$$\frac{ds}{dh} = g\rho\left(\frac{q}{\theta} - \frac{s}{p}\right) \quad . \quad . \quad . \quad . \quad (\mathrm{A})$$

For change of wind velocity with height, geostrophic winds

$$\frac{dv}{dh} = \frac{v}{\theta} \times \frac{d\theta}{dh} + \frac{g}{2\,\omega \sin\lambda}\,\frac{q}{\theta} \quad . \quad . \quad . \quad (\mathrm{B})$$

Cyclostrophic winds

$$\frac{dv^2}{dh} = \frac{v^2}{\theta}\,\frac{d\theta}{dh} + \frac{g\,\mathrm{E}\,q}{\cot r\,\theta} \quad . \quad . \quad . \quad (\mathrm{C})$$

DEDUCTIONS FROM THE THEORY OF EQUIVALENCE OF PRESSURE AND WIND.

These equations serve to explain the following facts established by observation[*] :—

1. *Light winds in the central region of an anticyclone.*

It follows from the fundamental equation F, when the negative sign is taken, as it must be for an anticyclone, that the values of v

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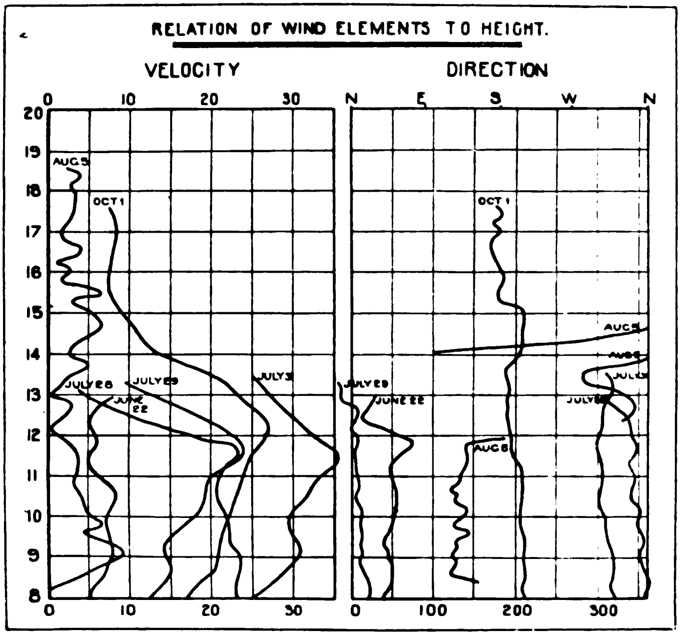
[*] The following references may be given for the statements enumerated here :

1. Barometric Gradient and Wind Force. Report by Ernest Gold, M.O. publication, No. 190.
2. Shaw, Journal of the Scottish Met. Soc., vol. xvi. p. 167, 1913.
3. Shaw, Q. J. Roy. Met. Soc., vol. xl. p. 111, 1914.
4. The Free Atmosphere of the British Isles. Report by W. H. Dines, F.R.S., M.O. publication, No. 202. C. J. P. Cave, The Structure of the Atmosphere in Clear Weather, Cambridge Univ. Press. E. Gold, The International Kite and Balloon Ascents, Geophysical Memoirs, M.O. publication, No. 210e.
5. Shaw, Principia Atmospherica, Proc. R.S.E., vol. xxxiv. p. 77, 1914.

The computations of equations B and C are not yet published.

will be given by the roots of a quadratic equation, which will be impossible if v is greater than $\frac{E\,\omega \sin \lambda}{\cot r}$. This for a circle of 70 miles diameter only allows a velocity of about 4 metres per second.

This is confirmed in practice, and furnishes a crucial test of the two theories. If an anticyclone is a place where air descends and flows outward, its velocity should diminish as the air spreads outwards, but the reverse is the case with an anticyclone.



Figs. 5 and 6.—Diagrams representing the velocity and direction of the wind from 8 kilometres upwards on six occasions in 1908. The scale on the lefthand side gives the height in kilometres, that at top and bottom of the velocity-diagram the velocity in metres per second. Directions are indicated by the points of the compass at the top, and the orientation in degrees from North, at the bottom.

2. *The small influence of the troposphere and therefore the dominance of the stratosphere in the distribution of surface pressure.*

This follows directly when numerical values are inserted in equation A. The right hand side of the equation consists of two terms which are of opposite sign, and numerically approximately equal in the middle regions of the troposphere. Their combined effect for the whole range is therefore relatively small, and the change of pressure produced in the troposphere is unimportant. The distribution of the stratosphere is dominant throughout the troposphere.

3. *The apparently capricious variations of wind and temperature with height disclosed in pilot-balloon ascents and ballons sondes.*

The results of the observations of *ballons sondes* show local variations of temperature, and those of the observations of pilot balloons show similar variations of the direction and velocity of wind. These variations can be connected numerically by Equation A in combination with Equation 1. A number of examples are given in a paper read before the Royal Meteorological Society. To quote one, the rapid transition from a southerly wind at 1100 metres through a calm to a northerly wind at 1500 metres on October 16, 1913, was shown to indicate a temperature-gradient of 7° per hundred kilometres towards the east, a condition that was in satisfactory accord with the meteorological circumstances of the time.

The same combination of equations enables us to specify the *conditions under which what is known as "Egnell's law"*—that wind velocity at different heights is inversely proportional to the density at those heights—*may be expected to be verified*, and the conditions prescribed are essentially reasonable.

4. *The rapid falling off of wind in the stratosphere noted in observations with pilot balloons.*

This is illustrated by a diagram compiled from the figures of high soundings reproduced in Captain Cave's "Structure of the Atmosphere in Clear Weather" (Fig. 5). The result follows directly from the application of Equation B to the special conditions of the stratosphere. The computations for the four occasions in which there was a wind of considerable magnitude at the base of the stratosphere give the following results :—

Date.		Rate of change of Velocity in the Stratosphere. m. s per kilometre.	Horizontal Temperature Gradient.	
			Computed. Degrees per Hundred Kilometres.	Observed. Degrees per Hundred Kilometres.
1908	Oct. 1 . .	– 7	2·1	—
	July 31 .	– 5	1·5	—
	„ 29 .	–11	3·3	3·3
	„ 28 .	–13	4·0	—
	„ 27 .	—	—	2·5

The calculation has been arranged to give the computed horizontal temperature gradient, because the values of that quantity are indicated in the models of temperature distribution constructed in the Meteorological Office for July 27 and 29 which are represented in Figs. 7 and 8 respectively. The order of magnitude which is indicated is quite reasonable, and for the one occasion on which the two can be compared the agreement turns out to be exact. That may be fortuitous, but we may take advantage of the circumstance to use the combination of the figures for the wind in the stratosphere, and the horizontal temperature gradient at 13 kilometres to compute the latitude of the place of observation with an accuracy that may lead us to reconsider the common remark that Meteorology is not an exact science.

The same equation applied to the troposphere, *assuming normal values for temperature, gives correctly the rate of change of velocity with height* as shown in the corresponding diagram.

5. *The permanence of vertical motion about a vertical axis in the atmosphere, which is indicated by the long travel of cyclonic depressions.*

From Equation C applied to the stratosphere, it follows that a circulation in the base of the stratosphere with a given horizontal temperature gradient, such as is found there, will have only a limited extension upwards. With a wind velocity of 20 metres per second, and a horizontal temperature gradient of 5 per 100 kilometres, the extension will be 1·4 kilometres upwards, so that the vortex will be covered by a cap in which the velocity gradually falls off to zero within a very limited height.

For the extension downward the calculation is more complicated, but the computed change of velocity is very small, so that the vortex must be regarded as reaching the ground, and it would appear that a vortex extending throughout the troposphere terminating with a cap in the stratosphere is a possible reality.

Thus the hypothesis of an atmosphere in which the wind velocity is everywhere adjusted to balance the pressure distribution, enables us to explain many of the ascertained facts that have been disclosed by the investigation of the upper air, and strongly supports the hypothesis that the pressure distribution at the surface is controlled by the stratosphere, and only modified locally by the local convection. Against the control of the distribution of pressure by the upper atmosphere may be urged the formation of anticyclones over the relatively cold areas of sea and land, especially the *winter* anticyclones of the great Continents of the Northern hemisphere. For the local result of surface-cold we have to bring into account the eddy-motion of the surface layer. Some examples are given in the Meteorological Report of the voyage of the *Scotia* in 1913 by G. I. Taylor, published by the Board of Trade in 1914. The formation of horizontal lines or rolls of low cloud in a steady air-current is probably another example of eddy-motion due to the friction of the surface frequently in evidence.

Temperatures and Pressures in a Block of Atmosphere 24 Kilomet

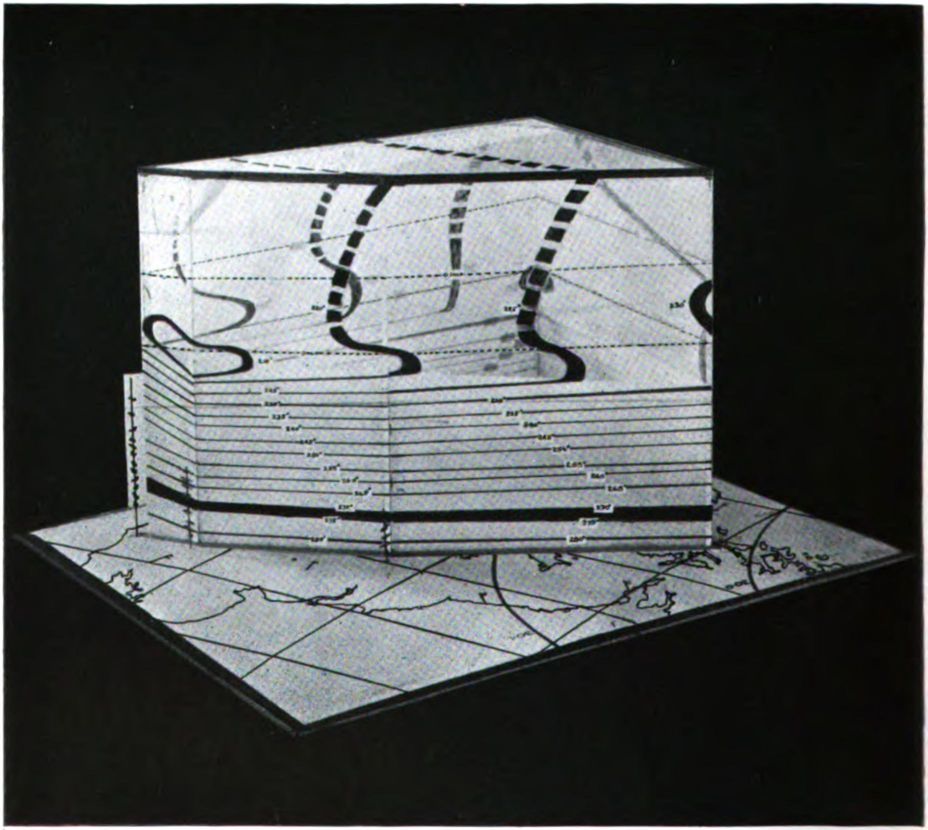


Fig. 7.—From observations taken on July 27th, 1908, at Ditcham Park, Watlington, Manchester, Crinan and Limerick. Block seen from the north-east. Isotherms are shown for each 5° Absolute from 280° A. to 215° A. The space between the isotherms of 270° and 275° is filled in; for other isotherms a thickness corresponding with $\frac{1}{2}$° C. is covered. The beaded lines in the stratosphere are isobars for 0·2 megadyne and 0·1 megadyne per square centimetre respectively. The arrows on the standards face the wind as determined by observations with theodolites.

(15 MILES) THICK OVER A TRIANGULAR PORTION OF THE BRITISH ISLES.

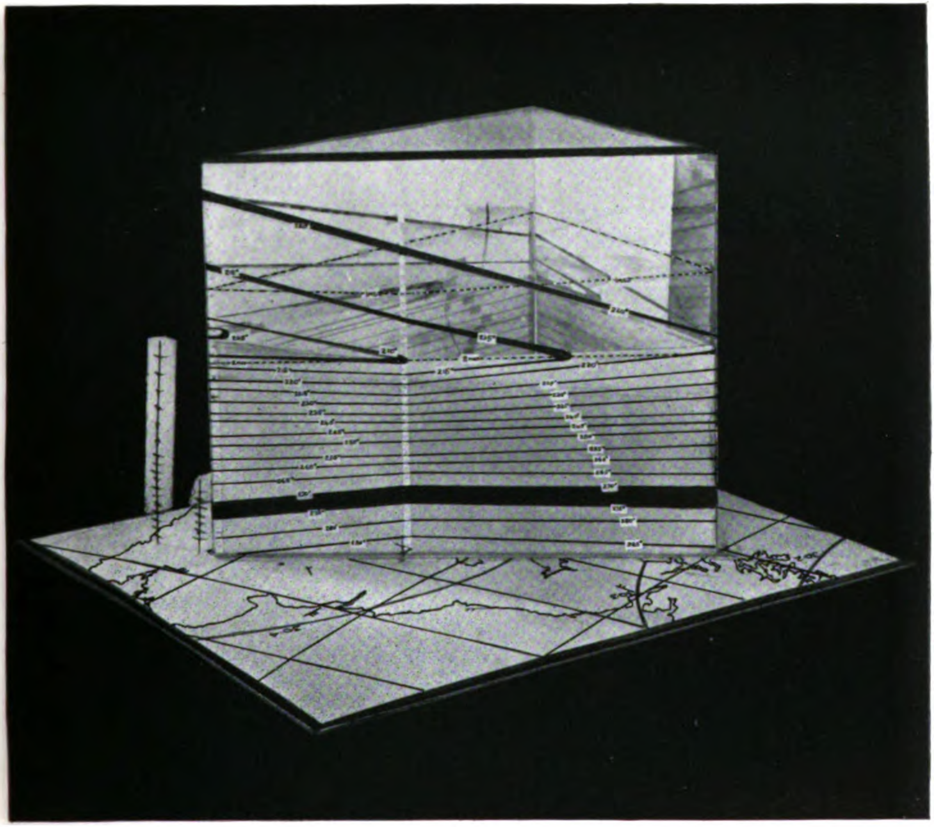


FIG. 8.—From observations taken on July 29th, 1908, at Watlington, Manchester, Crinan and Limerick. Block seen from the north-east. Isotherms are shown for each 5° Absolute from 285° A. to 205° A. The space between the isotherms of 270° and 273° is filled in; for other isotherms a thickness corresponding with ½° C. is covered. The beaded lines in the stratosphere are isobars for 200 mb. and 100 mb. per square centimetre respectively. The arrows on the standards face the wind as determined by observations with theodolites.

The reader should notice the isobar of 30·4 in. (1029 mb.) on the map in this figure which crosses the coast between Spurn Head and Flamborough Head, and compare its position with that of the isobar of 30·1 in. (1019 mb.) in Fig. 7 which is a little further to the north. In the interval of two days a layer of cold air has spread along the base of the stratosphere from the eastward, and the pressure at the surface has become greater by 10 mb.

It may, of course, be only another illusion. The beauty of an illusion is that we are only conscious of its existence when it has ceased to exist. I suppose that there is no subject so fruitful of illusions as the study of weather. The path of the history of science is paved with their petrified remains. Ever since the world began successive students of weather have always lived each in his own paradise of working hypothesis with which he could gratify his passion for tracing cause and effect, and which is just as good as real until it is petrified by some stony fact.

In the opening sentence of this lecture I referred to the origin of the Meteorological Office in the language of Greek myth. Permit me as a relief from the strain of strenuous meteorological thinking to pursue the idea further, and to go back to the Gorgon as a mythical elder sister of the Meteorological Office. The Gorgon is in truth symbolical of the effect of the hard facts we are accustomed to call trustworthy meteorological observations? The suggestion may strike you at first sight almost as absurd as that wind should really balance isobars, but are we not told that the home of the Gorgon was assigned by some writers in the West, the region of the Hesperides where our weather comes from, but by others in Libya the South, the controlling region of the permanent anticyclone? There were snakes in her hair and she was girdled with snakes, and we know that modern meteorology is made up of cyclones, whose insidious name comes from *kuklos*, the coil of a snake. What if the snakes of the Gorgon's hair and waist are really cyclones? She was the offspring of Neptune, born in the Temple of Minerva. That is exactly what Greek myth would say if it wanted to indicate such an event as the creation of meteorological observations by an arrangement between the Admiralty and the Royal Society, through the intermediary of the Board of Trade. For that crime her severed head is for ever in front of Minerva's shield, and to this day if you tackle some of the votaries of the goddess about her attitude towards the study of weather, they will flash upon you the petrifying spectacle of the accumulation of meteorological observations—too many snakes.

Let me draw your attention to a remarkable piece of evidence that the Gorgon is really a representation of meteorological observations. In "Nature" of June 4th, 1914 is a bird's-eye view of the result of meteorological observations over the whole world, and I place side by side with it one of the earliest representations of the Gorgon's head.* The similarity between the cyclonic depressions of the "Nature" artist and the coils of the Gorgon's hair is remarkable; even to the parting between the Northern and Southern hemispheres.

One point more. In spite of the drastic treatment which she received, the Gorgon always puts her tongue out. It is not at all polite. What can it mean? There is a story in Galton's reminiscences, of Herbert Spencer at the Athenæum describing to Huxley a nightmare that he had of a beautiful theory killed, as Huxley interjected, by an obstinate fact. My experience is that those people whose business in life, like that of meteorological observations, is to kill theories by exhibiting facts, are just a little inclined to put out the tongue, and if they had lived in the times of the Gorgon I can well imagine their behaving like the picture.

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* The slide was taken from the picture of the grotesque Medusa found in the foundations of the Parthenon in 1836 and now at Athens.

I find in the Gorgon's tongue something symbolical of the attitude of the practical man towards the efforts of meteorologists to read the riddles of the weather. The criticisms that reach us at the Meteorological Office are very suggestive of putting out the tongue. And whatever we may think about its manners or its intelligence it is very paralysing for diffident natures like ours.

I venture to go further and to say that this feature of the Gorgon's head is remarkably true to life as representing the traditional aspect of the British public towards science. I fear you may be disappointed that I have said nothing about the war, but so long as the Gorgon's tongue is visible at the British Museum (as shown in the slide taken from the copy of Pheidias' Shield of Athene preserved there) all that need be said is that putting one's tongue out, whether at science or anything else, is a very natural expression of the emotions, but it puts one in a position in which it is not possible to use to the best advantage either one's physical or one's mental faculties. Unless you take your tongue in all that you can do is to scream.

[N. S.]

WEEKLY EVENING MEETING,

Friday, March 17, 1916.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. LL.D. Sc.D. F.R.S.,
in the Chair.

AUBREY STRAHAN, M.A. LL.D. Sc.D. F.R.S., Director, Geological Survey of Great Britain.

The Search for New Coal-Fields in England.

WHETHER the alarms which have arisen from time to time as to the exhaustion of British coal-fields were justified or not, there can be no question about the importance of making the most of what we possess. Two Royal Commissions have considered the matter in all its bearings. Evidence was taken on a variety of subjects, and among these not the least important were estimates of the reserves believed, but not yet proved, to exist, and the utilization to the greatest possible advantage of all available coal. Both subjects are deserving of close attention, but the search for concealed coal-fields is that on which Geology has the most direct bearing, and is, therefore, the subject on which I propose to speak this afternoon. Much progress in the search has been made since the Royal Commission on Coal Supplies reported in 1905, and the problems which have arisen for solution are not devoid of interest.

When coal first came into use it was easy to obtain. The seams were visible at the outcrop, and were dug out by innumerable slants, levels, and shallow pits, rather to the annoyance of the miners of to-day, whose difficulties are increased by the admission of water to the seams through the old crop-workings. As the seams descended to greater depths the sinking of deeper shafts, well-equipped with machinery, became necessary, and eventually the modern colliery was developed, situated, generally, far away from the outcrop of the coal, extending to depths not far short of 4,000 feet, and worked by machinery which is not surpassed in efficiency by that of any other industry.

The collieries, however, were for a long time confined to what are known as "visible coal-fields." Less than a century ago it was commonly said by miners, "Under red rock no coal"; and it was not till Geology had been scientifically studied, and the geological mapping of the country had made much progress, that the existence and extent of "concealed coal-fields" under newer formations, whether red or any other colour, came to be realized.

The term "visible coal-field" is applied to those tracts where the coal measures crop out at the surface and are open to view except where they are covered by superficial materials such as gravels, boulder-clay, etc. These are the tracts which are shown as coal-fields on ordinary geological maps. But the coal measures occupy a definite position rather low down in the sequence of geological formations, and may be overlain by a variety of newer groups of strata, ranging from Permian upwards. A tract of coal measures overlain by any newer formation is known as a "concealed coal-field"; it is not indicated on ordinary geological maps, but is left to the intelligence of the observer to discover.

Obviously he must note, in the first place, what is the character of the limits of a visible coal-field as shown on the map. If the coal measures terminate because older strata rise from beneath them and usurp the surface, the limit, as shown, definitely includes all the area in which coal should be sought. If, on the other hand, the area shown as coal measures adjoins an area occupied by newer formations, then the mapped boundary indicates the limit of those newer strata, but not necessarily the limit of the coal measures. The map forming Fig. 1 has been constructed to illustrate the position of visible coal-fields in relation to older and newer formations. Upon it all areas occupied by formations older than coal measures are left white; visible coal-fields are shown in black; and areas occupied by newer formations are shown in grey. In the area left white search for coal is useless, though it has not on that account been abandoned. It was said years ago, and is probably still true, that the money expended in boring for coal in places where no coal could exist would suffice to pay the annual cost of the Geological Survey.

From the areas coloured black nearly all the coal of past years has been raised, and these are the sources the exhaustion of which is justifiably regarded as being within sight.

The areas coloured grey are those in which there is no *à priori* reason why coal should not occur. Somewhere within these areas lie the concealed coal-fields.

For example, in North Wales a visible coal-field extends through Flintshire and Denbighshire. Westwards older formations rise to the surface, and the coal-field is definitely terminated; eastwards the coal measures pass beneath New Red Sandstone, and form a concealed coal-field, of which the existence is certain, but of which the extent and accessibility have still to be proved. The South Wales coal-field, on the other hand, is almost wholly surrounded by older formations, and though some of it may lie at an inaccessible depth, only an insignificant part of it is "concealed" in the sense expressed above. In other parts of the country we find that the visible coal-fields of Cumberland, Durham and Northumberland, Yorkshire with Nottinghamshire and Derbyshire, Staffordshire, Shropshire, Warwickshire, Leicestershire, and Somerset with Gloucestershire, are all bordered on one side or the other by newer formations, and may, therefore, each be associated with a concealed field. The results which have been obtained up to the present in proving these extensions and in locating coal-fields not associated with any visible coal-field are shown by the black-and-white ruling on the grey ground.

The proved extensions are indicated by a black ruling. One of the largest is that which underlies parts of Yorkshire and Nottinghamshire; but in the Cheshire Basin, also, a concealed coal-field of great size could be shown in association with the visible coal-fields of Lancashire, Cheshire, Shropshire, and Denbighshire, were it not for the natural limit to the practicable depths of mines. On this subject much evidence was taken by the Royal Commission on Coal Supplies, with the result that the precedent of the Coal Commission of 1871 was followed, and 4,000 feet was adopted as the limit of practicable depth in working. Of scarcely less importance are the proved and

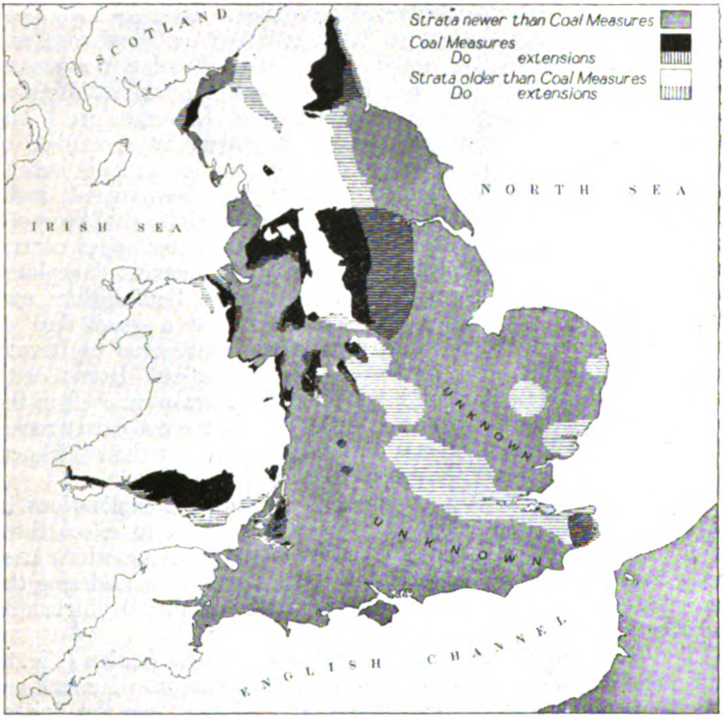
Strata newer than Coal Measures
Coal Measures
Do extensions
Strata older than Coal Measures
Do extensions
SCOTLAND
NORTH SEA
IRISH SEA
UNKNOWN
UNKNOWN
ENGLISH CHANNEL

FIG. 1.

possible extensions of the Midland coal-fields, while in Kent explorations carried on during the last thirty years have revealed the existence of a coal-field of not less than 170 square miles in extent, exclusive of the part which lies below the sea.

On the same map the regions where formations older than coal measures have been found to lie below the Secondary rocks are indicated by a lighter tint of grey. These regions, therefore, belong to the same category as the areas left white, and need no further consideration as possible sites of concealed coal-fields. It is to be noticed that the white ruling borders the Kent coal-field on its western and northern sides, and that it extends thence under London in a general north-westerly direction through Buckinghamshire, Oxfordshire, and Northamptonshire, towards Warwickshire and Leicestershire. The existence of this barren tract has been proved by a number of borings in and near London and in the counties named, but its limits have not been ascertained. On its north-eastern side rocks older than coal measures have been proved at Culford, Lowestoft, and Harwich, rendering the existence of coal measures under central and eastern Suffolk improbable, but there still remains unexplored a tract extending north-westward through Essex, Bedfordshire, and Rutland. On its south-western side there lies a great area of unexplored ground. The south coast from Folkestone to Devonshire, and adjacent areas in Sussex, Hampshire, and Dorset, with parts of Devonshire, Somerset, and Wiltshire, are unproved in the sense that no boring has yet reached the base of the Secondary rocks. What these rocks rest upon it is impossible to say, but their thickness is likely to be great near the south coast.

Time will not permit me to describe in detail the explorations in all the concealed coal-fields. I propose, therefore, to select three examples in order to illustrate the nature of the problems which arise in the investigation. The purpose will be served by considering the concealed coal-fields of Nottinghamshire with Yorkshire, Denbighshire, and Kent.

The Nottinghamshire coal-field is illustrated by a section (Fig. 2) drawn from near Crich, in Derbyshire, to Kelham, near Newark-on-Trent. Commencing in the Carboniferous Limestone, the line of section crosses the visible coal-field in a distance of about 6½ miles. Thus far it is founded on observations made at the surface on the outcrop of the strata, the order in which they succeed one another, and the angle of their dip. But it then enters a region in which Permian (magnesian) limestone, Bunter sandstone, Keuper sandstone, and Keuper marl in succession form the surface of the ground. These formations lie unconformably upon the coal measures; they are inclined at a gentler angle, and have not been affected by the folds which have bent the coal measures into synclines and anticlines. It follows that the newer strata are not parallel to the older, and may rest upon any part of the coal measures, or even upon any older

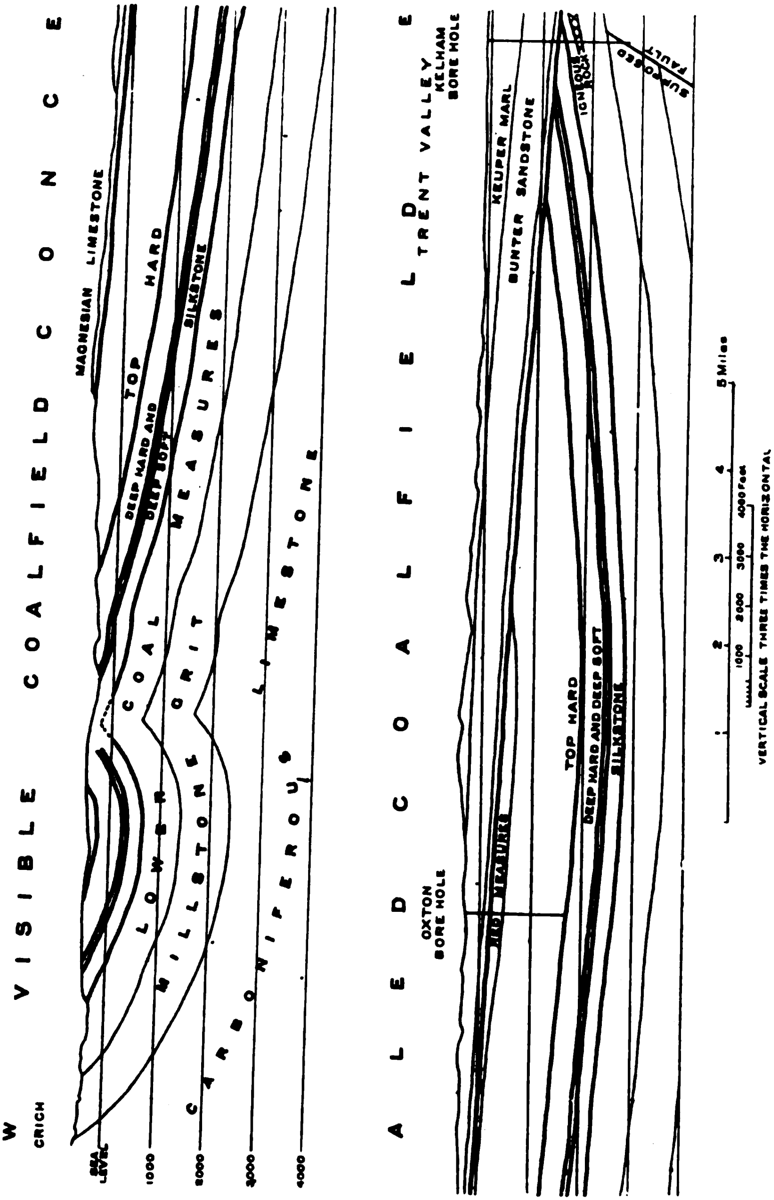
W
CRICH
E
VISIBLE COALFIELD CONCE
ALED COALFIELD
MAGNESIAN LIMESTONE
TOP HARD
DEEP HARD AND DEEP SOFT
SILKSTONE
COAL MEASURES
LOWER COAL MEASURES
MILLSTONE GRIT
CARBONIFEROUS LIMESTONE
SEA LEVEL
1000
2000
3000
4000
TRENT VALLEY
KELHAM BORE HOLE
KEUPER MARL
BUNTER SANDSTONE
IGNEOUS ROCK
SUPPOSED FAULT
OXTON BORE HOLE
RED MEASURES
TOP HARD
DEEP HARD AND DEEP SOFT
SILKSTONE
1
2
3
4
5 Miles
1000
2000
3000
4000 Feet
VERTICAL SCALE THREE TIMES THE HORIZONTAL

FIG. 2.

formation. Surface observations made upon the newer formations give no clue to the structure of the coal measures; reliance has to be placed on boreholes, and on the identification of the specimens obtained from them. The section therefore has been drawn through a borehole at Oxton, and near the Annesley Colliery now working, to a borehole at Kelham.

The measures of the visible coal-field contain many workable seams of coal, but for the sake of simplicity I have represented three only. Of these the Top Hard is the most important, and the object of search in the boreholes. The Oxton borehole was put down 7½ miles within the margin of the concealed coal-field, and furnished valuable information. It showed that the base of the newer formations descended eastwards 790 feet in that distance—that is, at a rate of 1 in 50—and thus rendered possible a forecast of the thickness of these formations that was likely to be met still farther east. It proved also that the Top Hard had descended 1,170 feet in the same distance, and was therefore more steeply inclined than the newer formations. This gives room for the appearance of higher coal measures eastwards than now exists towards the west; indeed, in the absence of further information, it appeared possible that the concealed coal-field might extend an indefinite distance, though it might descend to an inaccessible depth. As a fact, its eastward limit was the subject of prolonged consideration by both the Royal Commission of 1871 and that of 1905, but neither Commission had sufficient evidence to decide the question. Much light has been thrown upon it by the borehole at Kelham.

The Kelham borehole, situated nearly ten miles east of the Oxton borehole, proved that the eastward dip of the newer formations was maintained at the same gentle angle. At a depth of a little more than 1,500 feet it traversed a seam of coal, the identity of which is in doubt. At about 1,700 feet it passed through a dyke of igneous rock which is of no significance. More important is the fact that down to about 2,400 feet it was in strata which, by their character and fossils, could be identified as lower coal measures, that below them it met little more than 200 feet of millstone grit, and that it then entered carboniferous limestone.

More than one interpretation of this section is possible. I have chosen the simplest, and have assumed that the greater part of the millstone grit is cut out by a fault. But whatever explanation is adopted, and whatever the coal-seam may be—whether the Top Hard, as supposed by some, or the Silkstone, as appears more likely—an eastward rise of the coal measures has been proved to exist. The deepest part of the concealed coal-field has been passed, and the eastern limit is in sight. As a fact, the bottom of the trough is probably not far from Oxton, near which place the strata have been found to become nearly horizontal in the workings of the Annesley Colliery.

Similar explorations are being carried on across other parts of the concealed coal-field,* and the result has been to show that the eastern limit lies not far east of the valley of the Trent. Though not so large as appeared possible to the Commission of 1905, this extension is a notable addition to the visible coal-field, but its productiveness is still a matter of doubt. Observations on the thickness of coal-seams are difficult in boreholes, but so far the results have been disquieting.

The Denbighshire concealed coal-field offers a different problem. Here it is not so much the existence as the accessibility of the coal-seams which is in question, and the reason is found in a great development of upper coal measures, mostly of a red colour and barren of good coal, above the productive measures. The section (Fig. 3) has been drawn from the escarpment of carboniferous limestone near Llangollen across the coal-field into Shropshire. It will be noticed that the measures dip at a much steeper angle than in Nottinghamshire, and that they are repeatedly thrown up and down by faults. Both the angle of dip and the faults have been proved in colliery workings as far east as the part marked "proved" extends. Beyond that point the section is hypothetical.

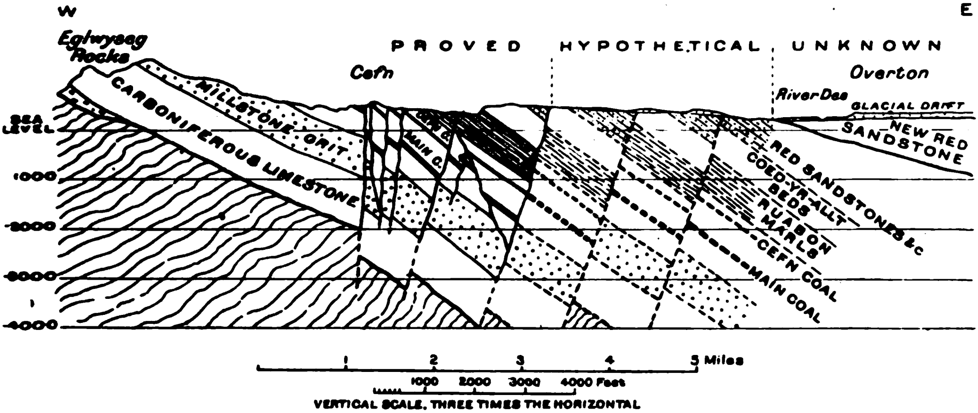
W
E
Eglwyseg Rocks
PROVED
HYPOTHETICAL
UNKNOWN
Cefn
Overton
River Dee
GLACIAL DRIFT
NEW RED SANDSTONE
MILLSTONE GRIT
CARBONIFEROUS LIMESTONE
MAIN C.
RED SANDSTONES &c
COED-YR-ALLT BEDS
RUABON MARLS
CEFN COAL
MAIN COAL
SEA LEVEL
1000
2000
3000
4000
1 2 3 4 5 Miles
1000 2000 3000 4000 Feet
VERTICAL SCALE, THREE TIMES THE HORIZONTAL

FIG. 3.

In the proved part a not uncommon relationship between the dip and the faults is observable. The steep dip, if not counteracted, would speedily carry the coal-seams to an inaccessible depth; but the tendency of the rather complicated faults is to bring them back to their level. As a consequence the main seam keeps at a depth of less than 2,000 feet for two miles or more.

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* An account of the investigation as a whole appears in "The Concealed Coal-field of Yorkshire and Nottinghamshire" (*Mem. Geol. Survey*), 1913.

It is of the greatest importance to note that in this coal-field the strata which first cover up the productive series consist of upper coal measures (for the most part devoid of coal), and that a considerable distance has to be traversed before the margin of the New Red Sandstone is reached. The importance lies in the fact that the upper measures are conformable and parallel to the productive strata. They follow on in an ascertained sequence, and if the horizon of any rock in the sequence is determinable at the outcrop, it becomes possible to estimate the depth to the productive strata at that spot. The New Red Sandstone and the Permian, when present, on the other hand, rest unconformably upon the carboniferous strata, and it is often impossible to predict what will be found below them.

The upper measures are distinguishable into three groups of strata, differing in general characters, but not so precisely defined as might be desired. The lower group consists mainly of red marls, the middle group mainly of yellow sandstones, and the upper group of red sandstones and marls not easily distinguished from the New Red Sandstone. Their characters render it possible for a skilled geologist to recognize the outcrops of these groups, and, provided that he knows their thickness, to deduce the depth to the coal below them. Such work as this has engaged the attention of the Geological Survey, both in North Wales and other parts of the country, for the past few years, and is throwing much light upon the accessibility of some concealed coal-fields which remain to be exploited.

One of the results of the work in Denbighshire has been to show that the three groups are repeated by faults—that is to say, that the counteraction of dip by faults which has been proved to exist in the proved extension of the coal-field is likely to exist also in the extension not yet proved. The inference follows that the seams are kept by the faults within an accessible depth for some distance farther than the underground explorations have extended. The details of the faulting, however, are not known, and I have therefore marked this part of the concealed coal-field as "hypothetical."

Finally, the New Red Sandstone comes on and effectually conceals all older formations. Under it the existence of a concealed coal-field is possible, but we have no evidence as yet to show whether or no its thickness is prohibitive. Towards the centre of the Cheshire basin it is certainly great: a boring near Northwich ended in New Red Sandstone at a depth of more than 2,500 feet.

As a third example we may consider the concealed coal-field of Kent. This differs from the other examples in that it is not connected with any visible coal-field, but is everywhere concealed by a blanket of Secondary and Tertiary strata, which generally exceeds 1,000 feet in thickness. The existence of coal measures under Kent was inferred on geological grounds many years ago, but was first proved in 1886 by a boring near Dover. Coal was first raised in

1911, and the annual output up to date is given by the Home Office as follows :—

Year.	Tons.
1911	150
1912	1,099
1913	59,203
1914	130,440
1915	158,389
	349,281

During the last few years many borings have been put down and the limits of the coal-field approximately determined. The information at our disposal is sufficient to enable us to realize the general structure of the strata. The Secondary and Tertiary strata which form the blanket are approximately horizontal, but the coal measures and other palæozoic rocks below them are folded and disturbed. The surface of the older rocks was planed off to a surface, known as the palæozoic floor, which is remarkably even, but slopes gently downwards to the south or south-west. Upon this floor the newer strata repose, and thicken as it descends, until they reach a thickness which, up to the present, has proved a bar to farther exploration. The surface configuration gives no clue to the structure of the palæozoic rocks, and statements that coal has been found at small depths in this part of England are based on erroneous identifications as coal of the lignite, which is common in some of the Secondary strata.

For a determination of the structure of the older rocks we are largely dependent upon the fact that many of the bore-holes have reached the top of the carboniferous limestone. They have passed directly from productive coal measures into the limestone, thus proving the absence of millstone grit. There is, therefore, a gap in the stratigraphical sequence, and possibly some slight discordance of dip between the coal measures and the limestone, but the form of the limestone surface is, notwithstanding, a matter of much significance. I have, therefore, prepared a map (Fig. 4) on which the borings are indicated, and on which the depth below sea-level of every point at which the limestone has been touched is shown.

When these entries had been made upon the map, it became apparent that there was sufficient evidence for drawing contour-lines in the limestone surface with a fair approximation to the truth. The lines are drawn at intervals of 500 feet from a level of 1,000 feet below the sea down to a level of 3,500 feet. They enable us to visualize the surface as a trough, the axis of which ranges a little west of north. We can see, further, that it is only in this trough that there is any room for coal measures. Towards the north it rises until rocks older than the carboniferous limestone come up into contact with the Secondary strata; southwards it descends continuously as far, at least, as the south coast. Eastwards the limestone surface ascends at a gradient of about 1 in 16 until it meets the Secondary base near Ramsgate; westwards it ascends at a slightly steeper angle (about 1 in 7½), and meets that base near Elham and Canterbury. These structures are illustrated by the section drawn across the trough at right angles to its axis. It will be noticed that no bore-hole has reached the base of the coal measures in the southern and central part of the trough. The coal measures there have been proved to exceed 2700 feet in thickness, and the limits of the extension under the sea are unknown.

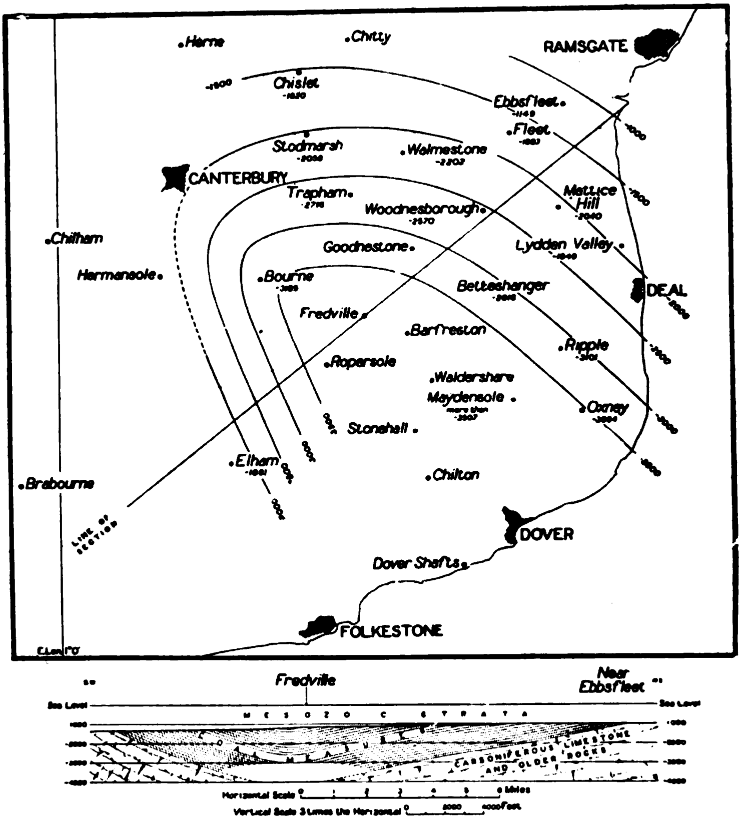

Herne
Chitty
RAMSGATE
Chislet
Ebbsfleet
-1149
Fleet
Stodmarsh
Walmestone
-2202
CANTERBURY
Trapham
-2716
Woodnesborough
-2570
Mattice Hill
-2040
Chilham
Goodnestone
Lydden Valley
Hermansole
Bourne
Betteshanger
DEAL
Fredville
Barfreston
Ripple
-3101
Ropersole
Waldershare
Maydensole
more than -3307
Oxney
Stonehall
Elham
-1661
Chilton
Brabourne
LINE OF SECTION
DOVER
Dover Shafts
FOLKESTONE
E.Lon 1°0'
SW
Fredville
Near Ebbsfleet
NE
Sea Level
-1000
-2000
-3000
-4000
MESOZOIC STRATA
CARBONIFEROUS LIMESTONE AND OLDER ROCKS
Horizontal Scale 0 1 2 3 4 5 6 Miles
Vertical Scale 3 times the Horizontal 0 2000 4000 Feet

FIG. 4.

The geological relations of the Kent coal-field to that of the Pas de Calais on the one side and to those of Bristol and South

Wales on the other side, have long been in doubt. Geographically, these coal-fields appeared to fall nearly on the line of a belt of disturbance which traverses Belgium and the north of France, where

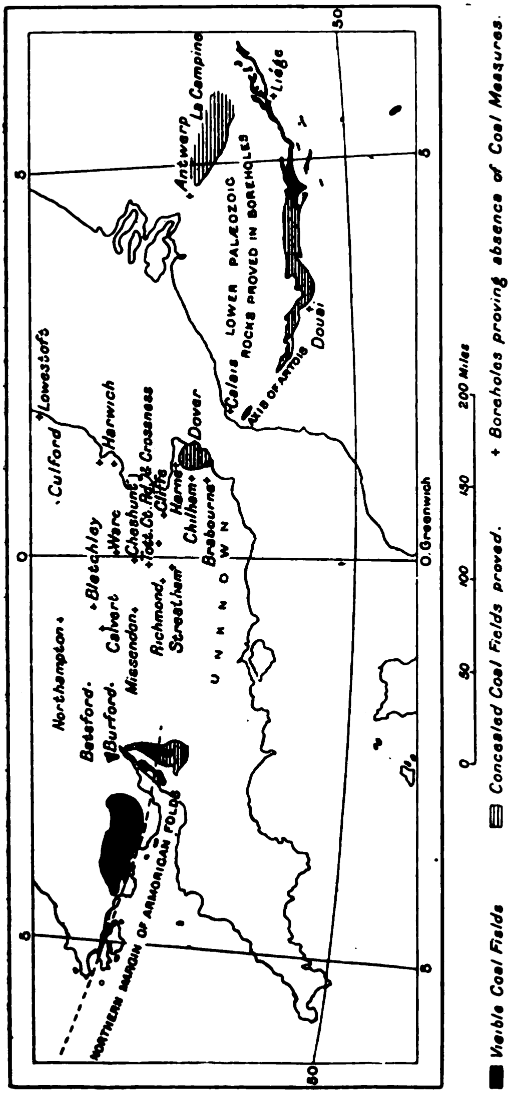

Lowestoft
Culford
Harwich
Crossness
Dover
Cliffe
Herne
Chilham
Brabourne
Cheshunt
Tott. Ct. Rd.
Ware
Bletchley
Calvert
Missenden
Richmond
Streatham
Northampton
Bedford
Burford
UNKNOWN
NORTHERN MARGIN OF ARMORICAN FOLDS
Antwerp
La Campine
Liége
Calais
Douai
AXIS OF ARTOIS
LOWER PALÆOZOIC ROCKS PROVED IN BOREHOLES
0 Greenwich
50
5
0
5
50
0 50 100 150 200 Miles
Visible Coal Fields
Concealed Coal Fields proved.
+ Boreholes proving absence of Coal Measures.

FIG. 5.

it is known as the Axis of Artois. Its direction, so far as it is observable, would carry it under the south of England; and that it does take this course is probable, but there are reasons for thinking that the Kent coal-field may lie on the north side of the belt and not in it.

The map forming Fig. 5 has been prepared to illustrate this point. It extends from Liége to West Pembrokeshire, and on it are distinguished visible and proved concealed coalfields. The course of the belt is marked by a long line of narrow coalfields, both visible and concealed. It coincides for some distance with the valley of the Meuse. Then, turning north-westwards, it passes by Béthune to the neighbourhood of Calais, and, if prolonged, would pass under the English coast near Folkestone. The disturbances of the palæozoic rocks along the belt are for the most part concealed by a blanket of Secondary and Tertiary strata, but by taking advantage of occasional exposures at the surface through the blanket, but still more by mining operations of great daring and skill, it has been shown that they are of a remarkable character. The strata have not only been thrown into the form of a trough, in which the coal measures have been preserved, but the trough has undergone deformation on a vast scale. So intense have been the earth movements that the southern margin of the trough has been doubled over, older formations have been pushed bodily over the coal measures, and the coal measures themselves have been thrown into complicated folds. There are places where a single vertical shaft may pass through Silurian and Devonian rocks, and under them into carboniferous limestone, and then into coal measures, all upside down.

The Kent coal-field shows no such structures. So far as can be judged by boreholes, the dip of the coal measures is generally gentle. Faults are certain to exist, though nothing is yet known of them, but there is no reason to suspect the existence of structures comparable to the gigantic dislocations of the Continental trough. It is difficult to suppose that those huge disturbances tamely die away in a gentle fold within the limits of Kent.

Some light is thrown on this question by the discovery of the Belgian coal-field of La Campine. About the year 1901 this coalfield was proved by boring through Tertiary and Secondary strata, and upwards of a dozen shafts were being sunk to it when the War broke out. So far as it has yet been explored, the coal-field branches off in a north-westerly direction from the disturbed belt in which the previously known coal-fields are entangled (Fig. 5). It lies on the northern side of the belt, and does not share in the intense folding and overthrusting which characterize the belt. It seems not improbable, therefore, that the Kent and the Campine coal-fields may be comparable both in their general structure and in their relations to the belt of disturbance. If this be so, the continuation of the belt may be expected to lie south of the Kent coalfield, and to pass under a part of England where the great thickness of Secondary strata has hitherto proved a bar to exploration. The question whether it links itself up with the system of Armorican folds which traverse the south of Ireland, Pembrokeshire, South Glamorganshire and Somerset, is not yet solved, nor is it possible to say whether any coal-fields are entangled in it, corresponding to those of the Continental trough. This can be settled by boring, and in no other way.

The examples of concealed coal-fields which I have brought before you illustrate the nature of the geological work which is required. The mapping of the newer formations presents no exceptional difficulties, and is being carried out by the Geological Survey with precision on the large scale maps now available. At the same time attention is especially concentrated on the less easy task of identifying the subdivisions of the upper coal measures, and of determining the effects of faults in them; or, what is equally important, of ascertaining where the sequence of strata below them is likely to be complete. For in some parts of the country these barren upper measures overlap all the lower and productive measures, and rest directly upon older formations. This is probably the case at Batsford, and may be the case at Burford (Fig. 5).

Progress in the search depends much also upon the correct interpretation of the results obtained in boreholes. The recognition of the principal formations traversed by the holes is important, but scarcely less so is the identification of certain bands in the productive coal measures which are characterized by marine fossils, or of the subdivisions of the upper measures by their lithological characters. These and other determinations can be made only by a skilled geologist, and as a fact the information that should have been gained in many deep boreholes has been lost to the nation for want of geological assistance.

The Royal Commission on Coal Supplies realized this defect in administration, and in their final Report (1905) inserted the following clause:—"A large number of borings have been made in various parts of the country ranging to upwards of 3,000 ft. in depth. At present no machinery exists for preserving any information thus obtained, and we think it would be of great advantage if particulars of borings could be collected and preserved in a Government office."

In 1913 I referred to the subject in a Presidential Address to the Geological Society in the following terms:—

"This recommendation contemplates the inclusion of scientific results among the other particulars to be preserved, and on this account it might not be acceptable to explorers who for financial reasons were desirous of keeping to themselves the information which they had been put to expense to gain. There would probably be no difficulty in meeting the wishes of the explorers on this point; but, at any rate, the objection would not apply to the registration of other particulars. At present it is open to any member of the community, after agreement with the owner of the land, to bore a hole wherever he likes, as large as he pleases, as deep as he can go, and to leave no record. There can be no object in keeping the site, the diameter, or the depth of the hole secret; in fact it is not possible to do so. These particulars at least should be compulsorily registered, and the site of an important boring permanently marked. . . .

"It may be thought that a sufficient record is kept by the Geological Survey. As a fact, it is only through courtesy or by chance that we learn that borings are in progress. There are many persons who feel strongly the desirability of records being preserved, and make a point of informing us when boring operations are undertaken; but it happens too frequently that we hear of a borehole having been made, too late for an adequate examination of the cores. Others we never hear of, nor are the cores submitted to any competent geologist; and in such cases records, if preserved at all, are apt to be worse than useless. Any hard rock is liable to be called granite; Lower Palæozoic shales, if they are black, become coal measures; all red rocks are New Red Sandstone. This is not the time for suggesting the exact form of the machinery which should be created for the preservation of boring records, but I take the opportunity of expressing to you my opinion that our methods of exploration are at present happy-go-lucky and unworthy of our great mineral heritage."

The matter remains as it was in 1905. Since that time many deep boreholes have been put down, especially in Kent. The majority of them were kept secret, and no access to the cores allowed to the Geological Survey. For a time the only published account in existence of some boreholes made by British companies was to be found in a German periodical called "Glückauf," published in Essen (No. 27, July 6, 1912); of others the German account is still the only available record. Truly our machinery must be deficient, when information of such value can go so far astray.

This, however, is a digression from my subject. It was the search for, and exploring of, concealed coal-fields to which I particularly wished to draw your attention, and incidentally to show that unless useless expenditure of money is of no consequence, the search must be conducted on scientific lines.

[A. S.]

WEEKLY EVENING MEETING,

Friday, March 24, 1916.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P.,
Vice-President, in the Chair.

PROFESSOR W. M. BAYLISS, M.A. D.Sc. F.R.S.

The Mechanism of Chemical Change in Living Organisms.

IF we take a general view over the large field of chemical reactions known, we notice that there is a great variety in the *rate* at which these reactions take place. Some, and especially those in which electrical forces play a part, reactions between inorganic ions, are practically instantaneous. They are familiar to all in the precipitations of the analytical chemist. Others, such as the hydrolysis of cane-sugar by water, are so slow as to be incapable of detection at ordinary temperatures, unless a very long time is allowed. There are, moreover, all possible stages intermediate between these extremes. Reactions between carbon compounds are, generally speaking, comparatively slow; but, as the name "organic" indicates, they are the characteristic changes of the living cell.

Early workers in the domain of physiological chemistry—Schönbein, for example—were struck by the fact that reactions which require, in the laboratory, powerful reagents, such as strong acids and high temperatures, to make them take place at a reasonable rate, occur rapidly in the living organism at moderate temperatures and in the presence of extremely weak acids or alkalis. I may refer to the decomposition of proteins into their constituent amino-acids, which is a part of the normal process of digestion, but, when ordinary laboratory methods are used, requires boiling for several hours with concentrated hydrochloric or sulphuric acid.

The problem before us, then, is to discover how a slow reaction can be made to go faster. The most obvious and well-known method of doing this is by raising the temperature; but this is clearly out of the question in living cells. Another possibility is to make use of mass action, increasing by some means the effective concentration of the reacting substances; in this way the number of contacts per unit time would be raised. This is possible in the cell. There remains a third, the formation of an intermediate compound with another substance. This compound may be supposed to be both formed and again decomposed at a rapid rate, so that the total time taken is much less than that of the original reaction.

Now it is evident that something of the kind contemplated by these two latter possibilities is at the bottom of the process called "catalysis" by Berzelius. This chemist directed attention to the numerous cases known, even at his time, where the presence of a third substance brings about an enormous acceleration of a reaction, without itself taking part in it, so far as appears at first sight; at all events, this third substance reappears at the end unchanged. An example is the effect of finely divided platinum on hydrogen peroxide. Similar phenomena were known to Faraday, and described by him about the same time, but without giving them a special name.

Agents of this kind were soon discovered to be present in living cells. Such catalysts are called, for convenience, "enzymes," as suggested by Kühne, although there is no real scientific necessity for the name. That of "ferments" is still sometimes used, and is not now liable, as it was in Kühne's time, to cause confusion by application to living microbes.

Since catalysts are, as a rule, found unchanged at the end of their work, it is clear that they do not themselves afford energy for the purpose. Indeed, the energy change of a catalysed homogeneous system is the same as that of the reaction when proceeding at its ordinary slow rate. How, then, do they act?

The first thing to note with respect to enzymes is that they are capable of activity in media in which they are insoluble. Whatever may be the nature of this activity, therefore, it is exerted by the surface of the catalyst. We may then reasonably ask, as the most obvious hypothesis, is there ground for holding that the increased rate of reactions brought about by enzymes is effected by increase of concentration of the reagents at the surface and consequent acceleration of the reaction by mass action? We know that substances which lower surface energy of any form are concentrated at such boundary surfaces. The process is well known as "adsorption," and is a consequence of the operation of the principle of Carnot and Clausius, which states that decrease of free energy always occurs, if it is possible for it to do so. In fact, such an explanation was given by Faraday of the effect of metallic platinum in causing combination of oxygen and hydrogen gases. Although the name "adsorption" was not used in this description, Faraday had very clear ideas of the process, and gives several interesting cases. He showed that the necessary condition for the activity of platinum in the case referred to is a chemically clean surface, in order that the gases may condense on it. It matters not whether the removal of deposit is effected by mechanical polishing; by the action of acid or of alkali; by oxidation or reduction—making it either anode or kathode in an electrolytic cell will serve. It should be mentioned that this view did not receive universal acceptance, but the fact that it recommended itself to the keen insight of Faraday is powerful evidence in its favour.

I would not venture to state that this hypothesis is yet in a position to explain all the facts met with in the action of enzymes themselves, but it is remarkable how many receive a satisfactory account. We are at once confronted by the difficulty of the considerable number of different enzymes. But we must not forget that adsorption is controlled by a great number of factors in addition to mechanical surface tension. All those properties which suffer modification at phase boundaries play their part—electrical charge, solubility, compressibility, even chemical reaction itself, may be mentioned. Moreover, as Hardy has pointed out, the act of condensation in itself may well be accompanied by the manifestation of molecular forces which result in increased chemical potential of the reacting substances. It is clear that experimental decision of the questions involved is almost impossible until we have in our hands pure preparations of enzymes. We cannot as yet exclude the possibility of the formation of intermediate *chemical* compounds between enzyme and substrate, but their existence has not been demonstrated, and what I may venture to call Faraday's view has the advantage of simplicity, and thus the support of William of Occam's "razor."

The important question of the synthetic action of enzymes demands a little attention at this point. All reactions may be regarded as being, in principle, reversible or balanced, and the greater part of those of the living organism are found experimentally to be so. If we take for consideration those enzymes the action of which consists in the addition or removal of the elements of water, we find that, as would be expected from the law of mass action, the position of equilibrium in the presence of a large excess of water is very near to that of complete hydrolysis, and this is the state of affairs in the usual laboratory experiments. On the other hand, the less water is present, the greater is the preponderance of the opposite—synthetic—aspect. Take the classical case of ethyl acetate. If the ester and water are mixed in molecular proportions, hydrolysis to acid and alcohol occurs until two-thirds of the ester are decomposed. Moreover, the same final composition is obtained if we commence with acid and alcohol, and so work in the other direction. But these reactions proceed by themselves with extreme slowness, taking months before coming to an end. But the presence of a catalyst, uch as mineral acid, brings about equilibrium in an hour or so, and we notice that it is the same as the spontaneous one. An enzyme, known as lipase, also brings about equilibrium rapidly. The important point in respect of the mechanism of living cells is that by changing the available amount of water, the reaction may be made to proceed in either direction at will. The series of curves given by Armstrong and Gosney (Proc. Roy. Soc., 88B, p. 176) show this fact very clearly. Further, if the equilibrium is brought about rapidly, even if to any position except that of complete change in one or the other direction, the enzyme must accelerate *both* reactions, and any hypothesis of special "synthesising" enzymes is superfluous. This is essentially the position taken by van't Hoff in the work with which he was engaged at the time of his death. What is required, then, is a means by which the cell is enabled to change the available water at the disposal of reactions occurring therein. We do not as yet know the precise nature of such mechanisms, but there is reason to believe that they are provided by changes in the surface area of colloidal constituents or in the power of imbibition possessed by certain contents of the cell.

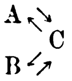
We here come across an interesting problem which cannot be said to be solved satisfactorily at present. We have seen that the equilibrium position of an ester system when reached rapidly under the action of a soluble catalyst is the same as the spontaneous one. But there is a certain difference when a heterogeneous catalyst, or enzyme, is used. Nevertheless, the equilibrium is a true one, being in the same position when approached from either end. The amount of butyric acid combined as amyl ester in a particular system under acid catalysis was found by Dietz to be 88 per cent. of the total; under the action of the enzyme lipase it was only 75 per cent. This fact has given rise to various suggestions, and has troubled people's minds because it appears to give a possibility of evading the second law of energetics. Now, it was pointed out to me by Prof. Hopkins that, on the hypothesis of a rapid attainment of equilibrium by condensation on the surface of the enzyme, it is necessary, if the natural equilibrium is to be unaltered, that adsorption of all the components of the system should be the same proportion of each, because the position of equilibrium must be the same on the surface of the enzyme as that which results in the body of the solution. In the presence of a large excess of water, it does not seem likely that a difference of equilibrium owing to this cause could be detected. But this should be possible when the equilibrium position is nearer the middle, so to speak, and I am at present engaged in experiments on the question. At any rate, difference in adsorption may be the cause of the phenomenon of Dietz. It would simply imply that water is adsorbed by the enzyme in relatively larger proportion than the other constituents of the system. It should be remembered that the solvent in these experiments was amyl alcohol containing about 8 per cent. of water, and, as Arrhenius has shown, all substances present are adsorbed, although the laws governing the relative proportion of these various substances are not yet completely worked out.

We see, by consideration of the facts relating to the action of enzymes, how important a part is played by changes in the rate of reactions, and there are two further points to which attention has been directed by Prof. Hopkins. Take, first, a series of reversible reactions in which the products of one form the starting-point of the next following:—

$$\mathrm{A} \rightleftarrows \mathrm{B} \rightleftarrows \mathrm{C} \rightleftarrows \mathrm{D} \rightleftarrows \text{ etc.}$$

If the rate at which B is converted into C is greater than that at which A changes into B, it is obvious that the amount of B present at any moment may be extremely small, although the whole of the final products have passed through the stage. The fact warns us from estimating the importance of any particular constituent of the cell by the quantity to be obtained.

The second point is this. Suppose that there are two independent reversible reactions, both leading to the same product, C,



and that $A \longrightarrow C$ is more rapid or easier that $B \longrightarrow C$. This latter reaction will be practically absent, being balanced by the excess of C. But, if the former reaction is abolished by the removal of A, then $B \longrightarrow C$ will take place in proportion as C is used up in other reactions. Thus, under special conditions, a reaction may take place which is not detectable under normal conditions, although capable of taking place.

One of the most difficult questions is the manner in which the various components of the cell are prevented from entering into chemical reaction except when required. Enzymes, for example, are not always in activity. The conception which states that the cell consists of numerous minute "reaction chambers," separated from one another by membranes, seems to present most possibilities. These membranes must be regarded as capable of removal and of reconstruction, or reversible as regards their permeability. The food vacuoles of an Amœba may serve as an illustration of such chambers on a comparatively large scale. In these vacuoles digestion processes are going on independently of other reactions in various parts of the same cell protoplasm, although this latter behaves as a liquid.

The general conclusion to which we arrive is that velocity of reaction plays an exceedingly important part in the regulation of cell mechanics. I venture to think that the conception is destined to replace static points of view, such as that of "lock and key" or the fitting together of molecular groupings. That there is still very much to be discovered is obvious. We have to find out how the living cell is able to modify and adjust together the large number of reactions known to the chemist. The study of the methods by which the rate of these reactions is affected is one of the most valuable of those accessible to us.

[W. M. B.]

WEEKLY EVENING MEETING,

Friday, March 31, 1916.

Edward Pollock, F.R.C.S., Vice-President, in the Chair.

Professor A. Fowler, F.R.S., Sec. R.A.S.

The Spectra of Hydrogen and Helium.

The investigation of the spectra of hydrogen and helium is intimately connected with problems apparently so diverse as the constitution of the atom and the order of celestial evolution. Hydrogen is remarkable as yielding the simplest of all known spectra, and, though helium ordinarily presents greater complexity, it is now believed to be capable of emitting another spectrum which rivals in simplicity that of hydrogen itself. These simple spectra naturally make a special appeal to those who are attempting to calculate the spectra of the elements from theoretical considerations relating to the structure of atoms, for here the problem is reduced to its simplest terms. The two elements are also of exceptional interest to astronomers, for hydrogen shows itself in nearly every self-luminous celestial body which has been observed, while helium is characteristic of an important special class of stars. Indeed, helium, as everyone knows, received its name from the occurrence of its yellow line D_3 in the sun's chromosphere, long before it was discovered on earth by Ramsay; and the spectrum of hydrogen is more completely displayed in some of the stars, and in the chromosphere, than in any laboratory experiment which has yet been made. It may therefore be of interest to bring together some of the principal results which have been obtained in connexion with the spectra of these two elements.

Let me first recall the spectrum of hydrogen, which we can readily obtain from a hydrogen vacuum tube. If we take a tube at random we shall most likely see two spectra superposed. One of them consists of many hundreds of faint lines, constituting what is called the *secondary spectrum*, while the other includes the well-known lines of the so-called *primary spectrum*, beginning with H_α in the red, and following on with H_β in the blue-green, H_γ in the blue, and so on. Either of these spectra can be suppressed, or nearly so, at will. Generally speaking, narrow tubes or strong discharges favour the primary spectrum, while the secondary is best developed in wider tubes or with weaker discharges.

Although for some time considered as possibly due to impurities, it is now generally conceded that this secondary spectrum really belongs to hydrogen. The spectrum is so complex as almost to defy analysis, but an important beginning has been made by Fulcher, who has shown that some of the constituent lines in the green and red are arranged like those of a band spectrum. These particular lines are relatively stronger at low pressures, and their near equality of step is clearly seen in small-scale photographs. On current theories, these band lines would be considered to emanate from hydrogen molecules, but it is not yet certain that this is true of all the lines of the secondary spectrum, as some of them show considerable Zeeman effects in the magnetic field. The secondary spectrum offers less attraction to astronomers than to physicists, for it has not yet been recognized in any celestial body whatsoever.

The primary spectrum of hydrogen is of more immediate interest. It may be obtained conveniently from an ordinary vacuum tube having a narrow capillary, if the gas be at a pressure of a few millimetres. Sealed tubes, however, soon deteriorate, in the sense that they give undue prominence to the secondary spectrum at the expense of the primary. Mr. Guild, of the National Physical Laboratory, has lately shown that this arises from increase of pressure caused by the evolution of gas from the electrodes, and that the life of a tube intended for the primary spectrum may be much prolonged by the addition of a large bulb, so that the rate of increase of pressure is much reduced. You will observe that the red glow of this tube is in striking contrast with the blue of the tube which gave us a strong secondary spectrum.

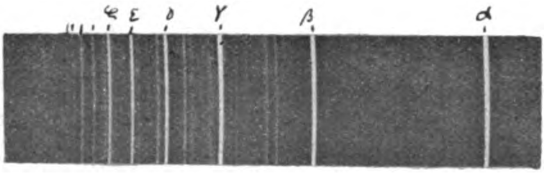


Fig. 1.—Balmer Series of Hydrogen Lines.

When we examine the primary spectrum, we are at once struck by the wonderful regularity in the arrangement of the lines (Fig. 1). We see how the lines gradually become closer together and diminish regularly in intensity as we pass from the red to the ultra-violet. It was not until 1885, however, that the law connecting the lines was discovered. It was then found by Balmer that the wave-lengths of the known lines could be accurately expressed by the simple formula

$$\lambda = 3646 \cdot 14 \frac{m^2}{m^2 - 4}$$

where λ is the wave-length in Ångström units (10^{-10} metre), and m takes the value 3 for H_α, 4 for H_β, and so on.

It is instructive to re-write the equation in the form

$$\lambda = 3646 \cdot 14 + \frac{3646 \cdot 14 \times 4}{m^2 - 4}$$

from which we see at once that there can be no members of this series beyond λ 3646·14, for this is the value of λ when m is infinite. The wave-length 3646·14 is thus the end, or "limit," of the Balmer series.

In such formulæ there are often very definite advantages in expressing the positions of the lines in wave-numbers rather than in wave-lengths, the wave-number being $10^8/\lambda$, or the number of waves per centimetre, corrected to vacuum. In these terms, the Balmer formula becomes

$$n = 27{,}418 \cdot 75 - \frac{109{,}675}{m^2}$$

or

$$n = 109{,}675 \left(\frac{1}{2^2} - \frac{1}{m^2}\right)$$

Balmer's formula clearly permits the calculation of an indefinitely great number of lines, but in laboratory experiments even ten or a dozen are not obtained without a little trouble. Sir James Dewar has found that admixture with helium tends to the better development of the ultra-violet members of the series, but it is in the white stars and in the sun's chromosphere that the Balmer series is most completely exhibited. In some of the stellar spectra photographed by Sir William Huggins we can count up to about the 20th member. In the chromosphere, during eclipses, the Astronomer Royal has photographed as far as the 28th, and Professor Mitchell as far as the 34th line. The Balmer formula continues to represent the lines with astonishing accuracy, so that no one hesitates to attribute the ultra-violet members to hydrogen, although experimental proof of their hydrogen origin is entirely wanting.

Besides the Balmer series there are other lines of the primary spectrum of hydrogen which do not come within the range of ordinary observation. There is an infra-red series which was predicted by Ritz and observed by Paschen; and another series in the extreme ultra-violet which has been partially recorded by Lyman with a Schumann spectograph. These two series run parallel to the Balmer series and are simply related to it, as we shall see later.

The number 109,675 which occurs in the Balmer formula is an important physical constant, and at the Imperial College Mr. Curtis has taken a great deal of trouble to ascertain its precise value. He obtained a fine series of photographs with the 10-foot concave grating, and it is believed that an accuracy approaching one-thousandth of an A.U. was attained in the measures. The new value for the "Rydberg constant," when expressed on the International scale, is 109,679·22, but this is not final, as it was not possible to take account of the fact that H_α and H_β are very close doublets, which could not be separately measured with the instruments available.

Balmer's discovery of the law connecting the lines of hydrogen soon led to the recognition of numerous somewhat similar series in the spectra of other elements, chiefly through the labours of Kayser and Runge, and of Rydberg. In place of the simple series shown by hydrogen, however, it was found that other spectra usually included several series superposed, so that in many cases the regularity in the arrangement of the lines was only disclosed after patient investigation. Lithium provides one of the simplest examples, and in Fig. 2 we may observe how the seemingly irregular spectrum has been resolved into several series, each of which bears a general resemblance to the hydrogen series. Usually there are three chief series, designated, in order of brightness, the Principal, Diffuse, and Sharp series. There are also certain subsidiary series, of which the one called the Fundamental series is perhaps the most important.

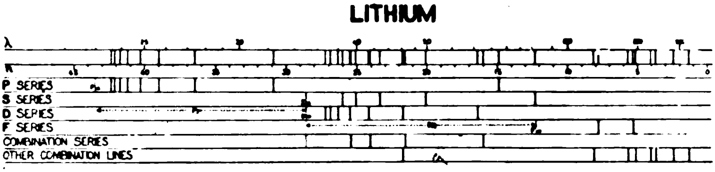


FIG. 2.—Series Lines of Lithium.

The Balmer formula is not applicable to any other series without modification, and Rydberg adopted the general formula

$$n = A - \frac{N}{(m + \mu)^2}$$

where A is the limit of the series, N the number 109,675 derived from hydrogen, and μ usually a fraction. This formula is only approximate, but it was sufficiently exact for the discovery of important relations between different series occurring in the same spectrum. Thus, the limits of the Sharp and Diffuse series are identical, and differ from the limit of the Principal series by an amount equal to the wave-number of the first Principal line. Similarly, the limit of the Fundamental series differs from that of the Diffuse series by an amount equal to the wave-number of the first line of the Diffuse series (see fig. 2). The chief relations are embodied in the following formulæ given by Rydberg:

$$\text{Principal series: } \mathrm{P}\,(m) = \mathrm{N}\left\{\frac{1}{(1+s)^2} - \frac{1}{(m+p)^2}\right\}$$

$$\text{Sharp series: } \mathrm{S}\,(m) = \mathrm{N}\left\{\frac{1}{(1+p)^2} - \frac{1}{(m+s)^2}\right\}$$

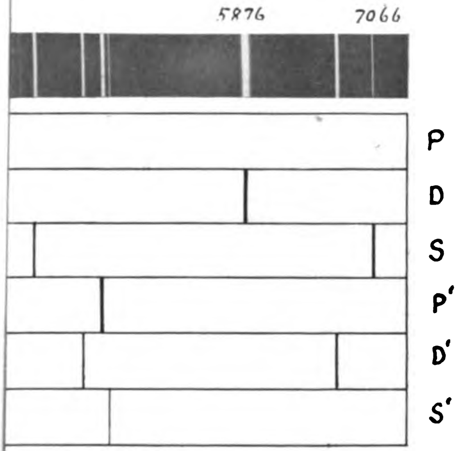
$$\text{Diffuse series: } \mathrm{D}\,(m) = \mathrm{N}\left\{\frac{1}{(1+p)^2} - \frac{1}{(m+d)^2}\right\}.$$

where p, s, and d are the values of μ for the respective series. The main point I wish you to observe is that the Principal and Sharp series are so related that if one of them be known the other can be immediately calculated from it by a mere transposition of terms. More accurate formulæ have since been employed, but the formulæ of Rydberg are the simplest and are sufficiently accurate for our present purpose.

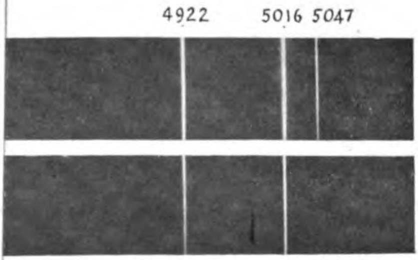
Coming now to the spectrum of helium, it was found by Runge and Paschen that the lines could be included in six series: a Principal, Sharp, and Diffuse series of very close doublets (including the yellow line D_3 at λ 5876), and a similar set of series of single lines. A portion of the spectrum is shown in Fig. 3.

The existence of two complete sets of series, with no recognized connexion between them, was for some time thought to be an indication that helium was a mixture of two gases, and the supposed element giving the family of single-line series was even named "parhelium." This idea was not wholly dependent on spectroscopic observations, but was apparently strengthened by the fact that either of the two systems could be developed with greater intensity than the other by varying the conditions of discharge. Thus, one of the tubes exhibited glows with a pale green light and shows the single line series most strongly, while the other shines with a yellow light and specially emphasises the doublet series. Helium, however, is not unique in showing such variations, or in giving more than one family of series, and the view that it might be a mixture of gases has long been abandoned.

Helium, like hydrogen, has also a very complex secondary spectrum, though it escaped notice for many years. It was missed, I think, because those who made helium-tubes in the early days were too sparing of the gas. It was discovered independently at the Imperial College and by Goldstein, and may be obtained quite brilliantly in the wider parts of a tube containing helium of sufficient density, if a small condenser and spark-gap be included in the circuit. This spectrum is definitely banded, but although each band is built up on the standard pattern for bands, some of them are arranged in accordance with the law of line series. In this respect helium is


5876
7066
P
D
S
P'
D'
S'

ngement in Series.


4922
5016
5047

(b) CONDENSED DISCHARGE.

ithout Condenser.

unique. The helium *bands* are not known to occur in any celestial spectrum.

The *line* spectrum of helium, however, is abundantly represented in the heavens. All the principal lines occur as bright lines in the chromosphere, though they do not ordinarily appear among the Fraunhofer lines in the spectrum of sunlight. They are also the characteristic feature of the "helium stars," many of which are found in the constellation of Orion.

A very important chapter in the story of hydrogen and helium was opened in 1896 by Professor Pickering's discovery of a new series of lines in the spectrum of the star ζ Puppis (Fig. 4). These lines occupy positions intermediate between the lines of the Balmer series, and within the limits of error of the measurements, the two series appeared to converge to the same limit. Also, if in the Balmer formula *m* be given successive values 3½, 4½, &c., the resulting wave-lengths are in close agreement with those of the Pickering lines. Now, these relations are appropriate to two subordinate series of the same element, and it was at once suggested by Pickering that the new series might also be due to hydrogen. Rydberg took up the question, and concluded that the Pickering lines formed the Sharp and the Balmer lines the Diffuse series of hydrogen, and he proceeded to calculate the Principal series from the Sharp in the way I have already indicated. Rydberg's scheme for hydrogen, in wave-numbers, thus became

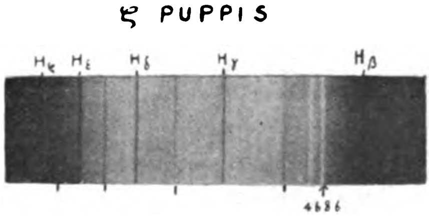


Fig. 4.—Spectrum of ζ Puppis

$$n = 109{,}675 \left\{ \frac{1}{2^2} - \frac{1}{m^2} \right\} \qquad \text{(Balmer series)}$$

$$n = 109{,}675 \left\{ \frac{1}{2^2} - \frac{1}{(m + 0\cdot5)^2} \right\} \qquad \text{(Pickering series)}$$

$$n = 109{,}675 \left\{ \frac{1}{1\cdot5^2} - \frac{1}{m^2} \right\} \qquad \left(\begin{array}{c}\text{Calculated}\\ \text{Principal series}\end{array}\right)$$

giving P(1) = λ 4688 ; P(2) = λ 2734, &c.

That is, if Rydberg were right, some of the stars ought to show a line in the blue, about wave-length 4688, and it should be very bright because it would be the first member of a Principal series. The other lines were not to be expected in the stars because they lie beyond the region for which our atmosphere is transparent. A very prominent line near 4688—actually at 4686—was in fact already well known in some of the special class of Wolf-Rayet stars, where it occurs as a bright line, in company with fainter bright lines of the Pickering series. It occurs also as a bright line in ζ Puppis, and in many of the gaseous nebulæ. A very interesting example of its occurrence is in the Ring Nebula in Lyra, where Max Wolf's photographs show it to be restricted to the apparently dark central space, which must accordingly shine with a feeble blue light.

Rydberg's view as to the constitution of the hydrogen spectrum thus appeared to be amply confirmed by astrophysical spectra. It is true that the new lines had not been obtained in the laboratory, but the numerical evidence of their hydrogen origin seemed strong enough to outweigh this objection. There was thus for many years a pretty general belief that the line 4686 and the Pickering lines were due to hydrogen in a particular state which was only realized under the extraordinary conditions which might conceivably prevail in some of the celestial bodies. Sir Norman Lockyer regarded them as representing one of the last stages of chemical simplification brought about by the action of high temperatures in the hotter stars, and he gave the name "proto-hydrogen" to the supposed simplified form of hydrogen which produced them. The alternative name "cosmic hydrogen" was, I believe, due to Miss Clerke.

The next step in the development of the story arose out of observations of total eclipses of the sun. A high-level chromospheric line in the vicinity of 4686 was first recorded in some photographs which I took for Sir Norman Lockyer in West Africa in 1893; but Rydberg's calculations had not then been made, and its special significance was not apparent. This line was still better shown in another series of photographs which I took under Sir Norman Lockyer's direction in India in 1898; its wave-length was then given as 4686·2, and it was noted that it might be identical with Rydberg's calculated line 4688. It so happened that in order to facilitate the reduction of the eclipse spectra, a number of laboratory comparisons were photographed with the same instrument, and among these were some spectra of helium tubes. Strangely enough, one of these showed a line not previously seen in helium, and it was apparently coincident with the high-level chromospheric line to which I have referred. The wave-length of this line was given by Lockyer and Baxandall a few years later as 4685·97. The tube which gave the mysterious line appears to have got broken, and it was not known whether the line was due to an unknown impurity, or to some special condition of discharge which had accidentally been dropped upon.

This old observation was kept in remembrance during occasional experiments on helium extending over several years, until at last the line 4686 was recovered very faintly in a tube which I had prepared for a British Association lecture in the summer of 1912. With this beginning, a systematic investigation resulted in due course in showing how the line might be produced at will, and with almost any desired intensity. Generally speaking, the whole secret was to apply condensed discharges of sufficient strength. The denser the gas, the greater the intensity of the discharge required to produce the line, and the brighter and broader the line obtained. If we make the experiment, you will see that the treatment is rather severe; indeed, the tubes very frequently break down under the strain. In Fig. 5 the spectrum given by a heavy condensed discharge in a tube at about 5 mm. pressure is compared with that given by an ordinary uncondensed discharge in the same tube, and it will be seen that the line 4686 appears with great intensity. At lower pressures the line is less bright, but more sharply defined and better suited for accurate measurement; the resulting wave-length was 4685·98, in close agreement with the corresponding line in the chromosphere, stars, and nebulæ. It was also believed to be in sufficient accordance with the wave-length 4687·88, calculated by Rydberg for the supposed first Principal line of hydrogen.

Identifications resting on a single line are justly looked upon with some suspicion, but in this case confirmation was apparently given by the presence of other lines calculated by Rydberg at 2733, 2385, and 2253. These lines were not in absolute agreement with Rydberg's calculations, but very nearly so, and within the possible range of error of the formulæ.

Having now obtained the long-sought lines, considerable embarrassment was caused by the fact that more lines were obtained than were expected. Besides the Rydberg lines, there was an intermediate set of lines at 3203, 2511, and 2306. However, as these lines could be put in a series having the same limit as the 4686 series, and as hydrogen might be supposed to have still another peculiarity, they were provisionally regarded as forming a *second* Principal series of hydrogen.

The experiments were less successful as regards the reproduction of the Pickering lines, but three of them were obtained with sufficient brightness for approximate measurement; their wave-lengths were 5411, 4541, and 4200. The difficulty with regard to these lines is, I believe, that the conditions which would be expected to brighten them cause them to become so diffuse as to be unrecognizable, for this is certainly what happens in the case of the ultra-violet members of the 4686 series. The stars manage these experiments rather better than we do.

Since hydrogen was always present as an impurity in the helium tubes employed, the new lines were at first attributed to hydrogen, in accordance with Rydberg's theory. They seemed to have a close numerical relation to the hydrogen series, and no relation whatever to the lines of helium. Moreover, there was evidence that helium aided the development of the ultra-violet lines of hydrogen, and it was reasonable to suppose that it might also facilitate the production of the lines under consideration. Nevertheless, the idea that the new lines were due to hydrogen was not wholly satisfactory, as the occurrence of lines not anticipated by Rydberg broke the analogy with the series of other elements. It therefore seemed desirable to seek for other examples of such series, in order to test the value of the numerical evidence on which the assignment of the new lines to hydrogen was still so largely dependent.

A series of the same character as the 4686 series, consisting of spark-lines of magnesium, had already been discovered, when fresh light was thrown on the subject from another quarter. Not from the stars this time, but from mathematical physics. In July, 1913, Dr. Bohr published the first instalment of his now famous theoretical investigation of the constitution of the atom in relation to spectral emission, and the lines of the 4686 and Pickering series became of great importance in this connexion.

Beginning with the Rutherford model of the atom, and introducing Planck's conception of the quantum of energy, Dr. Bohr quantitatively derived Balmer's formula for the hydrogen spectrum, and in such a form as to exclude the 4686 and Pickering series entirely. On this theory, the atom of hydrogen is supposed to consist of a relatively massive, but very minute, nucleus which is positively charged, and a single electron in orbital motion around it. The electron may revolve in one of certain specified orbits, subject to ordinary dynamical laws, exactly like a planet in motion around the sun, and in these so-called "stationary states" it radiates no light. When under the influence of electric discharge, we are to suppose that the electron is removed from the nucleus, and that on its return it jumps from one stationary state to another; in so jumping it radiates definite amounts of energy which determine the frequency, and therefore the positions, of the spectral lines. At any instant a given atom only contributes to one line of the series, but the summation of the radiations from a large number of atoms accounts for the whole series.

The helium atom is regarded as consisting of a doubly-charged nucleus of four times the mass of the hydrogen atom, and two electrons revolving round it. Moderate discharges are only supposed capable of detaching one of the electrons, and the ordinary spectrum of helium is emitted when this returns to the system; the theory of this spectrum, however, remains incomplete. Under the action of sufficiently strong discharges, both electrons are considered to be removed from the nucleus. The theory is simpler in this case, and Dr. Bohr derived a formula for the series of lines which should be emitted when only one of the electrons is regained by the nucleus. In a first approximation, the general formula is identical with that for hydrogen, except that the Rydberg constant has four times its ordinary value. This formula gives lines coinciding in position with the Principal series calculated for hydrogen by Rydberg, and also with those of the supposed second Principal series, and unites them in a single series of a new type. Not only this, a subsequent correction of the formula, no longer regarding the mass of the electron as negligible in comparison with that of the nucleus, accounted *exactly* for the slight differences between the positions of the lines 4686, etc., calculated by Rydberg and those in which they had been observed. Thus, according to Bohr's theory, the new helium-tube lines were to be attributed to a simplified form of helium and not of hydrogen. For the sake of distinction, we may conveniently follow Lockyer's notation and refer to the new spectrum as that of "proto-helium." Bohr's results may then be expressed as follows :—

Hydrogen—

$$n = N\left(\frac{1}{p^2} - \frac{1}{m^2}\right) \quad \begin{cases} p = 3 \text{ for infra-red series.} \\ p = 2 \text{ for Balmer series.} \\ p = 1 \text{ for extreme ultra-violet series.} \end{cases}$$

Proto-helium—

$$n = N^1\left(\frac{1}{p^2} - \frac{1}{m^2}\right) \quad \begin{cases} p = 4 \text{ for Pickering series.} \\ p = 3 \text{ for "4686" series.} \end{cases}$$

In the first approximation $N^1 = 4\,N$, but taking account of the mass of the electron, the theoretical expressions for N and N^1 are

$$N = \frac{2\pi^2 E^2 e^2 Mm}{ch^3 (M + m)}$$

$$N^1 = \frac{2\pi^2 (2E)^2 e^2 (4M) m}{ch^3 (4M + m)}$$

where E, M = charge and mass of nucleus of hydrogen atom ; e, m = charge and mass of electron ; c = velocity of light ; and h = Planck's constant. Using the best available determinations of these quantities, the resulting value for N in the formula for hydrogen is as near to 109,675 as can at present be expected. The ratio of N^1 to N can be determined precisely from the formulæ, as the more doubtful quantities cancel ; taking $M/m = 1835$, we find $N^1 = 4 \cdot 001632\,N$, in remarkable agreement with the value determined from the actual spectra. Assuming the truth of the theory, the spectroscopic data thus provide a valuable check on the generally adopted value for the mass of the electron.

Experimental support for Bohr's interpretation of the 4686 series was seemingly given by the subsequent observations of Evans and Stark, who found, as I have since found myself, that the line 4686 can be obtained from helium tubes which show no trace of hydrogen. But it is always difficult to be certain that no hydrogen remains in the tubes ; it is apt to hide itself in the glass, or in the electrodes, and to come out when not expected.

Other evidence in favour of Bohr's view, however, was found in a more general investigation of series of enhanced lines—that is, of lines which are intensified in passing from the arc to the spark spectrum. In the first instance, some additional lines of magnesium associated with the well-known spark line 4481 were discovered, and it was found that they could be arranged in a series of the new variety indicated by Bohr in connexion with the 4686 series of the helium tubes. Further experiments revealed many other enhanced lines of magnesium which had previously escaped observation, and it was found that they formed a large family of related series, generally similar to the ordinary series, but differing from them in requiring a "series constant" having four times the value of that derived from hydrogen. The same was found to be true of the enhanced lines of calcium, strontium and barium.

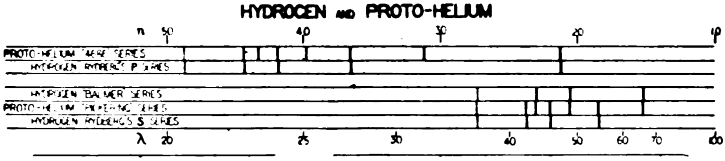
We have therefore now to recognize two distinct classes of series : (1) series of the arc type, having Rydberg's N for the series constant ; and (2) series of the spark type, having 4 N for the series constant. No relation has been found between the arc and spark series occurring in the same spectrum, and they seem to be as distinct as if they belonged to different elements. A logical extension of Bohr's theory leads to the supposition that series of the usual type are produced when only one electron is detached from each atom by the exciting agency, and those of the spark type when *two* electrons have been removed and *one* is regained by the nucleus.

Now the 4686 series is clearly one of the spark type, both as regards its mode of production and the formula which represents the lines. It can therefore no longer be supposed to be associated with the hydrogen series, which is of the arc type. We now see that the numerical relations between them are incomplete, and only approximate, and that they furnish no evidence of a common origin. Taking account also of the experimental results, we are therefore forced to the conclusion that the 4686 series is not due to hydrogen but to helium, as required by Bohr's theory. If it was one theory that led us astray in the first instance, it was another that helped to set us to rights. Nevertheless, the assignment of the 4686 series to a form of helium may now be considered to be independent of any theory.

The chief outstanding point needing confirmation by observation was that the Pickering series—which should run parallel to the 4686 series and also form a series of the spark type—should include intermediate lines very close to the lines of the hydrogen series and on their violet sides, as indicated in Fig. 6. The existence of these lines could not be proved from the available astronomical data, but at the same time there was no proof of their absence. Experimental evidence on this point, however, has been obtained by Evans, who succeeded after much patient work in photographing some of the lines in the positions predicted by Bohr. Further observations of these lines appear also to have been made very recently by Paschen, but full particulars are not yet to hand. The complete Pickering series, as we now know it, is shown in Fig. 6, from which it will be seen that Rydberg's theory only accounted for one-half of the lines, as was the case with the 4686 series.

Other predictions, relating to the composite structure of the lines of hydrogen and proto-helium, which have been made by Sommerfeld, based upon an extension of Bohr's theory, have also been partially verified by Evans and Paschen independently.

Whatever approach to the truth may be represented by Bohr's theory, it has at least been brilliantly successful in its application to the spectra of hydrogen and proto-helium. Prof. Nicholson has attempted, without success, to extend the theory to the ordinary spectrum of helium, but Dr. Bohr does not accept this negative result as final.


HYDROGEN AND PROTO-HELIUM
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PROTO-HELIUM 4686 SERIES
HYDROGEN RYDBERG'S P SERIES
HYDROGEN BALMER SERIES
PROTO-HELIUM PICKERING SERIES
HYDROGEN RYDBERG'S S SERIES
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70
100

Fig. 6.

The primary spectrum of hydrogen is thus, after many years, restored to its original simplicity, consisting only of the Balmer series and the parallel series in the infra-red and far ultra-violet. There is neither astronomical nor experimental evidence that it contains any other lines than these. For once, things are what they seem, and the primary spectrum of hydrogen is as simple as it looks.

If time had permitted, I might have attempted to indicate the importance of the lines now assigned to proto-helium in relation to the order of celestial evolution. They are among the very few lines of known origin which appear in the spectra of gaseous nebulæ and Wolf-Rayet stars, and although their mere presence adds nothing at all to our knowledge of the chemistry of these bodies, they may teach us a great deal more in view of what we have been able to learn from laboratory experience as to the special conditions requisite for their production. Even more important, the new work on helium points the way to further research regarding the unknown lines which appear in the spectra of bodies which are generally believed to be at an early stage of evolution. By similar experimental methods, Dr. Merton has, in fact, already shown that some of the most characteristic lines of the Wolf-Rayet stars are probably due to a form of carbon. The surprising thing, to my mind, is that the resources of our laboratories have already proved sufficient for the reproduction of some of the spectral lines which appear to require the most extreme conditions for their development in celestial bodies.

Anyone looking at our extensive maps and catalogues of spectral lines might well form the opinion that we already have quite enough to occupy our attention, but I hope I have conveyed the impression that lines are of varying degrees of interest, and that we may still hope to profit from further attempts to imitate the experiments which are continually in progress in the laboratories of the stars.

[A. F.]

GENERAL MONTHLY MEETING,

Monday, April 3, 1916.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

John Dewrance,
Samuel Nivison,
Arthur Edward Parsons,
Samuel Smiles, D.Sc.,

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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  Page's Weekly for March 1916. 8vo.
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  Power-User for March 1916. 8vo.
  Science Abstracts for March 1916. 8vo.
  Tchèque, La Nation, for March 1916. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 7, 1916.

J. H. Balfour Browne, K.C. D.L. LL.D., Vice-President, in the Chair.

William Samuel Lilly, M.A., Hon. Fellow of Peterhouse, Cambridge.

A Plea for War.

[For Discourse see "Dublin Review," July, 1916, Vol. 159, page 68.]

ANNUAL MEETING,

Monday, May 1, 1916.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S.,
President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1915, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Fifteen new Members were elected in 1915.

Sixty-two Lectures and Nineteen Evening Discourses were delivered in 1915.

The Books and Pamphlets presented in 1915 amonnted to about 276 volumes, making, with 527 volumes (including Periodicals bound) purchased by the Managers, a total of 803 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S.

MANAGERS.

Sir Thomas Barlow, Bart, K.C.V.O. M.D. LL.D. F.R.S.
Sir William Phipson Beale, Bart., K.C. M.P.
Horace T. Brown, LL.D. F.R.S.
Sir James Mackenzie Davidson, M.B.C.M.
Field Marshal The Right Hon. Lord Grenfell, G.C.B. G.C.M.G.
Charles Hawksley, M.Inst.C.E.
Colonel Sir Frederic Nathan, R.A.
The Hon. Richard Clere Parsons, M.A. M.Inst.C.E.
Sir James Reid, Bart., G.C V.O. K.C.B. M.D. LL.D.
Sir Napier Shaw, LL.D. Sc.D. F.R.S.
Alexander Siemens, M.Inst.C.E. M.I.E.E.
Silvanus P. Thompson, D.Sc. LL.D. F.R.S.
The Right Hon. Lord Wrenbury, P.C. M.A.
Sir Alfred F. Yarrow, Bart., M.Inst.C.E.
The Hon. Sir Robert Younger.

Visitors.

Horatio Ballantyne, F.C.S.
Sidney George Brown, F.R.S.
John F. W. Deacon, M.A.
William Duddell, F.R.S.
Lieut.-Colonel Henry E. Gaulter, F.R.G.S.
James Dundas Grant, M.D. M.A. F.R.C.S.
John W. Jarvis, F.G.S.
H. R. Kempe, M.Inst.C.E.
Francis Legge, F.S.A.
Ernest R. Moon, K.C. LL.B.
Henry G. Plimmer, F.R.S. M.R.C.S.
Sir William Wyndham Portal, Bart., D.L. J.P.
Alfred W. Porter, D.Sc. F.R.S.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.

WEEKLY EVENING MEETING,

Friday, May 5, 1916.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P.,
Vice-President, in the Chair.

Sir James Mackenzie Davidson, M.B. C.M. *M.R.I.*

Electrical Methods in Surgical Advance.

No institution in the world has contributed so largely to electrical science as the Royal Institution of Great Britain. All our modern electrical developments are based to a greater or less extent upon the work of Michael Faraday and his master, Sir Humphry Davy; and it is fitting that in the place which will always be associated with their labours some account should be given of those same electrical developments as applied in the present day to the art of the surgeon.

Surgery has benefited very greatly from electrical science. Before electricity came on the scene, the examination of wounded men who had bullets lodged in their tissues, or bones broken and injured by the impact of bullets, was largely dependent on guess-work. As an early instance of the tentative application of more scientific methods to cases of this kind, I may mention the famous Garibaldi, who, wounded in the ankle by a bullet at the battle of Aspromonte in 1862, suffered for a long time because the wound refused to heal. Distinguished surgeons were sent to his aid, but none of them could decide whether the trouble was due to an impacted bullet in the foot or not, until the celebrated Frenchman, Auguste Nélaton, devised an ingenious arrangement with which he solved the difficulty. He placed at the end of a whalebone probe a button of porcelain, and introduced this into the wound, pushing it forwards as far as it would go; then he twirled it round to make a rubbing contact with whatever it was touching, and on withdrawing it he rejoiced to find upon the tip a black mark caused by lead, thereby clearly proving that a bullet was impacted in the foot. The foreign body was subsequently removed, and the great upholder of the liberties of nations was completely restored to continue his heroic struggles.

Even this ingenious probe of Nélaton, however, was strictly limited in its utility, being able only to detect lead. Nowadays, there would be small scope for such a procedure, in view of the large numbers of wounded men having in their tissues foreign bodies which may be any one of several different metals. Fortunately, we have now at command various methods, of which I shall give you some account this afternoon, for detecting the presence of the embedded metal, and also for determining its exact shape, size, position and the angle at which it is lying, without any additional pain to the patient.

The discovery of X-rays has revolutionized surgical diagnosis. I had the honour some years ago of giving a discourse within these walls on the subject of X-rays, but many advances have been made since then, and I will endeavour to describe and illustrate the methods now adopted. X-rays are produced in a vacuum tube such as I show you, and proceed from a piece of metal fixed in the centre of the tube and called the anode or anti-cathode, or, more popularly, the target. Recently a great improvement in X-ray tube construction has been introduced by Mr. W. D. Coolidge, of Schenectady, U.S.A. His tube, which I have the honour also to show you, has some marked advantages, and very heavy currents can be passed through it, an intense X-ray output being the result. When the electrical current is passed through an X-ray tube, cathode rays—streams of electrically charged particles—are generated at the cup-shaped terminal which is called the cathode, and these, impinging with great velocity upon the target opposite, cause the generation of X-rays at the point of impact. X-rays are simply invisible light—as we now know from the researches of Professor W. H. Bragg—of extraordinarily short wave-length, and owing to this shortness of wave the rays are able to penetrate matter which is opaque to ordinary light. If the human body be interposed between the X-ray tube and the photographic plate, and the tube be excited by an electric current, the plate will show on development a shadow-image, revealing the inner structure of the tissues through which the rays have passed. The bones being denser than the flesh, these will appear white in comparison, and should any metal be present which is denser than either, it will be rendered still whiter in the negative. The opacity of various kinds of substances to X-rays follows the order of their atomic density; thus matter composed of light atoms such as flesh, skin, hair and clothes, allows X-rays to pass through very readily, whereas all metals, except the lightest ones, such as aluminium, are extremely opaque. Bone, seeing that it contains phosphorus and lime, stops the passage of the X-rays much more readily than flesh, which is composed of the lighter atoms, namely, carbon, hydrogen, nitrogen, and oxygen.

The shadow picture which is produced by the impingement of the X-rays upon a photographic plate or a fluorescent screen may, therefore, furnish a good deal of information as to the relative densities of any interposed materials; but however realistic this image may appear, it should always be remembered that it is not like an ordinary photograph from which the relative positions can be inferred accurately enough. It is a shadow of the object, and therefore may be very misleading. As an illustration of its possibilities of error, I may throw on the screen a slide showing, first, an ordinary photograph of a human skeleton with German bullet situated on the front of the breast bone, in the middle line. That shows the actual situation of the bullet, and next we have the X-ray photograph of this same skeleton with the tube placed centrally. Here it will be observed that the shadow of the bullet appears to be over the spine; it is still shown in the correct middle line, but no information whatever can be gathered from this picture as to its depth, and I do not think that anybody would believe, from a glance at this illustration, that the bullet could be situated on the front of the breast bone. Next we have two X-ray pictures of exactly the same subject, in which, however, the X-ray tube occupied slightly different positions. In the one picture the bullet appears to be in the lung, to the right side of the spine, and in the other, for which the tube was shifted to a slightly different position, the bullet appears to be in the opposite lung. I have prepared this example in order to show how fallacious the single X-ray photograph may be so far as concerns the imparting of correct information with regard to the position of the bullet. In spite of all this, when X-rays are properly used, they can afford data which enable us to ascertain the exact position of a bullet and its exact size.

To illustrate the methods now adopted, I propose to use a small electric lamp instead of an X-ray tube; it is easier to manipulate, and offers quite a serviceable analogy. I have here an opalescent screen to represent the ordinary X-ray screen, and a little electric lamp which will, for the moment, take the place of the X-ray tube, or, rather, of the point in the X-ray tube from which the rays originate. If I interpose a wire cone between the light and the screen, the shadow of the cone will be observed to give, on the flat, no proper idea of the shape of the object itself. If, however, I displace the lamp to one side, it will be found that the shadows alter their respective positions, and although the second single image may be as little informing as the first, it is upon the effect of this displacement that our precise methods are based.

In the first place, there is the stereoscopic method. If I have two little electric bulbs situated side by side, one of them being surrounded by a green film of gelatine, and the other by a red film of gelatine, it will be observed that each casts a shadow of the object from slightly different points of view. Accordingly we have a red shadow and a green shadow of the object. If each eye can be made to see its own image, the combined shadows will give the observer an impression of solidity, and he will see the shadows of this wire cone apparently in relief, just as though he were looking at the actual object. In order that this may be demonstrated, red and green spectacles have been handed round among the audience, and if the ladies and gentlemen will look through these glasses in the ordinary way, holding them close to the eyes, they will be able to see this for themselves. This effect will be possible, of course, only for those who are able to see with both eyes equally well and have binocular sight. Once the stereoscopic picture is obtained, it becomes exceedingly realistic. I would ask those who are able to see these in relief, to turn the glasses round so that the eye which had the red glass in front of it now has the green glass, and *vice versa* ; under these circumstances, they will find the perspective to be reversed, and while in the first instance the narrow end of the wire cone appeared to be nearer, in this case the larger end appears to be projecting itself towards the observer. With the X-ray tube we carry out precisely the same principles. We take one photograph from one point of view, then displace the tube to a known distance, and take a second photograph. The resulting negatives on casual inspection appear to be identical, but on placing them in the stereoscope, they appear in relief, and the exact position of the bullets can be seen in relation to the other parts of the human body.

Any practical use which is made of this method, of course, depends upon the ability of the observer to estimate the size and position of the object, and although it is most useful and invaluable in many respects, it is not precise enough for the purpose of exact localization, and in order to arrive at the measurements required with mathematical accuracy we adopt a different system. In the first place, we require to have a frame in which a pair of wires are made to cross each other at right angles ; this is placed in contact with the fluorescent screen, and the small electric light, which in this experiment represents the X-ray tube, is adjusted opposite the point where the wires intersect and at right angles to the plane of the screen. If, now, any object be introduced, such as a bone with a bullet in some fixed position in relation with it, a shadow of this object will appear on the screen. On the light being displaced sideways, to right or left, or upwards or downwards, to a fixed distance—6 cm. being the most convenient because it coincides approximately with the space between the eyes of the observer—a corresponding shift will be seen to take place in the shadow, and the relative positions of the objects will accordingly be altered. The distance between the source of light and the screen having been carefully measured, a tracing may be taken of each of the shadows of the cross wires and the object when the tube was occupying the two positions. This tracing will reveal the amount of displacement, or parallax as it is called, of the shadow of the bullet in relation to the bone.

The simplest way of interpreting this double tracing is to reconstruct the geometrical conditions under which the shadows were produced. This is most readily done by using a small table with a cross marked on its base, and an upright T-piece above it. Two threads are suspended from the T-piece 6 cm. apart, and the horizontal portion of the T-piece is adjusted to be exactly the same distance from the table as the light was from the screen or photographic plate in the first instance. As the rays travel in straight paths, the threads can be used to trace the direction of the beam, and if the tracing be so placed on this little table that the centre of the cross-shadow is vertically below one of the little slots from which the thread depends, the thread (which is passed through a weighted needle) can be brought down upon a definite part of the tracing of the bullet shadow, and will then indicate the path of the ray which caused the shadow. It must be remembered, and allowed for, that the shadow to the left was produced when the source of the rays was in the position to the right, and *vice versa.* If, now, the second thread be brought down to the corresponding point on the tracing of the other shadow, it will represent the path of the ray when the source of light was in the second position, and it follows that the two threads must cross each other in space. This point of intersection represents exactly the position of the bullet in relation to the body of the patient, and its distance from the table is equal to the actual depth of the bullet from the cross wires when the original observation was made. The distance can be accurately measured with a pair of compasses, and the position of the cross wires being already marked on the patient's skin, it is a simple matter to give the measurements which should guide the surgeon immediately to the detection of the foreign body.

If more rapid procedures are demanded, as they may well be by the exigencies of the present time, these can equally well be carried out on exactly the same principle of similar triangles, the measurement being done actually on a small fluorescent screen. I have here a hand fluoroscope and a device consisting of scale, cross wires, and sliding piece, calibrated so as to enable one by the simplest adjustment to determine the depth of a piece of metal below a marked point on the skin by noting the displacement of the shadow on the illuminated screen when the tube is moved to a given distance.

Having ascertained the position of the bullet in the body, we have at hand, fortunately, other electrical aids for the surgeon when he comes to deal with the problem of its extraction. One of the most useful of these adjuncts is the telephone probe as it has been called, or, as I prefer to call it, the telephone attachment. It consists of a telephone to one terminal of which may be attached the surgeon's steel, silver, or nickel-plated instrument, while a carbon plate is attached to the other terminal, which is applied to the patient's skin, moistened with salt-water to render the skin conducting. When any of the instruments come into contact with embedded metals, such as lead, nickel, copper, iron, or the iron alloys used in shell manufacture, a loud click is elicited, and if a rubbing contact be made with the exploring instrument, the click becomes a sharp rattle. The telephone receiver is attached to the surgeon's head when he is operating, and one section of the wire is spring-clipped on to his knife, probe, needle, or forceps, as the case may be. When the electrical circuit is completed by contact between the carbon plate and the foreign body, a small current is generated, which accounts for the microphonic impression, and thus the surgeon is able to exercise continual control over the foreign body. By means of a suitable relay, it would be a very simple matter, if desired, to make a sound more widely audible.

Another method of bullet extraction used by Captain A. E. Barclay is worthy of mention. It combines the use of the screen with a forceps, and, to replace the tactile sense of the surgeon, wires are employed to lead from the prongs to a bell circuit, so that an electric bell might be rung when both prongs were touching the foreign body.

Yet another device which has lately attracted a good deal of attention, and is largely used in military hospitals in France, is Professor Bergonié's electro-magnet. In this case a large electro-magnet is excited by an alternating current and held over the suspected part. If the magnetic field which is thus created embraces the embedded projectile, a vibratory motion is induced in the projectile, synchronizing with the pulsing of the current. The surgeon palpates the part, and becomes instantly aware of any vibration of the tissues which indicates the presence of the metal. The point of maximum vibration having been selected, he makes an incision at that point, if it is possible to do so, and then the magnet is again used, and the incision deepened in accordance with the information it gives. It is stated that vibration is induced, not only in the ordinary magnetizable metals, but also in some of the non-magnetizable, including aluminium aud copper, and even, with a very powerful instrument, German silver and lead. This is brought about by means of induced eddy currents.

I have attempted, all too hastily, to sketch the methods by which electrical science is aiding the surgeon to repair the wounds of war, but before we leave the subject we might well enlarge the view to include not only the apparatus and technique, but also the men who control it. The first place in our thoughts, and rightly so, is taken by the combatant army, fighting so gallantly in the cause of justice and freedom. It should not be forgotten, however, that behind this combatant army there is another, a shadow army, following the movements of the fighters as exactly as, in the experiments I have shown you, the shadows on the screen followed every alteration in the position of the lamp. This shadow army includes the men whose fingers are always on the fine adjustments and switches, the men who with infinite skill and sureness are using knife and forceps, whole ranks of men engaged in a multitude of remedial tasks, from the stretcher-bearer to the surgeon-general.

This shadow army is in a way prophetic. It is a shadow which, we trust, will increasingly materialize until, in some future day, the only warfare will be, not between man and man, but between a merciful science on the one hand and human suffering on the other. That day may seem distant to those of us who live in times so sadly out of joint as these, but the shadow army is at least a token that it is on its way.

[J. M. D.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Lady Margaret Boscawen,
A. F. Lake,
Mrs. Edith M. Neal,
Lady Petre,

were elected Members of the Royal Institution.

The Chairman reported that His Grace the President had nominated the following as Vice-Presidents during the ensuing year:—

Sir Thomas Barlow, Bart., K.C.V.O. M.D. LL.D. F.R.S.
Sir William Phipson Beale, Bart., K.C. M.P.
Charles Hawksley, Esq., M.Inst.C.E.
The Hon. Richard Clere Parsons, M.A. M.Inst.C.E.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
The Right Hon. Lord Wrenbury, P.C. M.A.
Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S. (Treasurer).
Colonel Edmond Hills, C.M.G. R.E. D.Sc. F.R.S. (Secretary).

The Rt. Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. Sc.D. F.R.S. M.R.I., was re-elected Honorary Professor of Natural Philosophy; and Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. Pres.R.S. M.R.I., was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Concrete for April 1916. 8vo.

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Electrical Industries for April 1916. 4to.

Electrical Times for April 1916. 4to.

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WEEKLY EVENING MEETING,

Friday, May 12, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Arthur C. Benson, C.V.O. LL.D., Master of
Magdalene College, Cambridge.

Vulgarity.

Mr. Benson said that he wished to take a word in common use, the word "vulgarity," which covered a large number of kindred phenomena, and to attempt to analyse it. He quoted the story of the mullet at the dinner-party in "Monte Christo" as a good instance of the unashamed glory of emphasising one's magnificence among less fortunate persons. But there was a certain good humour often to be found in combination with such vulgarity, and frank if complacent ostentation was at least better than the vulgarity which finds no intrinsic interest in things apart from the fact that others find them interesting.

Vulgarity, he said, in its true aspect has no sense of beauty except the beauty which is marketable, and is thus a foe to all progress, because progress can only be attained by those who mistrust the verdict of the world. The vulgar man despises the past and defies the future; he is late with his applause, and is never comfortable till he sees others applauding too.

Mr. Benson indicated Plato and Dr. Johnson as instances of minds where there seemed no least admixture of vulgarity. But turning to the poets and comparing Shelley and Keats with Byron, it was clear that Shelley had no touch of the quality, that Keats had just a superficial wash of it from his early associations, but that Byron was deeply tainted by it, and his life poisoned by stupendous vanity and selfish sensitiveness.

Vulgarity is not, however, a fault of breeding only. It is not a bad standard of taste so much as an excessive admiration of the wrong qualities, a sacred deference for the opinion of the majority.

The opposite of vulgarity is not refinement or fastidiousness, but generosity or disinterestedness. Vulgarity stands for self-confidence and insolence. It exploits men and it ignores God. But it is useless to make a frontal attack upon it; positive virtues are the things to aim at. Life and hope are the indestructible things, better than the most eloquent analysis of faults and failures.

[A. C. B.]

WEEKLY EVENING MEETING,

Friday, May 19, 1916.

The Right Hon. Lord Wrenbury, P.C. M.A.,
Vice-President, in the Chair.

Colonel Edmond H. Hills, C.M.G. D.Sc. F.R.S., Sec. R.I.

The Movements of the Earth's Pole.

More than a century ago it was shown by the mathematician Euler that if the axis round which the earth was rotating were not coincident with the axis of figure, which latter in the case of a spheroidally flattened earth is the shortest axis that can be drawn, the axis of rotation will revolve about the axis of figure in a period which, upon certain assumptions, can be precisely predicted. The time of one revolution of the pole of rotation around the pole of figure depends only upon the shape and degree of elasticity of the earth. In Euler's days the supposition that the solid earth had any appreciable elasticity was so far outside the range of experience that it was not considered by him. He calculated the period of the polar rotation on the assumption that the earth was perfectly rigid, and showed that this period would be about 305 days.

If we determine the latitude of a point on the earth's surface by observations of the stars we are in effect measuring the angular distance between the axis of rotation of the earth and the vertical line, or line through the zenith, at the point of observation. If, now, this axis of rotation moves the observed latitude of the place will change and, if we prolong the observations over a sufficient time, we ought to find that this observed latitude fluctuates backwards and forwards about a mean value with the same periodicity as that in which the earth's pole of rotation moves round the pole of figure.

Every observer who is engaged on making observations to determine the precise positions of the stars, a class of observation which up to a few years ago occupied a very large fraction of the time and energies of astronomers, is actually continually determining and redetermining the latitude of his instrument. There is thus an enormous mass of latitude observations available for examination, and it should prove a not too difficult task to analyse these with the object of detecting a periodic variation. Two causes, however, militated against success in this inquiry: firstly, the very small magnitude of this variation; and, secondly, the fact that the earth is by no means rigid, and hence that the true period of the precessional rotation differs very substantially from the Eulerian period of 305 days.

All the earlier attempts to find evidence of this variation were in fact hampered by this preconceived notion of the ten month period; the observations were carefully scrutinized with a view to detecting it, a process as we now see foredoomed to failure. It would be a useless task to recount here the various attempts that were made. Two of these, however, I should not like to pass over without notice, those of C. A. F. Peters at Pulkowa and Clerk Maxwell in this country.

Peters in his great and classic memoir on the parallax of the fixed stars devoted one section to a discussion on the variability of the latitude in a ten month period. He found that the actual variation derived from the observations was of so minute a magnitude that it was well within the limits of unavoidable sources of error and he therefore concluded that if there was any separation of the two poles it was too small to be detected by observation.

Clerk Maxwell examined the Greenwich observations of *Polaris* in 1851–54 and thought he found some small indications of maxima at about ten month intervals, but he considered the results as very doubtful and that more observations would be required to establish the existence of so small a fluctuation.

Substantially the same result was derived by other enquirers. Astronomers were therefore satisfied, up to the year 1884, that the earth's axis of figure was so nearly coincident with its axis of rotation that the difference between the two was inappreciable to the most refined observations. All methods of observation and all principles of the reduction of observations, both of astronomers and of geodesists, were tacitly based upon the idea of absolute coincidence between the two axes.

In 1884 the subject was independently reopened by two men—Chandler in America, and Küstner at Bonn—and entirely fresh light was thrown upon it. Their work was simultaneous and quite independent. I will take Chandler's first.

In 1884–5 he took a thirteen month series of observations at Harvard with an instrument of his own devising to which I will revert later. These observations showed a progressive change in the derived latitude, which appeared to him of a greater magnitude than could be accounted for by any instrumental errors. He, however, hesitated to ascribe it to a real change in the latitude without further confirmatory observations, which he could not then make. He therefore put these observations aside and was, six years later, drawn to re-examine them by the publication of some of Küstner's results, which were also only explicable on the hypothesis of an actual variation in the latitude of the place of observation. It was, however, quite obvious to Chandler that his series of observations contained no warrant for an Eulerian period of ten months and he therefore, to quote his own words, "deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself, and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever." This bold rejection of theory and appeal to observation alone was rewarded with immediate success, and Chandler was able to show that his observations of 1884–5 contained unmistakable evidence of the rotation of the one pole about the other in a period of not 305 days, but 428 days. Wherein, then, lay the deficiency of Euler's investigation? As already hinted this arose from the assumption of rigidity and it was shown first by Newcomb and afterwards, more completely, by Hough, that the 428 day period was fully in accord with a degree of elastic yielding of the earth quite consonant with probability. Hough showed that if the earth were as rigid as steel the period would become 440 days; that the actual period is somewhat shorter than this means that the earth as a whole is decidedly more rigid than steel, a result which accords perfectly with other known phenomena which depend upon the earth's elasticity, such as the rate of propagation of earthquake waves.

Immediately following on this initial success Chandler undertook a prolonged and most laborious examination of old observations and reached results which have not completely borne the test of subsequent review. He was confident that the whole movement of the pole might be explained as the superposition of two rotations, one circular with a 428 day period and one elliptical with a period of a year. He thought, further, that there was evidence that the longer period had varied in past times and that in 1770 it was less than a year. This last result was traversed by Newcomb, who showed its extreme improbability. While fully bearing in mind the lessons of past experience as to the unwisdom of relying too closely upon pure theory, we cannot resist the conclusion that to accept any large change in the 428 day period within recent years would be to set aside the whole dynamical justification for accepting this period as a reality, it being quite impossible to admit that the elastic constants of the earth can be subject to any appreciable alteration within such time as a century or so.

As regards an annual period we should now prefer to say that, while there are doubtless seasonal transfers of material upon the earth, such as the accumulation and melting of arctic ice, which may produce a movement of the pole with an approach to a yearly periodicity, the part of the movement due to a true annual period is very small and is quite masked by large irregular disturbances. We shall be on safe ground if we say that the observed polar motion is compounded of a precessional rotation in a period of something very near 428 days at an average distance of 20 feet from the mean pole, with an irregular movement superimposed on it; this irregular movement having sometimes the effect of modifying the rate of precessional rotation and sometimes of changing its amplitude—that is to say altering the distance between the pole of rotation and the mean pole—according as it is acting parallel to, perpendicular to, or at any intermediate angle to the direction of the precessional rotation. I shall revert to this question later and show how it is possible by a simple graphical construction to separate out this irregular motion and construct a diagram of it which should be helpful in elucidating its cause.

While it is thus to Chandler that the credit of discovering the 428 day period should be ascribed it is to Küstner that we owe the first real proof that there is an actual variation in the latitude of a point upon the earth.

Küstner's observations were made in the same years as Chandler's, 1884–5, and were designed to determine the constant of aberration, a class of observation identical with those which would be used to determine the latitude of the place. Upon reducing these observations the results were at first sight anomalous in that they gave an impossibly small value of the aberration constant. The anomaly was not due to any instrumental cause; it could not be due to any seasonal change in the refraction, as the morning observations of 1884 were not accordant with the morning observations of 1885, nor could it be explained by any possible error in the proper motions of the stars. Küstner was thus enabled to state positively that the latitude of the place of observation had actually changed. It must be admitted that the years 1884–5 were particularly favourable ones, and that both these astronomers were in a sense lucky in having chanced upon them. The movement of the pole happened at that time to be exceptionally rapid. I do not, however, mention this as detracting in any way from the merit of their achievements; they deserve to be remembered as simultaneous but independent discoverers of this important and interesting phenomenon, and should be honoured, Chandler especially for his courageous rejection of mathematical theory, and Küstner for the very high skill and exquisite refinement of his observational work.

The importance of Küstner's discovery was at once recognized upon the Continent and a proposal was made to the International Geodetic Conference to establish a chain of stations for carrying on a series of simultaneous observations and thus deducing the true law of this latitude variation. This suggestion was soon carried into effect. Six stations were chosen, all at the same latitude, 39°·1 N.—Carloforte, in an island close to Sardinia; Mizusawa, in Japan; Gaithersburg in Maryland, and Ukiah in California; all new stations, where special observatories had been built for the purpose; a new observatory, established by the Russian Government at Tschardjui, in Russian Asia; and the existing observatory at Cincinnati. The reason for selecting stations at the same latitude was that identical sets of stars could be observed at each place, and thus any errors due to defective knowledge of star places are similar for all. These began work in 1899. Later, two stations in the Southern Hemisphere, at latitude 31°·5 S.—Bayswater in Western Australia, and d'Oncatwo in the Argentine—were added.

The results were reduced and discussed by Prof. Albrecht at the Geodetic Institute, Potsdam and published with a diagram showing the actual polar movement as deduced from the mean of the observations at all the stations, from time to time.

It was not long before these observations yielded a result of the highest interest. The observatory which devoted itself most wholeheartedly to the work and at which the observations were most extensive and most precise is that in Japan. This was under the able direction of Prof. Kimura. By a searching discussion of the whole series of observations he showed that they became far more consistent if a new term were introduced into the expression for the latitude variation, this term having an annual period but being independent of longitude and having the same value for all the stations at the same date.

It will be readily seen that this term differs completely from those we have been considering hitherto. It is not a shift of the earth's axis or a movement of the pole of rotation, as it affects all places along a parallel of latitude equally the pole evidently does not move, but something which has an effect exactly the same as if the centre of gravity of the earth were shifted a few feet up and down, northward and southward, from its mean position.

The great difficulty in elucidating the Kimura term lies in its extremely small magnitude and in the consideration that there are so many possible sources of error affecting observations of this class which might have annual periodicities that their separation and evaluation are extraordinarily complicated questions. This is not the place to attempt any complete discussion but a mention of some of the lines along which a solution has been sought may detain us for a few minutes.

The magnitude of the term at the latitude of 39° is about 6/100ths of a second or arc, or 6 feet on the earth's surface. It has the same value and phase for every station on the same parallel and is zero on about March 9 and September 12, and maximum and minimum on June 10 and December 10, i.e. about ten days before the equinoxes and solstices respectively. It cannot be accounted for as a real shift of the earth's centre of gravity. It is true that in the alternate melting and accumulation of ice and snow at the two poles we have a periodic factor at work which does do this, but the amount is far too small. It was pointed out long ago by Van de Sande Bakhuysen that to fit in with the observed value of this term the apparent path of the centre of gravity must have an amplitude of 3 metres, which, if translated into terms of polar ice, would mean that a cap of ice 1 kilometre thick and 244 square degrees in area would have to form and disappear each year. This is obviously quite impossible. There are certain possible errors in the accepted values of the proper motions and parallaxes of the fixed stars which might produce an apparent variation in the observed latitude of this nature. As all parallaxes are based upon differential measures we cannot with certainty say that such errors are impossible ; we can only say that they appear to us very unlikely, and that, if they were actually proved to exist our ideas of the stellar universe would be profoundly modified.

If there were a yearly term in the refraction which had the effect of a periodic change in the apparent zenith we should get a corresponding periodicity in the observations. If, for example, there were a solar atmosphere, even of a quite tenuous nature, which extended into space beyond the earth's orbit, we should get a seasonal change due to the varying angular distance of the sun from the zenith of the place of observation. An atmosphere which could bend rays of light to the requisite amount, though undoubtedly extremely rare, would, however, be dense enough to offer an amount of resistance to a planet, or *à fortiori* to a comet, inconsistent with observed facts. It is, however, quite possible that the changing declination of the sun may curve or tilt the mean isobaric surfaces in the upper atmosphere in such a way that the apparent zenith moves north and south about its mean value and that it is to this cause we owe the greater part, if not the whole, of the Kimura term. Such a displacement of the isobars is highly probable and the phase times of the latitude variation—nil at equinoxes, maximum northward at summer solstice, and maximum southward at winter solstice—fits in perfectly with this explanation. The observations made in the southern hemisphere should form a crucial test. If this is the true cause the apparent latitude of a southern observatory will be shifted in the same direction as that of its northern counterpart, i.e. northward in June and southward in December. We have only a short series of observations from southern stations, but as far as they go they appear to conform. There is thus fairly strong evidence in favour of this explanation.

It must not, however, be assumed that the matter is settled beyond dispute. More observations are necessary and especially observations at widely different latitudes. The international stations are, as to the northern ones, almost exactly on a parallel, and, as to the southern ones, on a parallel differing only by 7½ degrees from the northern. This uniformity, highly advantageous for securing a precise record of the motion of the earth's pole, is disadvantageous for solving the riddle of the Kimura variation, and other places should join in the attack. Unfortunately the observations are very laborious and require the almost exclusive attention of an observer. There is therefore a very real want of an instrument which shall demand something short of the whole time of a skilled astronomer. With this object, and also with the intention of eliminating certain sources of error, instruments of new form have been devised. A short account of these will be of interest.

I shall not here attempt any description of the methods of observation used. It will be sufficient to say that, as what we want to find is the direction of the zenith at the place, all methods ultimately depend either upon a level, giving us the horizontal plane, or upon a plumb line, giving us the vertical, and that of these two the level is the one that has almost exclusively been employed by the astronomer. The level is an instrument capable of a high degree of precision but it has the disadvantage of being very susceptible to temperature changes and, as both the glass tube of the level and the spirit with which it is filled are bad conductors of heat, it is impossible to ensure that it is at an even temperature throughout. Irregularities are thus produced which the reading of both ends of the bubble only partially eliminates. The mere fact of an observer standing near a sensitive level to read it may seriously vitiate its accuracy.

Some of these errors may be avoided, and such errors as are due to faulty reading of the level graduations by the observer entirely eliminated, by making the level an integral part of the instrument by floating the whole in liquid. The first application of this principle to an astronomical instrument was by Chandler, who carried out his series of latitude observations, already mentioned, with an almucantar, being a transit telescope floated in a trough of mercury. The name almucantar means a small circle of the heavens parallel to the horizon, and it will be sufficiently obvious that if the telescope can be set at any angle with the float, then as the instrument is rotated in the trough, or the whole trough itself is turned, the line of sight of the telescope will move round such a circle. With this instrument the stars are observed, not as in a transit circle crossing a vertical line, but crossing a horizontal circle of constant altitude. For convenience of calculation this horizontal circle would generally be selected as that through the celestial pole at the place. Chandler's instrument was purposely designed so as to differ as little as possible from the ordinary visual type, and must have been a most difficult instrument to use. The fact that he got such excellent results from it is no small tribute to his manipulative skill. The use of this form of instrument cannot be said to have found great favour among astronomers; there is only one example of it in this country, and, as far as I know, none on the Continent. The one we have is at the Durham University Observatory, and was designed by the present

Astronomer-Royal for Scotland, in co-operation with the late Dr. Common. It marked a very decided advance upon the earlier type. In two points specially, the screening of the floating part from wind disturbance, and the attachment of the eye-piece to the fixed part, the designers had the idea of a movable instrument, which a slight touch or a puff of wind would set vibrating to such an extent that no observation would be possible for a minute or two, clearly before them. The almucantar method of observation, meaning by this not the use of a floating type of instrument, but the observing of stars crossing a horizontal circle, though appropriate for the particular class of observation we are here concerned with, those for determination of latitude, is not absolutely the best that can be used. To reduce every possible source of error to a minimum, particularly those due to refraction of the atmosphere, we want to observe stars as near the zenith as possible.

The floating principle has been applied with great success to a zenith instrument in the Cookson floating zenith telescope now at Greenwich, designed by the late Bryan Cookson, whose early death was a great loss to astronomy.

It is a photographic instrument, with a telescope or camera tube attached to a circular float which floats in a ring-shaped trough of mercury. The angle between telescope and float can be altered, so that it can be clamped to point either vertically upwards or at any angle, up to about 30°, from the vertical. It is used in the well-known Talcott method. A pair of stars is selected which cross the meridian within a few minutes of each other at nearly the same zenith distance, one north and one south of the zenith. The instrument is set so as to include the first star in the field, the lens is opened, and as the image of the star moves across the plate it traces a fine line or trail. After the star has crossed the meridian, the telescope is turned through 180°, leaving tube and float clamped in the same relative position, and the second star traces out its trail. The distance between the two trails on the plate, which is small if the difference of their zenith distances is small, when the appropriate corrections are applied, gives the observed difference of zenith distance of the two stars, and, therefore, the observed position of the zenith, and hence the latitude of the observer. By repeating the observation with a number of pairs of stars a very precise determination of the latitude is made.

Recently a zenith telescope, designed not on the floating but on the hanging principle, finding the vertical line by virtue of its free suspension in a gimbal ring, has been constructed, and would have been at work by now had it not been for the interruption caused by the war. Though it has thus not yet been tested by practical experience, a few words on it may not be out of place. The method of observation will be the same as I have just described, except that there is no arrangement for clamping the instrument at an inclination to the vertical; it is intended to be used only in the vertical position, and the angle covered by the photographic plate will be a few degrees from the zenith on each side. Exactly how far we can go from the zenith depends upon the qualities of the lens, and no confident statement can be made until this has been tested, but it is hoped that star trails perfectly sharp for measurement will be secured up to an angular distance of 3° from the centre. This gives us as available for our purpose the stars over a belt 6° wide down to the sixth and possibly the seventh magnitude. The actual work of observing will be very simple, and will only mean that the whole instrument is rotated through 180° at certain prearranged times, and that the lens is opened after twilight and covered before the dawn. It would be possible for this to be done by mechanism controlled by a clock.

As the telescope hangs freely always in a vertical position, we entirely get rid of one of the astronomer's anxieties, the risk of error due to flexure or bending of his telescope, for though the tube can be made apparently very rigid, the excessively minute degree of bending sufficient to introduce appreciable errors is difficult, if not impossible, to avoid in a telescope which has to be used in different positions. Then, again, the errors due to changes of temperature inside or close to the instrument should almost disappear in this form. Firstly, no temperature changes affect the suspension; so long as the body of the telescope remains undistorted the position of the true vertical in regard to the optical axis remains constant. Secondly, as the whole hanging part of the instrument is perfectly symmetrical about the vertical axis, with the trifling exception that the plate-carrier and photographic plate are not circular but rectangular, no temperature change should distort the axis. Any distortion that can take place will, in fact, be the very small change of scale that will result from the difference in the expansion of the glass plate and the brass tube. Thirdly, it is possible, and in this instrument has been done, to enclose the whole in an outer case which can be made air-tight and kept at a constant temperature by a thermostat. In order to close the instrument in front it is necessary to have a plane parallel glass of slightly larger aperture than the lens. As this glass has to be worked with the same refinement as a lens, and as a plane surface is more troublesome to work than a curved one, this is rather a costly addition. Whether, as a matter of fact, it is worth while keeping the instrument at the same temperature, or whether it will be better to reduce the temperature change to a minimum by covering the whole with non-conducting material, and then apply the very small corrections necessary to the measurements made on the plate, is a question for experience to decide.

As a heavy hanging mass would be liable to long-continued vibrations when disturbed, a four-armed vane attached to a rod at the base is immersed in a dash-pot or bath of glycerine. This rod must be centred in prolongation of the vertical axis, otherwise the capillarity between rod and liquid will introduce a force deflecting the telescope from the true vertical. While it would thus appear that in this form of instrument most of the familiar sources of error are minimized, it is interesting to note the introduction of one possible cause of error, quite unfamiliar to astronomers, namely, the deflection that might be due to the attraction of the earth's horizontal magnetic force upon the hanging part. If the telescope-tube were, as is customary, made of iron or steel, this would reach a serious magnitude, and even if a proportion only of the suspended weight were of iron a perceptible deviation might result. It would, in fact, not be safe to allow this proportion to exceed one-tenth of the whole weight and it therefore seemed better to exclude the use of iron or steel altogether. There is accordingly none, with the exception of the four thin flat pendulum springs which form the gimbal suspension.

In detaining you with these short descriptions of recently devised instruments I may appear to have been wandering rather far from my subject, the wanderings of the earth's pole. You will, however, appreciate that in reality they follow very closely from it, being instruments designed with the special object of solving the particular problem we are discussing.

We will now revert to the diagram of the observed polar motion, and I will indicate how it is possible to analyse this so as to separate the irregular movements from the more orderly fourteen-month precessional rotation. We are justified in assuming that this free precessional period is constant in duration and therefore determines the average rate of rotation of the pole of revolution. If, therefore, we take a diagram of the polar movement, which will naturally have its axes of reference fixed in relation to the earth, and convert it into another diagram, showing the same movement, referred to axes rotating in the earth at the average rate of the precessional rotation, we obtain a graph of the irregular part of the polar path. If this irregular part has any well-marked annual period, such period ought to be apparent on inspection of the converted diagram. In the actual diagrams obtained there seems little or no evidence of the existence of a yearly term.

We now take this second diagram and by the well-known process construct its hodograph, the curve which gives us a measure of the amount and direction of the force which could have caused the movement recorded in diagram number two. This will still be referred to the moving axes, so is not directly available for deducing the true direction of these forces in the earth. Before we can do this we must refer the diagram back again to axes fixed in the earth. Thus, finally, we obtain our diagram number four, which may be called the torque diagram, as it represents in direction and relative magnitude the torque or twisting force which has been acting upon the earth to produce the observed movement of the pole.

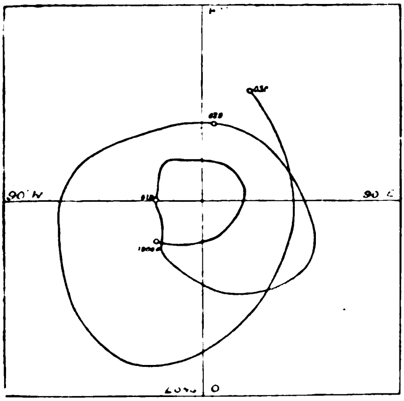

Fig. 1.

Track of polar movement, 1900–1903.

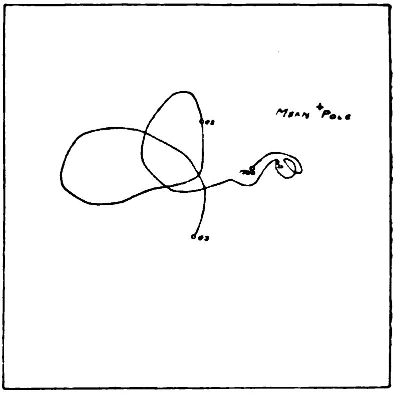

Fig. 2.

Same track referred to axis rotating in the earth with a 14 month period.

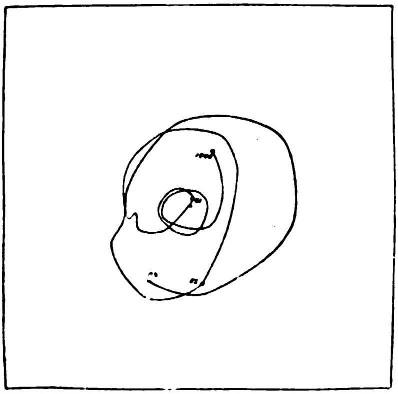

Fig. 3.

Hodograph of Fig. 2.

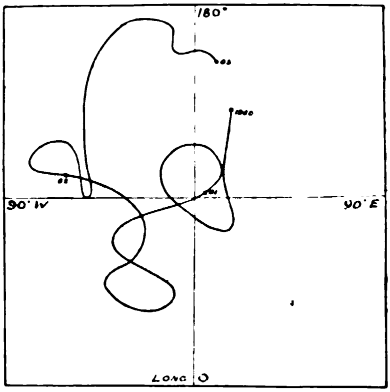

Fig. 4.

Hodograph referred back to axes fixed in the earth, or Torque Diagram.

The interpretation of such a diagram is a somewhat complex matter and has not yet advanced far. The causes that seem to be at work producing the irregular shift are either movements of the earth's crust, slow or rapid as in an earthquake; the transfer of arctic ice from one point to another or its accumulation and disappearance so far as this takes place unsymmetrically with respect to the earth's axis; and possibly extensive barometric changes extending over considerable areas.

Of these the transfer of ice is the largest factor and is probably the one to which most of the irregular polar movement may be ascribed. An earthquake, even of gigantic dimensions, would have an almost negligible effect. The late Professor Milne estimated that a very large earthquake might displace ten million cubic miles of earth through a distance of ten feet horizontally or vertically. Such a vast cataclysm would only change the position of the pole by a few inches.

In conclusion it will be an act of natural curiosity to enquire whether there is any evidence of the amplitude of these polar wanderings having been greater in past times than at present, and whether there is any likelihood of their being greater in the future. To both these questions the answer is "No." The axis of rotation is always kept near the axis of figure by internal friction, and it would require a large change in the distribution of mass to move the axis of figure very far.

As regards the future the probabilities point still more strongly in the same direction. Each shrinkage of the earth, whatever its immediate effect on the position of the axis of rotation may be, tends ultimately to bring it nearer to the axis of figure or principal axis of inertia, and therefore tends to reduce the average amplitude of the polar path. The distance of the pole of rotation from the mean pole will therefore gradually decrease as the world grows older, while at the same time as the earth cools and becomes less elastic and more rigid the rate of rotation will quicken.

[E. H. H.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

John Quiller Rowett

was elected a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM
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Botanic Society, Royal—Botanical Journal, Vol. V. No. 1. 8vo. 1916.
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Dax, Société de Borda—Bulletin, 1915, Nos. 2–4. 8vo. 1915.
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  Amateur Photographer for May 1916. 4to.
  American Journal of Science for May 1916. 8vo.
  Athenæum for May 1916. 4to.
  Author for May 1916. 8vo.
  Chemical News for May 1916. 4to.
  Chemist and Druggist for May 1916. 8vo.
  Church Gazette for May 1916. 8vo.
  Concrete for May 1916. 8vo.
  Dyer and Calico Printer for May 1916. 4to.
  Electrical Engineering for May 1916. 4to.
  Electrical Industries for May 1916. 4to.
  Electrical Times for May 1916. 4to.
  Electricity for May 1916. 8vo.
  Engineer for May 1916. fol.
  Engineering for May 1916. fol.
  Ferro-Concrete for May 1916. 8vo.
  General Electric Review for May 1916. 8vo.
  Horological Journal for May 1916. 8vo.
  Illuminating Engineer for May 1916. 8vo.
  Journal of Physical Chemistry for May 1916. 8vo.
  Journal of the British Dental Association for May 1916. 8vo.
  Junior Mechanics for May 1916. 8vo.
  Law Journal for May 1916. 8vo.
  Marine Engineer for May 1916. 8vo.
  Model Engineer for May 1916. 8vo.
  Musical Times for May 1916. 8vo.
  Nature for May 1916. 4to.
  New Church Magazine for May 1916. 8vo.
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  Power for May 1916. 8vo.
  Power-User for May 1916. 8vo.
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  Tchèque, La Nation, for May 1916. 8vo.
  War and Peace for May 1916. 8vo.
  Wireless World for May 1916. 8vo.
  Zoophilist for May 1916. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIV. No. 259. 8vo. 1916.
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  Weekly Weather Reports for May 1916. 4to.
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New York, Society for Experimental Biology—Proceedings, Vol. XIII. No. 6. 8vo. 1916.
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Royal Colonial Institute—United Empire, Vol. VII. No. 5. 8vo. 1916.
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Sarat Chandra Das, C.I.E. (the Author)—An Introduction to the Grammar of the Tibetan Language. 4to. 1915.
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Smithsonian Institution—Miscellaneous Collections, Vol. LVI. No. 2. 8vo. 1916.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. V. Disp. 4*a*. 4to. 1916.
Statistical Society, Royal—Journal, Vol. LXXIX. Part 2. 8vo. 1916.
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United States Department of Agriculture—Journal of Agricultural Research, Vol. V. No. 26; Vol. VI. Nos. 4-7. 1916. 8vo.
Experiment Station Record, Vol. XXXIV. Nos 5-6. 8vo. 1916.
United States Department of Commerce and Labour—Observations made at the U.S. Coast and Geodetic Survey at Cheltenham, Maryland, 1913-14. 4to 1915.
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Washington, National Academy of Sciences—Proceedings, Vol. II. No. 5. 1916.
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WEEKLY EVENING MEETING,

Friday, June 9, 1916.

The Hon. Richard Clere Parsons, M.A. M.Inst.C.E. M.I.Mech.E.,
Vice-President, in the Chair.

Ernest Clarke, M.D. F.R.C.S.

Eyesight and the War.

When the War is over the Dutch will be celebrating at Leyden or Utrecht the centenary of the birth of Donders, the great Dutch ophthalmologist, and as our subject this evening is mostly concerned with refraction and with Donders' great work, I thought it fitting to remind you of this. If a man's fame and the endurance of that fame are judged by the amount of good he has done to his fellow-creatures, then I consider that no one has a bigger claim than this grand old Hollander. In 1864 he published in English—it was never published in Dutch—his work on the Accommodation and Refraction of the Eye. That work soon became—and it has remained up to the present day—a classic. To celebrate the fiftieth anniversary of the appearance of this work, I brought out two years ago a small book, in which I set forth the work that Donders had done and indicated the progress made since, and as a small return compliment I published that first in Dutch. It was dedicated to the Dutch Ophthalmologists, and was very well received by them, but unfortunately the War intervened, and all communications were stopped.

Where Donders found chaos and ignorance he left order and knowledge, and so perfect was the foundation he laid that not one of his important statements has been upset even at the present day. On that perfect foundation we who have followed him have been able to raise a superstructure of which I am perfectly certain he would have approved.

One of the important subjects that Donders threw more light upon was that of Myopia, or short-sight. He showed that it was the over-developed eye, or too long eye, and that it was the opposite of the Hypermetropic or under-developed eye. [Diagrams were here shown of the Emmetropic, or normal eye, and the Hypermetropic and Myopic eye, and the manner in which parallel rays of light are received on their respective retinæ.] You will see from the diagrams that in the normal eye, the emmetropic eye, parallel rays focus, by means of the dioptric apparatus in the eye, on to the retina. In the hypermetropic eye they focus behind the retina. The hypermetropic eye has the power, by the focussing arrangements in the eye, of making the rays converge on the retina; or, what is better still, these parallel rays may be made to converge and focus on to the back of the eye by a suitable convex glass. In the myopic. or short-sighted eye, the parallel rays are focussed in front of the retina, and when they meet the retina they are out of focus. The myope has no power of focussing as the hypermetrope has, and only rays coming from a near point in front of the eye, that is, divergent rays, or parallel rays made divergent with a concave glass, focus on the back of the eye.

Now myopia has been the *bête noire* of the War Office for a great many years. Thousands of eligible young men, keen on getting a commission, have been refused because of myopia. We hear constantly the same tale—a well-set-up young man, with muscles of iron, keen and intelligent, but a myope, and lost to the country as a good officer. Why this fear of myopia? It is bound up with another question—Why has the War Office always set its face against the wearing of glasses? The answer is not far to seek. Hitherto our small army was chiefly employed abroad, and mostly in the tropics, and it was difficult to replace lost or broken glasses. That has all changed now. There was a very good reason for fearing myopia in the old days, but there is no fear now. The reason was that when myopia was not properly treated it tended to progress. The tunics of the eye stretched more and more, and so much damage was done to the back of the eye, either by rupture or atrophy of the retina, or hæmorrhage, that the person often became blind. [Lantern slides were here exhibited, showing the changes occurring in high myopia.]

Apart from the changes at the back of the eye, we may get rupture and detachment of the retina. In high myopia the vitreous becomes liquefied and fails to support the retina sufficiently, some jar takes place through taking a false step, jumping, etc., and a small rent takes place in the retina, the fluid vitreous then passes behind it and separates it from the choroid, and the person becomes blind.

The cause of progressive myopia is undue or excessive convergence. The old treatment for myopia was to give glasses for distance, but not for near work because the individual could see well without them. The result was that the individual had to come nearer his work than was necessary. This extra pull on the internal recti muscles caused the eye to be stretched antero-posteriorily. This meant more short-sight, which meant bringing the work nearer still, and so a vicious circle was introduced. This shows how important it is even in the nursery to prevent undue convergence. The soft eye of the child is especially prone to the ill-effects of this undue convergence, and such amusements as the threading of beads or fine sewing should be strictly forbidden, especially if the children have a history of myopia, that is, if one or both parents are myopic.

The treatment is very simple. The eyes are thoroughly tested under some form of atropine if the patient is under forty or forty-five, all the errors are corrected completely, and the glasses are given to be worn *always*. It is very difficult to get young people to do this, as they can see so well without glasses for near work, but it is most imperative that it should be insisted upon; thus, not only is the work put further back from the eye and undue convergence prevented, but the muscle of accommodation is brought into play. It is not only the excessive convergence which is bad, and which causes increased myopia, but it is the excess *of* convergence over accommodation which does harm.

Some years ago I collected statistics of 532 myopes that I had fully corrected and watched for a period of five years. Out of those 532 myopes only four progressed to any extent. We now know that myopia is not a serious disease if the person is allowed to wear glasses.

This brings us to the main question, Why should there be any difficulty about glasses being worn in the Army? The whole subject of refraction has so developed during the last fifty years, mainly, as I have previously pointed out, through the work of Donders, that we are in quite a different position to-day from that which existed even twenty years ago. So many men are now giving up their time entirely to eye-work that there is no difficulty in getting young oculists to form part of the Army equipment. Each centre can have its oculist, just as it has its surgeon, its chaplain, or its veterinary surgeon, and under that oculist there can be one or more working opticians, who would keep a register of all the glasses worn by the men in that centre. Under these circumstances the reasons against the wearing of glasses disappear, as a lost or broken spectacle can be replaced in an hour or two, and there is no reason to fear any want of proper working in the Army through the absence of glasses. At the present time the War Office has made certain concessions. They have appointed oculists at different centres, and also working opticians, but we want to see the scheme adopted in its entirety. We want to do away with our present bad plan of judging vision by uncorrected sight instead of by corrected sight, as happens abroad. In Great Britain vision *without* glasses counts, whereas in all the Armies abroad vision WITH glasses counts. Seven dioptres of myopia are allowed in France and Italy, six in Austria, and 6·5 in Germany. Our limit is about 2·5 of myopia, and the minimum uncorrected sight allowed is $\frac{6}{24}$ in the right eye and $\frac{1}{10}$ in the left eye, which is the last concession made.

[Snellen's Sight Test was here explained.]

TABLE OF ARMY SIGHT TEST.

TABLE SHOWING THE VISUAL STANDARDS FOR RECRUITS IN THE CHIEF EUROPEAN ARMIES. (PATERSON AND TRAQUAIR.)

—	Amount of Short Sight (Myopia) allowed.		Standard of Corrected Vision.		Remarks.
	Combatants.	Non-combatants.	Combatants.	Non-combatants.	
GERMANY	6·5 D. For Landsturm no limit if standard of corrected vision attained.	—	1/2 in better eye. Other eye may have minimal vision. For Landsturm vision = 1/4. If one eye has vision = 1/2 the other may be blind.	—	Vision with glasses (corrected vision) counts.
AUSTRIA	6 D.	Above 6 D. no limit if standard of corrected vision is attained.	Group 1, 1/2 in each eye. Group 2, 1/2 in one; 1/4 in other.	1/4 in one; 1/10 in the other.	Vision with glasses counts.
FRANCE	7 D.	Above 7 D. no limit if standard of corrected vision attained.	1/2 in one eye; 1/20 in the other.	1/4 in one eye; 1/20 in the other.	Vision with glasses counts.
ITALY	7 D.	—	1/3 in each eye, or 1/12 in one eye if the other has 1/1 (full vision).		Vision with glasses counts.
GREAT BRITAIN	No amount specified, but according to vision required highest amount possible is about 2·5 D.	No amount specified, but according to vision required highest amount possible is about 2·5 D. in better eye and 3·5 D. in worse eye.	No correction allowed for general service. Uncorrected vision must be 1/4 in each eye, or 1/4 in the right eye with 1/10 in the left.	Uncorrected vision must be 1/4 in better eye, 1/10 in worse eye. The better eye may be the left.	Vision without glasses counts. For home service, garrison service, and garrison service abroad, glasses are allowed within unspecified limits.

I have just received from the War Office their last concession, and they say that if an individual can read $D = 24$ at 20 ft., or 6 metres, without glasses with the right eye, and not less than $D = 60$ with the left eye without glasses, he will be considered fit.

To show how effete this old system is, I will give you an example illustrating how people who get into the Army may be far worse from the eyesight point of view than those who are prevented from getting in.

Let us assume that two men, aged twenty, are applying for commissions. One has a myopia of 6 D, and the other hypermetropia of 6 D. If you look at Diagram I. you will see that at the age of twenty the ordinary individual has 10 dioptres of accommodative power, so that if he has a hypermetropia of 6 D he has 4 left for near work, so that he practically "romps" in. The myope has no chance of getting in, but we will assume for the moment that he gets in by dodging his examiner.

Assume now that these men are seen fifteen years later on the field of battle, and they have both lost their glasses. A written despatch from the General is handed to the + 6 man; by that time he has lost so much of his accommodative power that all that is left him he has to use up to correct his defect, and *cannot read without glasses*; the – 6 man has to read it, and can read it; and he will be able to read without glasses when he is a hundred! Which of the two is the more useful man now? Surely the myope who ought not to have been admitted under our present regulations. I think you could not have a better illustration of the reason for getting rid of this effete system.

What I have previously said about officers applies equally to the ordinary soldier. If there were any objection to a spectacled man being in the front firing line, this could easily be avoided by placing him in some department where glasses would be no disability. But what we want to see is that in the future, not only for this War but for all time, glasses will no longer be considered a disability in the English Army. In the Navy you can quite understand that, owing to weather conditions, glasses cannot be allowed. Therefore, it would be wise to advise those who have specially good sight to enter the senior Service.

I have referred under this heading to myopia because that is the commonest cause which keeps men from getting into the Army; but high hypermetropia, or high astigmatism, both of which are curable by glasses, keep many men out of the Army under the old system. At the beginning of the War I saw a young man who was in splendid condition, but he had high astigmatism. In his particular case he saw quite well when he looked through a horizontal slit, and he could read $\frac{6}{12}$ with the eyes half shut, but with the eyes open he could not read $\frac{6}{60}$. By personal influence we got that man into the Army, and I am pleased to say that he has since received the D.S.O.

We now pass to the important subject of eyestrain as it affects our soldiers.

We have seen that the eye may vary from the normal by being too long or too short, and combined with these defects we may have another defect, viz. astigmatism. This error lies chiefly in the cornea, and is due to its surface not being spherical. In regular astigmatism any one curve of the cornea is different from any other curve, the maximum difference being between two curves at right angles to each other. Each curve has its separate focal point, so that rays of light passing through a regular astigmatic surface focus in a line of points instead of one point.

2500 Individuals whose sight after correction was normal and who had no disease of the eyes.	(1) Same refraction in both eyes (657)	(*a*) Emmetropia (see Presbyopia below)	9
		(*b*) Hypermetropia	63
		(*c*) Myopia	22
		(*d*) Astigmatism—	
		Hypermetropic	438
		Myopic	113
		Mixed	12
	(2) Refraction different in the two eyes (Anisometropia)		1843
			2500

5000 eyes (as above)—

Emmetropia	56
Hypermetropia	425
Myopia	216
Astigmatism	4303
	5000

Of the 2500 individuals, 961 were presbyopic, and only 9 of these were emmetropic.

By including low errors of astigmatism (which we now know are the most important) we find that this is a much more common defect in the eye than people imagine. In the 5000 eyes of the 2500 individuals that I carefully examined, you will see by the above table that no less than 4303 were astigmatic. That, probably, is a higher average than would have been the case if those 2500 individuals were taken promiscuously. They were all people that were sent to me because there was a suspicion about their eyes. But it is not a very great exaggeration, because I include in that low errors of astigmatism which we shall soon see are most important.

Regular astigmatism is cured by cylindrical glasses. When rays pass through a cylinder they are not refracted along the line of the axis of the cylinder, but are more and more refracted as they pass from the axis, the maximum refraction being at right angles to the axis. A convex cylindrical lens is made, as it were, from a solid cylinder of glass, and the concave cylinder may be considered to be the mould of the convex cylinder.

The vision of an astigmatic person who has a gross error is not like the vision of a myope. For instance, the myope might not see even $\frac{6}{60}$, but a person suffering from gross astigmatism might see even some letters in the normal line, but in a "fuzzy" manner. [A demonstration was then given of the vision of an astigmatic person and the effect of correcting it by a cylinder.] If the cylinder is turned a little out of the true the picture at once becomes indistinct, hence the importance of making sure that the axis of the cylinder with which you are correcting the astigmatism is absolutely true. These gross errors are very simple; they take care of themselves, and go to the oculist or the optician to be corrected.

But the real importance of this subject is only revealed when we come to discuss the effect of small errors. The possessor of this form of astigmatism is usually quite unconscious of it, and when an up-to-date physician suggests that the headaches or neuralgia, or some other functional neurosis, is due to the eyes, they are often indignant, declaring the vision to be perfect, which apparently it is. The fact is the ciliary muscle is correcting the defect, if, for instance, the horizontal curve of the cornea is flatter than the vertical, the ciliary muscle can contract irregularly and cause the vertical curve of the lens to become flatter and so correct the astigmatism. It is only very small errors that can be corrected in this way. The individual is not conscious of this work done, *but* the nervous system *is*.

Associated with this is another cause of eyestrain, viz. anisometropia, "odd" sight. That, as you see in the diagram, is much more prevalent than people would imagine. Out of these 2500 individuals no less than 1843 had "odd" sight. If the amount of difference between the two eyes is very small, i.e. 0·25 D or 0·5 D, impulses can pass down from the brain to the ciliary muscle, and the anisometropia is unconsciously corrected.

The third cause of eyestrain is muscle-strain. We who have perfect sight, and see only one object when we look at it, are unconscious of the fact that we really see two, and that by the wonderful balance of our muscles those two pictures are merged together to form one. If there is a small want of balance between the external muscles of the eye, impulses pass down from the brain to one muscle, and the balance is preserved and diplopia prevented, but at the expense of nerve energy.

Those three forms of eyestrain may co-exist, and you can easily imagine the enormous waste of nerve energy going on all the waking hours, caused by the constant passage of nerve impulses to the muscles from the brain. How insidious this is we realize when we know that sometimes there is *no* symptom to point the danger.

I do not think there is a single *functional* nerve trouble that may not be due to, or seriously aggravated by, eyestrain caused by the presence of low errors of astigmatism, by low anisometropia, or want of balance of the external muscles. I will show you how this waste of nerve energy affects the soldier.

You have heard a great deal recently of the effect of high explosives. The wind pressure in high explosives is enormous; so enormous is it that I had one case of an officer whose eye was absolutely smashed, full of blood, and the whole of the retina detached, although no part of the bomb touched it—simply by the wind pressure. On the other hand, by the swing of the pendulum we get a high vacuum. In the September air raid a high explosive fell in St. Bartholomew's Close, causing enormous destruction. Close by is the big hall of St. Bartholomew's Hospital, which contains a large number of stained-glass windows, all of which were closed; many of those windows were blown *out* by the explosion, showing that at some time the pressure inside the building was higher than the pressure outside, and that happened in some other places in London. That was exemplified in a case I had of a man wounded at Festubert. His two companions on the left were killed, and he felt, on putting his hand up to his eye, that it had been injured. He walked back a mile and half to the dressing-station, where a pad and a bandage were put on. He was sent home, and came under my care at the King George Hospital. He was put on the operating-table and chloroformed, with the idea of removing the remains of the damaged eye, but to my surprise, on clearing away the blood-clot, I found no eye there at all—simply the ragged ends of torn muscles. They had not been cut, but torn. That eye had been pulled out on the battlefield by the vacuum caused by the high explosive. If such enormous effects take place, you can easily imagine the harmful effect on the men who are in the neighbourhood. There may be no external injury at all, but they suffer as they would in a really bad accident, such as a railway collision; "virtue" has been knocked out of them; an enormous amount of nerve energy has leaked out.

We have seen that eyestrain causes leakage of nerve energy. We want in these men to save every bit in the "store-house" that is left to them, and therefore if eyestrain is present we want to correct it. We are very fortunate at the King George Hospital in having as our Resident Medical Officer in the eye wards Dr. Harwood, who is keenly alive to the importance of eyestrain and the effects of low errors of astigmatism, and wonderful and miraculous have been some of the cures he has made with simply a pair of glasses. One case—perhaps the most marked—I cannot refrain from alluding to: the man, a sergeant-major, aged thirty-eight, was hit by a wet sand-bag falling on him from a height of eight feet while he was lying in his dug-out in Gallipoli, on November 24, 1915. He was not rendered unconscious, but found he could not walk, and was very giddy. He was put on a battleship, and became gradually worse. He was taken first to Lemnos, and on January 3 of this year was admitted into the King George Hospital. He suffered from constant headache, could not stand, had a marked squint, could not read, and could not face the light. His mentality was normal but slow, and his memory sluggish. No lesion was discovered, and no treatment did any good until he was placed under atropine by Dr. Harwood, when he at once improved. The atropine paralyzed the focussing muscle, and therefore he could not correct his defect. While he was under the atropine he was thoroughly tested, and finally given glasses correcting a very small astigmatic error, 0·37 in one eye, and 0·25 in the other. He could read comfortably a few hours after getting the glasses, and within a week he could walk, and his headaches and giddiness had disappeared. It simply meant that the patient had had nerve energy knocked out of him, and that, when the glasses were put on, the tap of further leakage was turned off, and he was able to use his nerve energy.

Neurasthenia which follows head injuries is very commonly due to eyestrain, and a large number of cases have been cured by putting them into proper glasses. Unfortunately sometimes the injuries in these cases are so bad and the men are so ill that they cannot leave their beds, and to do the work of testing them properly they must come into our dark room and go through a thorough testing. Dr. Harwood has obtained wonderfully good results in a large number of cases simply by bandaging up one eye. Where there is a want of balance only one eye is left, and no attempt is made to send out impulses to the other eye. In some cases where there was astigmatism present as well he bandaged up both eyes for some time in the day, and a very large amount of good was obtained.

Again, a large number of the soldiers in our wards have bad open wounds, and sometimes these remain absolutely stationary; they do not seem to make any headway at all. The men read a great deal—some of them read more than they have ever read in their lives before—and if eyestrain is present you can imagine they are using up nerve energy that is wanted for the repairing process. By putting them into proper glasses an improvement at once occurred.

From the foregoing remarks you can easily imagine how very careful we have to be in doing this testing. It is better to give no glasses at all than to give a glass that is *almost* right, because the eye can supply the deficiency. The eyes of all these men are put under atropine or homatropine, because all the men are under forty, and then our most useful servant is the ophthalmometer, or astigmometer. This instrument has passed through many improvements since it was first devised by Javal of Paris, and the perfected model made by Meyrowitz of New York is now before you. [The Ophthalmometer (and its working) was here explained.] This instrument records an astigmatism as low as 0·12 D and an obliquity of even 2°.

One word before I leave this important subject, which takes us back to the question of wearing glasses in the Army. The glasses must be worn *always*. You can quite understand that if you wear glasses at one time and not at another, the muscle being probably in a state of spasm does not yield to the glass, and you do not get the result you want. The important result is that, in young people, by wearing the correction, the error tends to disappear.

I think you would like to know how in every department at the King George Hospital the soldier is considered. Everything is up-to-date, and whether in the X-ray department, or the dental rooms or the operating theatre, you will find the "last word" in appliances and instruments. In my own department—the eye department—the greatest care is taken in testing the sight. The very best possible glasses, supplied by Messrs. Hamblin, are supplied to the patients, and Messrs. Hamblin generously charge us for this work less than cost price. Moreover, the fitting of the glasses is not left to chance, for properly qualified opticians attend at the hospital and take the most careful measurements. These opticians measure and fit every glass, so that the patients get the fullest value. We have heard so much during the War of mistakes and abuses that I think it is only fair, and certainly it is most pleasing, to be able to concentrate our attention for a short time on a different picture.

So far we have been reviewing the effects of the War on combatants; we now turn to the effects on those of us who are disqualified by age to take an active part.

We have been considering defects of the eyes due to their shape, and we have seen how prevalent these defects are, yet some eyes (it is true, very few) are normal. Now, there is a defect that attacks all eyes if the individual lives long enough, viz. presbyopia, or old sight.

It may not be manifest, and the individual may be unconscious of it; but nevertheless, as we shall see, no eye after about the age of forty-five escapes it. It is a senile change, and is, as Donders observed, no more a disease than is grey hair.

At the beginning of life the crystalline lens, which by focussing rays of light on the retina gives us our distinct vision, is nothing more than a little bag of semi-fluid jelly. By making this lens thicker or thinner we can focus for near or distant objects. This work is done by the ciliary muscle, and chiefly by that portion of the muscle which surrounds the lens pressing on it in the form of a sphincter. The old theory (Helmholtz's) of the mechanism of accommodation was that the lens *at rest* was compressed by its capsule, and that when the ciliary muscle acted it pulled on this capsule and released it, thereby allowing the lens to assume a more convex form. The new theory, advanced by Tscherning, and supported entirely by clinical proof, is that the lens at rest is unacted upon, but that when the ciliary muscle acts it squeezes the periphery and causes the centre to bulge, and so the increased convexity is limited to the centre, and the aberration that would result is prevented by the contraction of the pupil that always normally accompanies the act of accommodation, just as in photography we stop down a lens when focussing for near objects. I have known this "squeezability" of the lens (if I may use the term) in a child so great as to amount to 20 D. It gradually decreases with age. At the age of ten the average accommodative power is about 14 or 15 D; whereas, at the age of thirty, it has been reduced to 7 D, i.e. half (see Fig. 1). This loss of elasticity is due to the sclerosing or hardening process which takes place in the lens, as in all the tissues of the body, as age advances, but it is not recognized by the individual until middle life, in the normal about the age of forty-five or forty-eight, and then the accommodation has sufficiently failed to necessitate the use of convex glasses for near work, and this, mark you, not through any loss of muscle-power, but through the hardening of the lens.

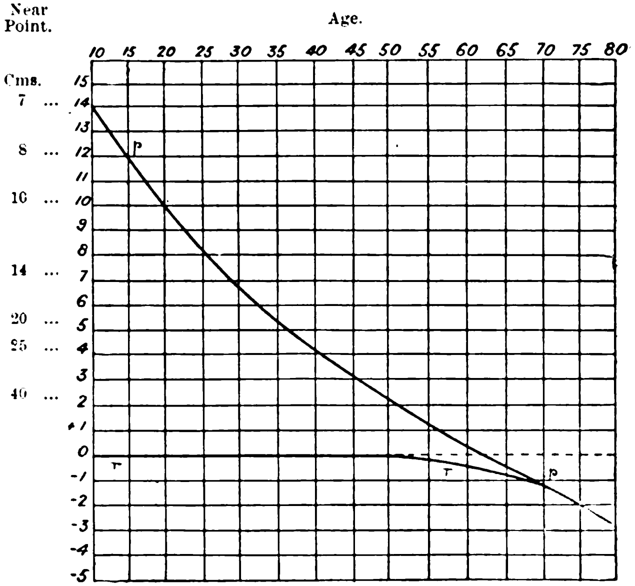

Fig. 1.

Perhaps Donders' best work was on the subject of presbyopia. His well-known diagram (Fig. 1), showing the loss of accommodation due to age, was made from 150 patients, and amongst these he owns that he probably included some with latent hypermetropia.

The following diagram was made by me from 1200 patients, all of whom were rendered normal before their accommodation was taken :—

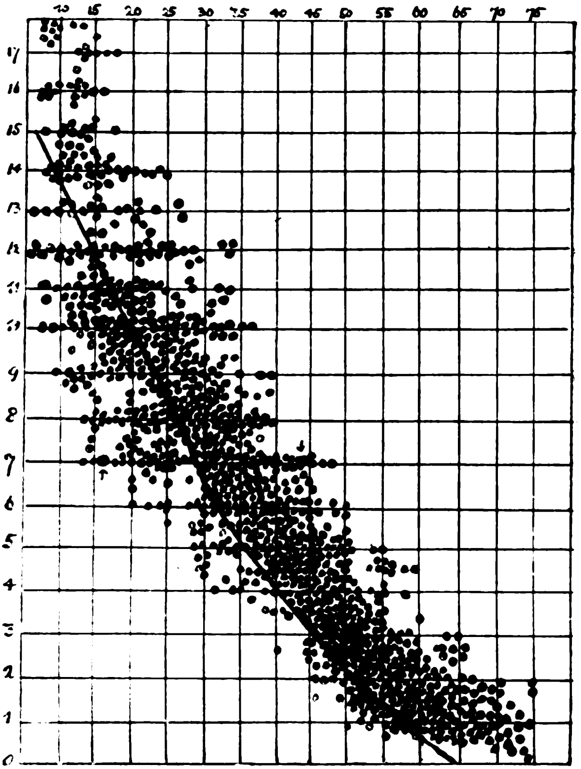

Fig. 2.

The line on the diagram is Donders' mean line, and it is seen to coincide from the age of thirty with what would be my minimum line, my mean line being considerably above it, showing that age for age the individual has more accommodative power than Donders allowed.

The following table shows the minimum, mean, and maximum accommodative power for different ages, deduced from my diagrams :—

Age.	Minimum.	Mean.	Maximum.
7-10	9	14	18
10-15	7	12	18
20	6	10	14
25	5·5	9	13·5
30	4·5	7·5	12
35	4	6·5	10
40	2·5	5·5	8·5
45	2	4	7
50	1	3	6
55	0·75	2	5
60	0·50	1·75	4
65	0·50	1·5	3
70	0·00	1	2

It must be borne in mind that although this hardening process affects all lenses, no matter what the refraction may be, it is naturally not manifested in all alike; for instance, the myope of 5 D whose far point is 20 cms. from the eye will be able to read small print to the end of life without glasses, whereas the hypermetrope of 5 D when he reaches the age of thirty-five has to use up all his accommodative power to correct his defect, and so must have convex glasses for near work.

We see, then, that in the normal individual, or those made normal by the correction of the refractive error, the average age for the onset of presbyopia is about forty-eight, and yet if we look at the table (Fig. 2) we see an enormous variation from this average. Take the line of *age fifty:* we see one individual has 6 D accommodative power, whereas another has only 1 D. What is the cause of this variation?

Sir William Arbuthnot Lane has shown that one of the commonest causes of premature senility is intestinal toxæmia, and I have shown elsewhere that the lens is perhaps *the most delicate index* of this degeneration.

In making a large number of investigations I found that if the person looked older than his years, his lens invariably showed that he possessed less accommodative power than the average—in other words, the lens was sclerosed more than it should be. It is common knowledge that the average man of fifty looks fifty, and acts as if fifty, but many aged fifty look sixty-five, and to all intents and purposes *are* sixty-five, whereas others may look only thirty-eight or forty.

Another cause of premature senility, as evidenced in lessened accommodative power, is *eyestrain.* This is probably due not only to loss of nerve energy, but also, to a certain extent, to degenerative changes caused by the unequal contraction of the ciliary muscle, which I have already referred to.

But perhaps to-day the most common causes of premature senility are care, worry and anxiety, and overwork.

This awful War has hastened the onset of presbyopia, and rapidly increased the defect in those already presbyopic, throughout Europe. The constant strain and anxiety has sapped our nerve energy, and we are all more or less prematurely older. A great writer once said, referring to the universal unrest caused in Europe by the first Napoleon, "The lives of millions were affected by it, the secret hope of thousands undermined by it. It disturbed the sleep of half the world, and made men old before their time." These remarks can more than ever be applied now.

Bearing in mind that the eye seems to be the first index of this premature senility, the presence of premature presbyopia is a danger signal that something is wrong. We cannot remove the anxiety and the worry, only Peace can do that; but we can, and we should, try to conserve all the nerve energy left to us.

The physician or the surgeon should treat the intestinal toxæmia.

We should prevent eyestrain by saving our eyes, by not hesitating to take to glasses for reading, even if we have not reached the average age.

In the treatment of presbyopia a great deal has been accomplished in recent years by improvements in the optician's art. Bi-focal lenses, correcting distant and reading sight in the same glass, are most valuable helps in removing eyestrain in all cases except simple presbyopes. If we correct all our refractive errors in *two* separate glasses we do not change them when we should, and we thus cause strain. To my mind it is impossible to exaggerate the value of these glasses. They come to us at a time when the conservation of nerve energy is more than ever imperative. We have lost the recuperative power and the elasticity of youth. Responsibilities, and with them cares and anxieties, have been increased, and we have not yet reached the callousness of old age!

Finally, in doing our best to alleviate the strain and worry of this War, we can save ourselves and those around us considerably by not anticipating trouble, and by taking care "not to burden to-day's strength with to-morrow's loads."

[E. C.]

GENERAL MONTHLY MEETING,

Monday, July 3, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Mrs. Charles Chenevix-Trench,
James Watt,

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of The Right Hon. Sir James Stirling, P.C. LL.D. F.R.S., on June 27, 1916, and of Professor Silvanus P. Thompson, D.Sc. LL.D. F.R.S., on June 12, 1916, Members of the Royal Institution; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of The Right Hon. Sir James Stirling, a late Lord Justice of Appeal, Privy Councillor, Senior Wrangler, and first Smith's Prizeman in Trinity College, Cambridge, 1860, LL D. (Aber.) F.R.S.

Sir James Stirling was a Member of the Royal Institution for twenty-six years, and discharged the official duties of a Manager and Vice-President. He took a great interest in the affairs of the Institution, and rendered it many valuable services in giving the Managers the benefit of his great legal knowledge.

After his retirement from the Bench, he took up again his mathematical and botanical studies, and kept up his interest in the Advancement of Science up to the last. The presence of his familiar figure in the Library and Reading Rooms of the Institution which he so frequently visited, and his attendance at the Lectures, will be much missed.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Stirling and the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Professor Silvanus P. Thompson, D.Sc.(Lond.) LL.D. (Birmingham and Bristol), F.R.S., Principal and Professor of Physics in the City and Guilds Technical College, Finsbury; Past-President of the Institution of Electrical Engineers, the Physical, and other Societies. He contributed a large number of original papers to scientific societies, and published many educational and scientific works, among them being the following:—"Elementary Lessons in Electricity and Magnetism"; "Dynamo-Electric Machinery"; "The Electro-Magnet and Electro-Magnetic Mechanisms"; "Polyphase Electric Currents and Motors"; "Light: Visible and Invisible"; "Magnetism in Growth"; "Design of

Dynamos." He also wrote the Lives of Michael Faraday, Lord Kelvin and Philip Reis, and a translation of the "De Magnete" of William Gilbert of Colchester.

Professor Silvanus P. Thompson was a Member of the Royal Institution for thirty years, and discharged the official duties of a Visitor and a Manager. He always took a keen interest in everything connected with the history, progress and general scientific work of the Royal Institution. He was a man possessed of exceptional gifts, for quite apart from his scientific qualifications, he was an artist of no mean pretensions, and had extraordinary literary and linguistic aptitudes. He was an eloquent and clear exponent of scientific problems, and the experimental illustrations to his lectures were always carefully devised and extremely original. He delivered no less than thirty-six lectures to the Members of the Royal Institution in the course of twenty-seven years, which were always exceedingly attractive, and highly appreciated by the Members and the World of Science.

The subjects of his Christmas Courses of Lectures were:—"Light: Visible and Invisible," published in 1897; and on "Sound: Musical and Non-Musical."

He delivered Day Lectures on the following subjects:—"The Dynamo"; "Electric Eddy-Currents" (The Tyndall Lectures); "The 'De Magnete' and its Author: 1. The Book; 2. The Man"; "Studies in Magnetism" (The Tyndall Lectures); "Illumination: Natural and Artificial"; "Faraday and the Foundations of Electrical Engineering."

The Friday Evening Discourses identified with his own special field of enquiry—viz. Acoustics, Optics, Electricity and Magnetism—were as follows:—

"The Physical Foundation of Music"; "Optical Torque"; "Transformations of Electric Currents"; "Electric Shadows and Luminescence"; "Magnetism in Transitu"; "The Electric Production of Nitrates from the Atmosphere"; "The Secret of the Permanent Magnet"; "Corona and other Forms of Electric Discharge," the last of which was delivered this year.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Mrs. Thompson and the family in their bereavement.

The Chairman declared, in the terms of Bye-Laws, Chapter IV. Article 2, that there was a vacancy in the Office of Manager through the decease of Dr. Silvanus P. Thompson, and that at the next General Meeting on November 6, the vacancy will be filled up in accordance with the said Bye-Law.

The Following Report from the Managers to the General Monthly Meeting of the Members was Read:

The Managers desire to inform the Members that they have received a communication from Mr. Robert Mond, acting on behalf of the Executors of the late Dr. Ludwig Mond, Founder of the Davy Faraday Laboratory of the Royal Institution, informing them that they have been and are taking the necessary steps to accumulate Trustee Securities, which will enable them in the near future to discharge their liabilities under the Will and Trust Deed towards the Managers of the Royal Institution.

The Executors add that they are pleased to think that, with the high rate of interest prevailing, the income of the Davy Faraday will be substantially increased. The Executors hope that in view of the gradual awakening to the necessity of Scientific Research Work which is slowly making itself perceptible, the facilities provided by the Davy Faraday Research Laboratory will substantially assist in the progress of research.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XI. Part 2. 8vo. 1916.
Report on Agriculture, 1914–15. 8vo. 1916.
Pusa: Memoirs of the Department of Agriculture: Botanical Series, Vol. VII. No. 7. 8vo. 1915; Chemical Series, Vol. IV. Nos. 4–5. 8vo. 1916.
Kodaikanal Observatory Bulletin, Nos. 49–50. 4to. 1916.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXV. 1° Semestre, Fasc. 9–11; Classe di Scienze Morali, Serie Quinta, Vol. XXV. Fasc. 1–2. 8vo. 1916.
Accountants, Association of—Journal for June 1916. 8vo.
American Chemical Society—Journal for June 1916. 8vo.
Journal of Industrial and Engineering Chemistry for June 1916. 8vo.
American Geographical Society—Geographical Review for June 1916. 8vo.
American Journal of Physiology—Vol. XL. No. 4. 8vo. 1916.
American Philosophical Society—Proceedings, Vol. LIV. No. 220, Oct.–Dec. 1915. 8vo. 1915.
Astronomer Royal—Report of H.M. Astronomer at the Cape of Good Hope, 1914. 4to. 1915.
Report to the Board of Visitors of the Royal Observatory. 8vo. 1915.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVI. No. 6. 8vo. 1916.
Bankers, Institute of—Journal, Vol. XXXVII. Part 6, 1916. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIII. Nos. 15–16. 4to. 1916.
British Association for the Advancement of Science—Report of the Eighty-Fifth Meeting (Manchester, 1915). 8vo. 1916.
British Astronomical Association—Journal, Vol. XXVI. No. 7. 8vo. 1916.
Buenos Aires—Monthly Bulletin of Municipal Statistics for Jan.–Feb. 1916. 4to.
British Museum, The Director—Catalogue of Printed Books, Accessions, New Series, Part 707. 4to. 1916.
Canada, Department of Mines—Geological Survey: Memoirs, Nos. 55, 77 and 79. 8vo. 1914–15.
Mines Branch: Bulletin, Nos. 12–13, 1916.
Investigation of a Reported Discovery of Phosphate in Alkali.
Description of the Laboratories of the Mines Branch of the Department of Mines, Ottawa. 8vo. 1916.
Summary Report for 1915. 8vo. 1916.
Chemical Industry, Society of—Journal, Vol. XXXV. Nos. 10–12. 8vo. 1916.
Chemical Society—Journal for June 1916. 1916.
Editors—Aeronautical Journal for June 1916. 8vo.
Amateur Photographer for June 1916. 4to.
American Journal of Science for June 1916. 8vo.
Athenæum for June 1916. 4to.
Author for June 1916. 8vo.

Editors—continued.
Chemical News for June 1916. 4to.
Chemist and Druggist for June 1916. 8vo.
Church Gazette for June 1916. 8vo.
Concrete for June 1916. 8vo.
Dyer and Calico Printer for June 1916 4to.
Electric Vehicle for June 1916. 8vo.
Electrical Engineering for June 1916. 4to.
Electrical Industries for June 1916. 4to.
Electrical Times for June 1916. 4to.
Electricity for June 1916. 8vo.
Engineer for June 1916. fol.
Engineering for June 1916. fol.
Ferro-Concrete for June 1916. 8vo.
General Electric Review for June 1916. 8vo.
Horological Journal for June 1916. 8vo.
Illuminating Engineer for June 1916. 8vo.
Journal of Physical Chemistry for June 1916. 8vo.
Journal of the British Dental Association for June 1916. 8vo.
Junior Mechanics for June 1916. 8vo.
Law Journal for June 1916. 8vo.
Marine Engineer for June 1916. 8vo.
Model Engineer for June 1916. 8vo.
Musical Times for June 1916. 8vo.
Nature for June 1916. 4to.
New Church Magazine for June 1916 8vo.
Nuovo-Cimento for March-April 1916. 8vo.
Page's Weekly for June 1916. 8vo.
Physical Review for April-May 1916. 8vo.
Power for June 1916. 8vo.
Power-User for June 1916. 8vo.
Science Abstracts for June 1916. 8vo.
Tchèque, La Nation, for June 1916. 8vo.
Terrestrial Magnetism for June 1916. 8vo.
War and Peace for June 1916. 8vo.
Wireless World for June 1916. 8vo.
Zoophilist for June 1916. 8vo.

Electrical Engineers, Institution of—Journal, Vol. LIV. No. 260. 8vo. 1916.

Faraday Society—Transactions, Vol. XI. Parts 2–3. 8vo. 1916.

Franklin Institute—Journal, Vol. CLXXXI. No. 6. 8vo. 1916.

Geneva, Société de Physique—Compte Rendu, No. 32, 1915. 8vo. 1916.
Mémoires, Vol. XXXVIII. Fasc. 4–5. 4to. 1916.

Geographical Society, Royal—Journal, Vol. XLVII. No. 6. 8vo. 1916.

Geological Society—Abstracts of Proceedings, No. 992. 8vo. 1916.

Horticultural Society, Royal—Journal, Vol. XLI. Part 3. 8vo. 1916.

Johns Hopkins University—Circulars, 1915, Nos. 6–10; Studies, Series XXXIII. No. 4 and Series XXXIV. No. 1. 8vo. 1915–16.

Life-Boat Institution, Royal National—Journal for May. 8vo. 1916.

London County Council—Gazette for June 1916. 4to.

Marconi's Wireless Telegraph Co., Ltd.—The Year-Book of Wireless Telegraphy, 1916. 8vo.

Meteorological Office—Monthly Weather Reports for May 1916. 4to.
Weekly Weather Reports for June 1916. 4to.
Daily Readings for April 1916. 4to.

Microscopical Society, Royal—Journal, Part 3, 1916. 8vo.

Montpellier Académie des Sciences—Bulletin, Nos. 4–5. 8vo. 1916.

New South Wales—Report of the Comptroller-General of Prisons for the Year 1915. fol. 1916.

New York Academy of Sciences—Annals, Vol. XXVII. pp. 1-15, 17-22, 23-29. 8vo. 1916.

New York, Society for Experimental Biology—Proceedings, Vol. XIII. No. 7. 8vo. 1916.

New Zealand, High Commissioner for—Official Year-Book, 1915. 8vo.
Patent Office Journal, May 1916. 8vo.
Statistics, 1914, Vol. III. 4to. 1915.

Numismatic Society, Royal—Numismatic Chronicle, 4th Series, No. 61, Part 1. 1916. 8vo.

Pharmaceutical Society of Great Britain—Journal for June 1916. 8vo.

Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXVII. Part 3. 8vo. 1916.

Photographic Society, Royal—Journal, Vol. LVI. No. 6. 8vo. 1916.

Physical Society of London—Proceedings, Vol. XXVIII. Part 4. 8vo. 1916.

Post Office Electrical Engineers, Institute of—Notes on the Dessication of Lead-Covered Paper Core Cables. By J. Caradoc Jones, A.M.I.E.E.
Power Circuit Interference with Telegraphs and Telephones. By S. C. Bartholomew. 8vo. 1916.

Quekett Microscopical Club—Journal, Series 2, Vol. XIII. No. 78. 1916. 8vo.

Rio de Janeiro—Annuario, 1916. 8vo.

Rockfeller Institute for Medical Research—Studies, Vol. XXII. 8vo. 1915.

Royal Colonial Institute—United Empire, Vol. VII. No. 6. 8vo. 1916.

Royal Engineers' Institute—Journal, Vol. XXIII. No. 6, June 1916. 8vo.

Royal Society of Arts—Journal for June 1916. 8vo.

Royal Society of London—Philosophical Transactions, Series A, Vol. CCXVI. Nos. 543-547; B, Vol. CCVII. Nos. 343-347. 4to. 1916.
Proceedings: Series A, Nos. 640-641; Series B, Vol. LXXXIX. Nos. 613-614. 4to. 1916.

Sanitary Institute, Royal—Journal, Vol. XXXVII. No. 2. 8vo. 1916.

Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXII. No. 6. 8vo. 1916.

Selborne Society—Selborne Magazine for June 1916. 8vo. 1916.

Smithsonian Institution—Report on U.S. National Museum, 1915. 8vo. 1916.
Miscellaneous Collections, Vol. LV. No. 9; Vol. LVI. No. 1. 8vo. 1916.

Tôhoku Imperial University, Sendai, Japan—Science Reports, First Series, Vol. V. No. 1. 8vo. 1916.

Tôhoku Mathematical Journal—Vol. IX. No. 3. 8vo. 1916.

United Service Institution, Royal—Journal for June 1916. 8vo.

United States Department of Agriculture—Journal of Agricultural Research, Vol. VI. Nos. 8-11. 1916. 8vo.
Experiment Station Record, Vol. XXXIV. No. 7. 8vo. 1916.

United States, Department of Commerce and Labour—Observations made at the U.S. Coast and Geodetic Survey Observation, 1913-14. 4to. 1916.

United States, Department of the Interior—Geological Survey: Bulletins, Nos. 606, 616, 620 I, K, L, M, N, P; 621 K, L, N, O. 8vo. 1916.
Water Supply Papers, Nos. 351, 359, 372, 373, 385, 397, 400A. 8vo. 1915-16.
Mineral Resources, Part IA, I. 14-24; Part II. 31-34. 8vo. 1916.
Geologic Atlas, Folios 195-198. fol. 1915.
Thirty-sixth Annual Report. 8vo. 1915.

United States, Library of Congress—Report of the Librarian, 1915. 8vo. 1916.

United States Patent Office—Official Gazette, June 1916. 8vo.

Washington, National Academy of Sciences—Proceedings, Vol. II. No 6. 1916.

Western Australia, Agent-General for—Statistical Abstract for Jan.-Feb. 1916. 4to.

Zoological Society of London—Proceedings, 1916, Part 1. 8vo. 1916.

GENERAL MONTHLY MEETING,

Monday, November 6, 1916.

HIS GRACE THE DUKE OF NORTHUMBERLAND, President, in the Chair.

Lord Glenconner,
Sir William Trollope, Bart.,

were elected Members of the Royal Institution.

The Secretary announced the decease of Sir Victor Horsley, M.B. B.S. F.R.S. F.R.C.S., on July 16, 1916, a Member of the Royal Institution; and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:

RESOLVED, That the Managers of the Royal Institution of Great Britain, at this their first Meeting held since his decease, desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Sir Victor Horsley, M.B. (Lond.) B.S. F.R.S. F.R.C.S., late Secretary to the Royal Commission on Hydrophobia; Fullerian Professor of Physiology, Royal Institution; Professor of Pathology to University College.

Sir Victor Horsley was Consultant with the Forces in Mesopotamia, and was not only a very distinguished Surgeon, but an Original Investigator in the Field of Medicine and Surgery. He was First Medallist of the Lannelongue International Prize in Surgery; Royal Medallist of the Royal Society; LL.D. and D.C.L. Montreal and McGill Universities; Foreign Associate of the Academy of Medicine, Paris; Member of Medical Societies in Rome, Petrograd, Sweden, Philadelphia, etc. He was awarded the "Cameron" and "Fothergill" Gold Medals, and was First Chairman of the Representative Meeting of the British Medical Association.

Sir Victor Horsley was a Member of the Royal Institution for twenty-nine years, and discharged the official duties of Professor, Visitor, and Manager. He was the Author of the following Books:—"Structure and Function of the Brain and Spinal Cord"; "The Cerebellum"; "On the Alleged Responsibility of the Medical Profession for the Re-introduction of the Rum Ration into the British Army"; "Alcohol and the Human Body."

The titles of the Lectures delivered by Sir Victor Horsley during his occupancy of the Fullerian Professorship of Physiology (1891–1894) were:—"The Modern Physiology of the Brain and its Relation to the Mind"; "The Structure and Functions of the Nervous System: The Spinal Cord and Ganglia"; "The Brain"; "The Functions of the Cerebellum and the Elementary Principles of Psycho-Physiology."

Sir Victor Horsley delivered five Friday Evening Discourses, on the following subjects:—"Motor Centres of the Brain, and the Mechanism of the Will" (1885); "Brain Surgery in the Stone Ages" (1887); "Hydrophobia" (1891); "The Destructive Effects of Projectiles" (1894); "Roman Defences of South-East Britain" (1899).

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Horsley and the Family in their bereavement.

The Special Thanks of the Members were returned to Richard Pearce, Esq., F.G.S., for his donotion of £100, and to John Y. Buchanan, Esq., for his Donations of £100 to the Fund for the Promotion of Experimental Research at Low Temperature, and of £100 to the General Fund.

Dr. H. E. Armstrong, Ph.D. F.R.S., was elected a Manager in place of the late Professor S. P. Thompson.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XI. Part 3. 8vo. 1916.
Special Indian Science Congress No. 1916.
Geological Survey of India, Records, Vol. XLVII. Parts 1, 2.
Contents and Index of Memoirs, Vols. XXI.-XXXV. 8vo. 1916.
Agricultural Research Institute, Pusa: Bulletin, No. 61. 8vo. 1916.
Pusa: Memoirs of the Department of Agriculture: Botanical Series, Vol. VIII. Nos. 1-3; Chemical Series, Vol. IV. No. 7. 8vo. 1916.
Kodaikanal Observatory Bulletin, No. 51. 4to. 1916.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXV. 1° Semestre, Fasc. 12, and 2° Semestre, Fasc. 1-7; Classe di Scienze Morali, Serie Quinta, Vol. XXV. Fasc. 3-4. 8vo. 1916.

Accountants, Association of—Journal for July-August, 1916. 8vo.

Admiralty, Lords Commissioners of the—Nautical Almanac for 1919. 8vo. 1916.

Allegheny Observatory—Miscellaneous Scientific Papers, Vol. III. Nos. 22-23. 4to. 1916.

American Chemical Society—Journal for July-October, 1916. 8vo.
Journal of Industrial and Engineering Chemistry for July-October, 1916. 8vo.

American Folk-Lore Society—Journal, Vol. XXIX. No. 3, January-March, 1916. 8vo.

American Geographical Society—Geographical Review for July-October, 1916. 8vo.

American Journal of Philology—Vol. XXXVII. No. 3. 8vo. 1916.

American Journal of Physiology—Vol. XLI. Nos. 1-4. 8vo. 1916.

American Philosophical Society—Proceedings, Vol. LV. Nos. 1-5. 8vo. 1916.

Asiatic Society, Royal—Journal for July-October, 1916. 8vo.
Bombay Branch:—Journal, Vol. XXIV. No. 2. 8vo. 1916.

Astronomical Society, Royal—Monthly Notices, Vol. LXXVI. Nos. 7-8. 8vo. 1916.
List of Fellows, 1916. 8vo.

Backer, Dr. H. J.—Macht en Idealen der Organische Chemie. 8vo. 1916.

Badgley, Colonel W. F. (*The Author*)—Electricity and its Source. 8vo. 1916.

Bankers, Institute of—Journal, Vol. XXXVII. Parts 7-8, 1916. 8vo.

Batavia, Royal Magnetical and Meteorological Observatory—Regenwaarnemingen in Nederlandsch. India, 1914. Deel 2. 8vo. 1915.
Verhandelingen, No. 4. 8vo. 1916.
Observations Vol. III. (1913). 8vo. 1916; and Vol. XXXV. (1912). 8vo. 1915.
Boston Public Library—Bulletin, Third Series, Vol. IX. Nos. 2-3.
Sixty-fourth Annual Report, 1915-16. 8vo. 1916.
Botanic Society, Royal—Botanical Journal, Vol. V. No. 2. 8vo. 1916.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIII. Nos. 17-20. 4to. 1916.
British Astronomical Association—Journal, Vol. XXVI. Nos. 8-9. 8vo. 1916.
Memoirs, Vol. XX. Part 3. 8vo. 1916.
List of Members, 1916. 8vo.
British Museum Trustees—Library Catalogue, Vol. V. 4to. 1916.
Catalogue of Ungulate Mammals, Vol. V. 8vo. 1916.
Catalogue of Fresh-water Fishes of Africa, Vol. IV. 4to. 1916.
Catalogue of Cretaceous Flora, Part 2. 8vo. 1916.
Guide to Disease-carrying Insects. 8vo. 1916.
The Louse and its Relation to Disease. 8vo. 1916.
Brooklyn Institute of Arts and Sciences—Brooklyn Museum: Science Bulletin, Vol. III. No. 1. 8vo. 1916.
Buenos Aires—Monthly Bulletin of Mnnicipal Statistics for March-June, 1916. 4to.
Cambridge Philosophical Society—Proceedings, Vol. XVIII. Parts 5-6. 8vo. 1916.
Canada, Department of Marine—Report of the Meteorological Service for 1913. 8vo. 1916.
Monthly Record of the Meteorological Observations in the Dominicn of Canada, etc. January, 1916. 4to.
Canada, Department of Mines—Geological Survey: Memoirs, No. 83. 8vo. 1914-15.
Mines Branch: Bulletin, No. 11. 8vo. 1915.
Carnegie Endowment for International Peace—Year-Book for 1916. 8vo.
Carnegie Institution—Communications to the National Academy of Sciences, Nos. 27-34. 8vo. 1916.
Contributions from the Mount Wilson Solar Observatory, Nos. 109, 112-114. 8vo. 1916.
Chemical Industry, Society of—Journal, Vol. XXXV. Nos. 13-19. 8vo. 1916.
Chemical Society—Journal for July-October, 1916. 1916.
List of Fellows, 1916. 8vo.
Chemistry, Institute of—Proceedings, Part 3, 1916. 8vo.
Chicago, John Crerar Library—Twenty-first Annual Report, 1915. 8vo. 1916.
Chile, Central Meteorological Institute—Publications, Nos. 13-17. 4to. 1915.
Christiania, Royal Frederick University—Abstracts. By Kr. Birkeland. 8vo. 1915-16.
Civil Engineers, Institution of—List of Members, 1916. 8vo.
Minutes of Meetings, Vol. CCI. 8vo. 1916.
Committee of Privy Council for Scientific and International Research. Report for 1915-16. 8vo. 1916.
de Candolle, M.I.—Un Anglais Prisonnier a Verdun. Par F. F. Roget. 8vo. 1916.
Dunk, John L. (*The Author*)—Hyperacoustics (Div. I). 8vo. 1916.
East India Association—Journal, New Series, Vol. VII. Nos. 3-4. 8vo. 1916.
Editors—Aeronautical Journal for July-September, 1916. 8vo.
Amateur Photographer for July-October, 1916. 4to.
American Journal of Science for July-October, 1916. 8vo.
Athenæum for July-October, 1916. 4to.

Editors—*continued.*
Author for July-October, 1916. 8vo.
Chemical News for July-October, 1916. 4to.
Chemist and Druggist for July-October, 1916. 8vo.
Church Gazette for July-October, 1916. 8vo.
Concrete for July-October, 1916. 8vo.
Dyer and Calico Printer for July-October, 1916. 4to.
Electrical Vehicle for September, 1916. 8vo.
Electrical Engineering for July-October, 1916. 4to.
Electrical Industries for July-October, 1916. 4to.
Electrical Times for July-October, 1916. 4to.
Electricity for July-October, 1916. 8vo.
Engineer for July-October, 1916. fol.
Engineering for July-October, 1916. fol.
Ferro-Concrete for July-October, 1916. 8vo.
General Electric Review for July-October, 1916. 8vo.
Horological Journal for July-October, 1916. 8vo.
Illuminating Engineer for July-October, 1916. 8vo.
Journal of Physical Chemistry for October, 1916. 8vo.
Journal of the British Dental Association for July-October, 1916. 8vo.
Junior Mechanics for July, September, October, 1916. 8vo.
Law Journal for July-October, 1916. 8vo.
Marine Engineer for July-October, 1916. 8vo.
Model Engineer for July-October, 1916. 8vo.
Musical Times for July-October, 1916. 8vo.
Nature for July-October, 1916. 4to.
New Church Magazine for July-October, 1916. 8vo.
Page's Weekly for July-October, 1916. 8vo.
Physical Review for June-October, 1916. 8vo.
Power for July-October, 1916. 8vo.
Power-User for July-October, 1916. 8vo.
Science Abstracts for July-October, 1916. 8vo.
Tchèque, La Nation, for July-October, 1916. 8vo.
War and Peace for July-October, 1916. 8vo.
Wireless World for July-October, 1916. 8vo.
Zoophilist for July-October, 1916. 8vo.

Florence, Biblioteca Nazionale—Bulletin, Nos. 186-189. 8vo. 1916.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. XIII. Disp. 2-4. 8vo. 1916.
Franklin Institute—Journal, Vol. CLXXXII. Nos. 1-4. 8vo. 1916.
Geographical Society, Royal—Journal, Vol. XLVIII. Nos. 1-5. 8vo. 1916.
Geological Society—List of Fellows, 1916. 8vo.
Quarterly Journal, Vol. LXXI. Part 3. 8vo. 1916.
Abstracts of Proceedings, Nos. 993, 994. 8vo. 1916.
Geographical Survey of Great Britain—Summary of Progress, 1915. 8vo. 1916.
Imperial College of Science—Calendar, 1916-17. 8vo.
Imperial Institute—Bulletin, April-June, 1916. 8vo.
Innes, John Rose, M.A. B.Sc. M.R.I.—Germany in Defeat. By Count de Souza and Major Macfall. 12mo. 1916.
Monographs on Modern Mathematics. Edited by J. W. A. Young. 8vo. 1911.
Theory of Infinite Series. By T. J. Bromwich. 8vo. 1908.
Jedlowski, Dr. Josif—Various Pamphlets and Bulletins of the Southern Slav Library. 12mo. 1916.
Life-Boat Institution, Royal National—Journal for August. 8vo. 1916.
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Zoology, Vol. XXXII. No. 221; Vol. XXXIII. No. 222. 8vo. 1916.
London County Council—Gazette for July-October, 1916. 4to.
London Society—Journal, No. 11. 8vo. 1916.

Madrid, Reale Academia de Ciencias—Revista, Tomo XII. Nos. 8-12. 1914; Tomo XIII. Nos. 1-12. 1914-16; Tomo XIV. Nos. 1-11. 1915-16. 8vo.
Annuario, 1915-16. 16mo.
Manchester Municipal School of Technology—Report of the Godlee Observatory, 1913-15. 8vo. 1916.
Journal, Vol. VIII., 1914. 8vo. 1916.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. LX., Parts 1-2. 8vo. 1916.
Mechanical Engineers, Institution of—List of Members, 1916. 8vo.
Proceedings 1916, Jan.-May. 8vo.
Mersey Conservancy—Report on the present state of the Navigation of the River Mersey (1915). 8vo. 1916.
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Meteorological Society, Royal—Journal (Quarterly), Vol. XLII. Nos. 179-180. 8vo. 1916.
Metropolitan Asylums Board—Annual Reports, 1915. 8vo. 1916.
Mexico, Sociedad Cientifica "Antonio Alzate."—Memorias, Tome XXXII. Nos. 11-12. 8vo. 1915.
Microscopical Society, Royal—Journal, Parts 4-5, 1916. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 318, 320-322. 8vo. 1916.
National Physical Laboratory—Annual Report, 1915-16. 8vo. 1916.
New South Wales, Royal Society of—Journal and Proceedings, Vol. XLIX. Part 3-4. 8vo. 1915.
New York Academy of Sciences—Annals, Vol. XXVI. pp. 395-486. 8vo. 1916.
New York, Society for Experimental Biology—Proceedings, Vol. XIII. No. 8. 8vo. 1916.
New Zealand, High Commissioner for—Official Year-Book, 1914. 8vo.
Patent Office Journal, June-October, 1916. 8vo.
Statistics, 1914, Vols. I, II., IV. 4to. 1915.
Mining Handbook, 1906. 8vo.
North of England Institute of Mining Engineers—Transactions, Vol. LXVI. Nos. 3-7. 8vo. 1916.
Annual Report, 1916-17. 8vo. 1916.
Numismatic Society, Royal—Numismatic Chronicle, 4th Series, No. 62, Part 2. 1916. 8vo.
Paris, Académie des Sciences—Comptes Rendus, Tome 163. 4to. 1916.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May-June, July-August. 4to. 1916.
Pharmaceutical Society of Great Britain—Journal for July-October, 1916. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXVIII. Parts 1, 2. 8vo. 1916.
Physical Society of London—Proceedings, Vol. XXVIII. Part 5. 8vo. 1916.
Popovic, Professor Pavle, Belgrade University—Three Booklets dealing with Serbian Literature. 8vo. 1915.
Post Office Electrical Engineers, Institute of—Journal, Vol. IX. Parts 2, 3. 8vo. 1916.
Papers Nos. 65, 67.
Pratt, Edwin A. (*The Author*)—A London Transport Trust. 8vo. 1916.
Radcliffe Observatory, Oxford—Results of Meteorological Observations in five years, 1911-15. 8vo. 1916.
Ralli, Mrs. Stephen, M.R.I.—The Political Life of M. Tricoupi, 1896-97. 8vo. 1911.
Rio de Janeiro—Archives of Botanical Gardens, Vol. I. Fac. I. 8vo. 1915.
Rockfeller Institute for Medical Research—Studies, Vol. XXIII. 8vo. 1916.
Rome, Ministry of Public Works—Giornale del Genio Civile for March-June, 1916. 8vo.
Rontgen Society—Journal, Vol. XII. Nos. 48, 49. 8vo. 1916.

Roumanian Academy, Bucharest—Bulletin Scientifique, Vol. IV. No. 10; Vol. V. No. 1. 8vo. 1916.
Royal Colonial Institute—United Empire, Vol. VII. Nos. 7-10. 8vo. 1916.
Royal Dublin Society—Scientific Proceedings, Vol. XV. Nos. 1-14, and Title, Vol. XIV. 8vo. 1916.
Economic Proceedings, Vol. II. No. 11. 8vo. 1916.
Royal Engineers' Institute—Journal, Vol. XXIV. Nos. 1-5. 1916. 8vo.
Royal Irish Academy—Proceedings, Vol. XXXIII. A, Nos. 1-5; B, Nos. 1-3; C, Nos. 1-11. 8vo. 1916.
Vol. XXXII. B, Nos. 9-12; C, No. 21.
Royal Society of Arts—Journal for July-October, 1916. 8vo.
Royal Society of Edinburgh—Transactions, Vol. L. Parts 3, 4, 1914-15. 4vo. 1916.
Royal Society of London—Philosophical Transactions, Series A, Vol. CCXVI. No. 548; Vol. CCXVII. Nos. 549, 550; B, Vol. CCVIII. Nos. 348-351. 4to. 1916.
Proceedings: Series B, Vol. LXXXIX. No. 615. 4to. 1916.
Sanitary Institute, Royal—Journal, Vol. XXXVII. No. 3. 8vo. 1916.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXII. Nos. 7-10. 8vo. 1916.
Selborne Society—Selborne Magazine for July-October, 1916. 8vo. 1916.
Smithsonian Institution—Miscellaneous Collections, Vol. LXII. No. 5; Vol. LXIV. No. 4; Vol. LXVI. Nos. 3, 4, 5, 7, 8. 8vo. 1916.
Annual Report of the Smithsonian Institution, 1915. 8vo. 1916.
Società degli Spettroscopisti Italiani—Memorie, Series 2, Vol. V. Disp. 5-9. 4to. 1916.
South Australian School of Mines and Industries—Annual Report for 1915 and Prospectus for 1916. 8vo. 1916.
Statistical Society, Royal—Journal, Vol. LXXIX. Part 3, 4. 8vo. 1916.
The Committee of the Pilgrims, Pilgrim's Club, Savoy Hotel—America and the Allies: Address by The Hon. James M. Beck at a Meeting of the Pilgrims on July 5, 1916. 1916.
Tôhoku Imperial University, Sendai, Japan—Science Reports, First Series, Vol. V. Nos. 2-4. 8vo. 1916.
Tôhoku Mathematical Journal—Vol. IX. No. 4; Vol. X. Nos. 1, 2. 8vo. 1916.
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United States Department of Agriculture—Journal of Agricultural Research, Vol. VI. Nos. 12-25. 1916. 8vo.
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Results of Magnetic Observations at Tucson, Arizona, 1913 and 1914. 4to. 1916.
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Washington, National Academy of Sciences—Memoirs, Vol. XIV. First Memoir. 4to. 1916.
Proceedings, Vol. II. Nos. 7-10. 1916.
Western Society of Engineers—Journal, Vol. XX. No. 10. 8vo. 1915; Vol. XXI. No. 1-6. 8vo. 1916.
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Transactions, Vol. XXI. Part 1. 4to. 1916.

GENERAL MONTHLY MEETING,

Monday, December 4, 1916.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

His Excellency M. Paul Painleve,
Vito Volterra,

were elected Honorary Members of the Royal Institution; and

Neville Bayly,
William D. Mitchell Cotts,
Mrs. Maude Dickinson,
Arthur L. Gibson,
Mr. Arthur Keith,

were elected Members.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Palæontologia Indica: New Series, Vol. IV. Nos. 1-4; Vol. V. No. 3; Fifteenth Series, Vol. II. Nos. 1-5; Vol. V. Nos. 1-3; Vol. VI. 1-2; Vol. VII. Nos. 1-2. 4to. 1915.
Geological Survey of India: Memoirs, Vol. XLIII. Part 2. 8vo. 1916.
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Accountants, Association of—Journal for Sept.-Nov. 1916. 8vo.
American Chemical Society—Journal for Nov. 1916. 8vo.
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Birmingham and Midland Institute—Meteorological Observations, 1915. 8vo. 1916.
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British Architects, Royal Institute of—Journal, Third Series, Vol. XXIV. Nos. 1 & 2. 4to. 1916.
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Electrical Engineering for November, 1916. 4to.
Electrical Industries for November, 1916. 4to.
Electrical Review for November, 1916. 4to.
Electrical Times for November, 1916. 4to.
Electricity for November, 1916. 8vo.
Engineer for November, 1916. fol.
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Ferro-Concrete for November, 1916. 8vo.
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Horological Journal for November, 1916. 8vo.
Illuminating Engineer for November, 1916. 8vo.
Journal of Physical Chemistry for November, 1916. 8vo.
Journal of the British Dental Association for November, 1916. 8vo.
Junior Mechanics for November, 1916. 8vo.
Law Journal for November, 1916. 8vo.
Marine Engineer for November, 1916. 8vo.
Marine Magazine for November, 1916. 4to.
Model Engineer for November, 1916. 8vo.
Musical Times for November, 1916. 8vo.
Nature for November, 1916. 4to.
New Church Magazine for November, 1916. 8vo.
Page's Weekly for November, 1916. 8vo.
Physical Review for November, 1916. 8vo.
Power for November, 1916. 8vo.
Power-User for November, 1916. 8vo.
Science Abstracts for November, 1916. 8vo.
Tchèque, La Nation, for November, 1916. 8vo.
Terrestrial Magnetism for September, 1916. 8vo.
War and Peace for November, 1916. 8vo.
Wireless World for November, 1916. 8vo.
Zoophilist for November, 1916. 8vo.
Franklin Institute—Journal, Vol. CLXXXII. No. 5. 8vo. 1916.
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Journal: Botany, Vol. XLIII. No. 292. 8vo. 1916.
Zoology, Vol. XXXIII. No. 223. 8vo. 1916.
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Royal Society of Arts—Journal for Nov. 1916. 8vo.
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Washington, National Academy of Sciences—Proceedings, Vol. II. No. 11. 1916.
Wisconsin Academy—Transactions, Vol. XVIII. Part 1. 8vo. 1915.

WEEKLY EVENING MEETING,

Friday, May 26, 1916.

Sir William Phipson Beale, Bart., K.C. M.P. F.C.S.,
Vice-President, in the Chair.

Professor C. G. Barkla, M.A. D.Sc. F.R.S.

X-Rays, Atomic Structure, Electromagnetic Radiation.

[Abstract.]

I propose to give an outline, not of the properties of Röntgen radiation in general—that would be too large a task—but only of some of those properties which have an obvious bearing on two of the most important and the most fascinating problems in Physical Science—the problems of atomic structure and of electromagnetic radiation, particularly of X-radiation.

Before Sir Joseph Thomson's discovery of the electron, that is, of small particles of electricity freed from ordinary matter, it had been generally assumed that all bodies contained equal quantities of positive and negative electricity, which just neutralised each other. The process of electrification of any body was simply a process by which the body was given an excess of one kind of electricity or the other. Thomson's experiments showed that in a space relatively free from ordinary matter (the space inside a highly exhausted discharge tube) these particles could be obtained free, whatever the gas inside the tube and whatever the nature of the electrodes conveying electricity to and from the gas.

Since these early experiments, electrons have been obtained from matter by very different methods—by (1) heating; (2) bombardment by swiftly moving small particles as α and β rays; (3) exposure to light, especially of short wave-length; (4) exposure to X-rays; and (5) they are spontaneously emitted by substances we call radioactive substances.

In some of these cases the supply of electrons is largely dependent on the presence of other substances, or is influenced by surface conditions, while radioactive processes have only been observed in a limited number of elements. Perhaps the most convincing proof that electrons are not obtained from an impurity common to all substances, or that they do not reside simply on the surfaces of atoms, is that the number emitted from a substance exposed to X-rays is an atomic property unaffected by physical conditions or even by chemical combination, and it can be foretold for an element if we know its atomic weight, or, more accurately, its atomic number.

Further, we can by X-rays get electrons from the *interior* without permanently affecting the stability of an atom.

We can, as I shall show, find the *number* of electrons in the atom; we can divide these into groups at different depths within the atom, and compare their properties—properties of course depending on their position and state within the atom.

Let us indicate methods of studying the electrons emitted by substances exposed to Röntgen radiation in the form of the secondary corpuscular radiation.

Precise measurements upon the corpuscular (electronic) radiation emitted by substances exposed to X-rays may be made by various methods:—

(*a*) By exposing a substance in an evacuated chamber to X-rays the magnetic deflection of the emitted particles may be observed much as in Thomson's experiment on Kathode Rays. Such measurements were made by Innes, and, assuming a value for e/m, the velocity was obtained.

(*b*) Observations, as by Townsend, of the variation of ionisation between two parallel plates with their distance apart, when one or both plates is exposed to X-rays, show the maximum distance traversed by the particles in air or any other gas. (The plates may be kept fixed and the pressure of the gas varied.) From Whiddington's Law, connecting length of path d and initial velocity v, i.e. $v^4 = ad$ when a is a known constant, the maximum velocity of emission may be determined.

(*c*) By C. T. R. Wilson's condensation experiments the tracks may be seen and measured, and the velocity of the electrons deduced.

(*d*) As all X-ray ionisation is by the swiftly moving electrons (secondary corpuscular radiation), determinations of the number of electrons emitted by a gas may be made from observations on the ionisation in the gas. From relative ionisations we can get relative numbers of ejected electrons (Barkla, Phil. Trans. A, v. 217, pp. 315-360), and from measurement of the saturation current and a knowledge of the number of ions produced per electron ejected it is possible to get the absolute number of electrons produced by a given beam.

Such experiments give us the number and velocity of electrons expelled by X-rays, and show us how few these are in comparison with the whole number of atoms traversed by the radiation. They show us that the negative electron is a constituent of all matter, but give us no idea of the total number of electrons in an atom.

On the other hand, positive electricity, which must also be a constituent of all matter, has never been obtained in a form which shows it to be free or entirely distinct from matter as ordinarily known. Thus in an evacuated tube the least massive positive particles have been found to possess the mass of the atom of hydrogen; positive particles are not set free either by light or by X-rays; even when liberated in radioactive processes they are atoms of helium.

Positive electricity is thus much more securely fixed in matter. For these and other reasons, Kelvin suggested, and J. J. Thomson developed, the idea that an atom was built up of a uniform sphere of positive electricity with a large number of negative electrons. As the more diffusely distributed a charge is the less is its mass, such a charge would possess little mass—approximately the whole mass of the atom would reside in the electrons. It was therefore necessary to assume a large number of these, say 1700 per atom of hydrogen, and they were supposed to be arranged in rings or shells revolving round the centre of the atom.

It is on this point as to the number of electrons per atom that X-radiation phenomena afford most valuable evidence.

When X-rays are incident on matter, the matter itself emits X-radiations (secondary X-radiations). I have found the secondary X-radiations are made up of radiations of two types, which I have called (*a*) scattered X-radiation, and (*b*) fluorescent (characteristic) X-radiation.

The principal secondary radiation, proceeding from light elements exposed to a primary X-radiation, is a radiation of the same wave-length and the same penetrating power. From observation of the character and intensity of this scattered radiation, I concluded that per electrons in matter were the scatterers, and that the scattering the atom (consequently the number of electrons per atom), for light atoms at least, was proportional to the atomic weight.

J. J. Thomson stated this more precisely by pointing out that each charged particle, when under the influence of the primary X-radiation, is acted upon by the varying electric field, is moved by that field, and so sends out a wave-motion along its own tubes of force. This wave-motion is the scattered radiation.

From measurements of the intensity of radiation scattered applied to J. J. Thomson's calculated value for the energy of a beam lost by scattering from ions (the fraction lost per cm. of path $= \frac{8\pi}{3} N \frac{e^4}{m^2} \mu^2$) I found, with data available, there were about 200 electrons per molecule of air. Applying more recent and exact data, we find approximately $14 \cdot 5 \times (2 \cdot 75 \times 10^{19})$ electrons per c.c. of air under standard conditions.

This and the corresponding measurements for scattering by other substances indicates 7 electrons per atom of N, 8 for O, 6 for C, 16 for S, 1 for H. It is remarkable that the determined number of electrons is as near to 7 for an atom of nitrogen as it is possible to estimate the pressure of the air upon which the experiments were made.

At about the same time as the announcement of these corrected values, Rutherford from experiments on the scattering of α particles arrived at a conclusion which is complementary to this. He concluded that the deflections of α particles observed are not such as could be explained either by the electrons or the diffuse positive charge in the atom. He showed it was necessary to replace the diffuse positive charge by a very concentrated positive charge, and found for it a value approximately equal to half the atomic weight times the charge of an electron, i.e. $\frac{w}{2}e$. This just agrees—though the measurements did not permit of equal accuracy—with the $\frac{w}{2}$ electrons with charge $-e$. The two are complementary and give us a neutral atom.

Later work by Moseley on the spectra of the elements given by interference phenomena with X-rays has further supported this conclusion, by showing equal steps between the spectrum of each atom and that of the next in the periodic series, each step roughly corresponding to an increase in the atomic weight by two units.

Bohr has extended the theory to account for the line spectra of light elements.

This, then, is the structure which at present perhaps finds greatest acceptance—a very small nuclear positive charge of unknown but probably quite complicated structure surrounded by a number of electrons at various distances from the centre; the positive nucleus having a charge approximately equal to half the atomic weight times the charge of an electron (e), now more accurately expressed as the atomic number (n) times (e); the number of negative electrons outside the nucleus being approximately half the atomic weight, or, more accurately, equal to the atomic number.

It is evident that a great burden is placed on the solitary electron which with the positive nucleus constitutes a hydrogen atom. To get over the difficulty Bohr has suggested that the electron may be in one of many distinct orbits. The normal hydrogen atom is supposed to have a diameter $1 \cdot 1 \times 10^{-8}$ cm., but according to Bohr's theory it also may be 4, 9, 16, 25 times this, and to explain stellar spectra must be $1 \cdot 2 \times 10^{-5}$ cm. in diameter.

Writing of this theory Jeans says "it explains so many facts, and the numerical agreement with observation is so complete, that little doubt will be felt that it is at least based on a very substantial substratum of truth."

Before proceeding to explain how we can get further information of the atoms from X-ray experiments, let us look at the *theory of scattered radiation* which enabled us to calculate the number of electrons. It is briefly this: The primary waves spread in all directions from the anti-kathode of an X-ray tube and influence every electron in the matter through which they pass. Whatever the normal motion of an electron may be, this produces no appreciable X-radiation ; but the perturbations produced by the primary radiation set up a wave motion along the tubes of force which extend from the electrons in all directions. This radiation from the electrons is called the scattered radiation. It is really a radiation from electrons moved by the primary radiation, and in fact always controlled (so far as the motion producing radiation is concerned) by the primary radiation.

The evidence in support of the fundamental truth of this theory, such as the quality, polarisation, distribution and intensity of the scattered radiation, is so overwhelming that it cannot be questioned. But many of the experimental results, as polarisation and distribution, may be reproduced by transverse wave motion along strings, and only really prove the electromagnetic radiation to be a transverse wave motion. They do not indicate whether the waves are localised and proceed along definite paths or tubes, or whether they spread out in spherical shells. But this is an important question in view of what has been called the quantum theory of radiation, suggesting that radiation occurs in definite units of energy.

According to this theory of light, X-rays, etc.—in fact, of what is called electromagnetic radiation in general—radiation is made up of definite units or bundles, each bundle containing an amount of energy E proportional to the frequency *n* of the radiation with which it is associated. Thus $E = hn$, where h = Planck's constant.

But when we see the results of the old spreading wave theory giving for the number of electrons in a c.c. of air under standard conditions a value $14 \cdot 5 \times 2 \cdot 75 \times 10^{19}$ (i.e. an average of 14·5 electrons per molecule), and when we think that *a priori* there was no reason why it should not be 1000 times this or $\frac{1}{1000}$ of this, we cannot but conclude that this theory is not only adequate, but that it would be exceedingly difficult to supersede.

[If we imagined that ions were the scatterers, the calculation would give 1,000,000 times more ions than molecules present, or trillions times the actual number.]

This is really most remarkable confirmation of the older electromagnetic theory of radiation. It can mean nothing else than that the process is a continuous process, radiation being emitted in any quantity, however small. There is not only no suggestion of a quantum or entity in radiation, but these phenomena of scattering become absolutely meaningless on any such theory.

This, however, is only one of the facts obtained from experiments on the scattering of X-rays that support the theory : the whole body of evidence is remarkably strong.

But close though the agreement is, the simple theory does not seem sufficient to explain all X-ray phenomena. The result of many experiments suggest that the process of radiation is a discontinuous process, that it does not take place steadily but in a jerky fashion, and that radiation and absorption must, as I have said, be in definite units of energy. For instance, the velocity of electrons ejected by X-rays is independent of the intensity of the radiation; it depends only on the wave-length (or frequency), being given by $\frac{1}{2}mv^2 = hn$ from any substance.

This certainly looks as if the radiation consists of indivisible bundles of energy—as though these were totally absorbed or not at all absorbed.

It has also been shown by Beatty and others that when X-rays are produced, radiation of a given wave-length is only emitted by an anti-kathode when the generating kathode particles have a velocity v greater than that given by $\frac{1}{2}mv^2 = hn$.

On the other hand, the phenomena of scattering suggest—in my opinion, prove—that the emission and absorption of X-rays take place in any quantity whatever. It is the phenomena of the transformation of energy from that of moving electrons into X-radiation and from X-radiation into corpuscular radiation that suggests "quanta" of radiation.

These newly discovered facts concerning radiation have not yet been made to fall in completely with the older theory, but attempts are being made to reconcile them as far as possible, and I have quite recently got some most suggestive experimental data on the subject, which I will briefly explain.

Each element under suitable stimulus emits characteristic X-radiations, that is, radiations of wave-length characteristic of the element. The total characteristic radiation of an element thus gives a line spectrum consisting of several widely separated groups of neighbouring lines. These various groups are the J, K, L and M radiations.

To excite one of these groups of radiations, say the K radiation, it is essential that the primary beam be of shorter wave-length than the constituent of shortest wave-length.

It can be simply shown from the variation of absorption in an element with the wave-length of the primary radiation, that certain portions of the absorption are definitely associated with the emission of each group of characteristic radiations. Thus there is the K absorption associated with the emission of K fluorescent characteristic radiation; L absorption associated with the emission of L characteristic radiation; etc.

Similarly we can show that the whole secondary corpuscular radiation emitted by a substance can be divided into groups, each associated with the emission of a particular fluorescent characteristic X-radiation. Thus the three phenomena of absorption of primary radiation, emission of secondary corpuscular radiation, and emission of fluorescent characteristic X-radiation occur together. We have K absorption, K secondary corpuscular radiation, K fluorescent characteristic X-radiation; L absorption, L secondary corpuscular radiation, and L fluorescent characteristic X-radiation; etc.

From measurements of the energy absorbed and the energy re-emitted as corpuscular and as fluorescent characteristic X-radiations, certain very important conclusions may be drawn. The significance of the results is most easily understood if we express them approximately. [Energy has been measured by the ionization method.]

Let the wave-length of the primary radiation be λ and that of the K fluorescent characteristic radiation of the substance bromine be λ_K. Then when λ is greater than λ_K, for a given absorption of the primary radiation the same number of electrons is emitted by bromine (L, M, N, etc., electrons) as by air (K, L, M, N, etc., electrons). When λ is just less than λ_K the additional K absorption in bromine is accompanied by the emission of only half as many electrons, that is, only half the K energy absorbed is re-emitted as energy of K corpuscular radiation. But as λ becomes smaller, an increasing fraction of the energy absorbed (K absorption) goes into the K corpuscular radiation. And the rate of change indicates that with a primary radiation of very small wave-length practically the whole of the K absorption would be accounted for by the K corpuscular radiation alone. There is in addition very strong indirect evidence that when λ is very small nearly all the energy absorbed is re-emitted as corpuscular radiation, for whatever the absorbing substance under these conditions the energy of this corpuscular radiation is approximately a constant.

Also when the wave-length λ of the primary radiation is just less than that λ_K of the fluorescent X-radiation, of the energy specially absorbed in association with the emission of the fluorescent radiation (K absorption) nearly 0·5 is re-emitted as fluorescent X-radiation. As λ becomes less the fraction diminishes almost proportionately until for primary radiation of very small wave-length the energy of the fluorescent radiation becomes a very small fraction of the energy of the primary radiation specially absorbed. It is evident at once that the energy of K fluorescent X-radiation from bromine is approximately complementary to that of the K corpuscular radiation. From the study of these relations we find:—

The energy of primary radiation absorbed to expel one K electron is greater that that required to expel one L electron, by a quantum of the K radiation. We then naturally consider the K electron to be nearer the centre of the atom, and conclude that in general the electrons associated with the higher frequency X-radiations are nearer to the centre than those associated with lower frequency radiations. Thus J, K, L and M electrons are arranged in this order from the centre outwards.

As both K and L electrons leave the atom with the same velocity —this has been recently demonstrated—we may express the fact in this way: A quantum of K radiation is the energy required to move an electron from the position and state of a K electron to the position and state of an L electron; by analogy it may be concluded that a quantum of L radiation is the energy required to move an electron from the position and state of an L electron to the position and state of an M electron.

We also find a quantum of K radiation is emitted for each K electron ejected, as shown in the Table:—

Approximate wave-length of primary X-radiation.	Number of quanta of K fluorescent radiation emitted per K electron ejected.
cm.	
$0{\cdot}87 \times 10^{-8}$	1·09
$0{\cdot}71 \times 10^{-8}$	0·95
$0{\cdot}56 \times 10^{-8}$	0·85
$0{\cdot}49 \times 10^{-8}$	0·81
$0{\cdot}35 \times 10^{-8}$	0·90

With primary radiation of just shorter wave-length than that of the K radiation, for each electron emitted there is absorbed the energy of a quantum of K radiation as well as that carried away by the electron; therefore *each absorbing atom has absorbed about two quanta of primary radiation.* As λ decreases, the energy absorbed by each atom is that of one quantum of primary radiation (going to an electron) plus the energy of one quantum of secondary characteristic radiation (going to the fluorescent characteristic radiation). This varies from the energy of two quanta to that of one quantum of the primary radiation as the wave-length diminishes. Absorption, therefore, is not in whole quanta.

There is thus again no evidence of an entity or indivisible quantum of radiation even in the phenomena supposed to strongly support such a theory. The reason why it has usually been supposed that energy is absorbed in quanta is that the second part is frequently small; it may, however, be any fraction of another quantum of primary radiation.

We have shown that the emission of each electron of the corpuscular radiation is associated with the emission of a quantum of the corresponding fluorescent X-radiation, but there is conclusive evidence that the electron, after emission, takes no part in the phenomenon of fluorescence—that is, the emission of characteristic radiation is entirely independent of the electrons after their emission. We must, therefore, conclude that the atom which emits an electron emits also a fluorescent radiation characteristic of the atom itself. The observed relations lead to the following considerations:—

When a K electron is hurled out of the atom it is possible that for stability another electron must fall into the position and state of the K electron. If it falls from the position and state of an L electron, the excess energy, which is that of a quantum of K radiation, must be re-emitted. The emission of a quantum of K radiation for each electron expelled we have found to be an experimental fact. By analogy the energy of a quantum of L radiation will presumably be emitted when an electron falls from the position and state of a lower frequency electron to the position and state of the displaced L electron.

It is possible that there are not electrons of every series in every atom of the same substance, or, if there are, they may not be in the best position to enable them to replace an electron of an adjacent higher series. If, for instance, a K electron were ejected from an atom which did not contain an L electron, or in which the L electron was situated on the other side of the centre, its place might be taken by an M electron, with the result that the energy emitted as characteristic radiation would be equal to that of one quantum of K radiation + one quantum of L radiation. This would be equal to one quantum of radiation of slightly greater frequency than what we have previously regarded as the principal component of the K radiation, and might account for a neighbouring higher frequency spectral line in the K radiation, as was suggested by Kossel.

Whatever the exact process of emission of the fluorescent characteristic radiations, the experiments described above lead to the conclusion that a characteristic radiation (of series K, say) is probably emitted when an outer electron falls into the position of a displaced K electron, the energy previously absorbed in displacing the K electron in excess of that carried away as kinetic energy being now re-emitted as a quantum of K radiation, or, what seems more probable, as a quantum of K radiation accompanied by quanta of lower frequency radiations, due to successive falls of electrons between the outer rings.

In conclusion, it seems absolutely certain that electric radiation can be and is both emitted and absorbed in quantities which are not multiples of any particular unit; that radiation even by individual electrons is in many cases a continuous phenomenon. But that in certain processes within the atom—probably when an electron falls from one ring to another—the phenomenon takes place in a very definite manner, involving the radiation of a definite amount of energy which is proportional to the frequency of vibration.

Though there are many problems of absorption and radiation still awaiting solution, as indeed there always must be, the difficulties seem to be definitely shifting from the radiation itself to the structure of the atom and to processes taking place within the atom. The evidence obtained from experiments on X-rays indicates that there is no such thing as a quantum of radiation in the sense in which it has been frequently used. There is no entity apart from the atom. The structure of the atom only permits of the true absorption of a quantum of energy, and when the original configuration is reached it re-emits that quantum.

It may be briefly added that the evidence suggests that mere collision of electrons with an anti-kathode is not of sufficiently sudden a nature or sufficiently violent to result in the generation of X-rays as ordinarily detected. The probability is rather that the function of the kathode particle is to displace electrons from atoms in the anti-kathode, and thus to produce vibrations in the atoms which result in the emission of the primary X-radiation. Some of these vibrations may be due to the fall of electrons from definite positions—outer rings—in which case characteristic radiations are emitted; or they may be due to falls from any other positions, in which case the radiation is not characteristic of the anti-kathode. Such a mixture of radiations has been observed.

[C. G. B.]

WEEKLY EVENING MEETING,

Friday, June 2, 1916.

Sir James Crichton Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Lieut. Paul Hyacinthe Loyson.

La France dans l'Histoire comme Champion du Droit.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 14, 1916.

COLONEL E. H. HILLS, C.M.G., R.E. D.SC. F.R.S., Secretary and Vice-President, in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.SC. Pres.R.S. M.R.I.

The Genesis and Absorption of X-Rays.

A YEAR ago I brought before your notice the subject of the production and properties of very soft Röntgen rays. The rays which emerge from an ordinary X-ray tube are produced by cathode rays which have fallen through a potential difference of many thousand volts, while the potential difference for the rays I referred to last year, and to which I wish to recur this evening, varies from 20 to 5,000 volts. I will begin by considering the method of producing the rays and also of the voltage from which they derive their energy. The source of the rays was a tungsten spiral of the kind used in Coobridge tubes, and I am indebted to Mr. Coobridge for his kindness in presenting me with a supply of these spirals. These spirals are heated to incandescence by an electric current, and give out streams of cathode rays, which fall through the voltage between the hot spiral and an anode and acquire energy proportional to the voltage. To get rays of a definite type it is necessary to have means of producing a constant potential difference. One method I have used for doing this is a battery of a large number of small storage cells; another method, which, except for very small voltages, is more convenient and more flexible, is to use a high-tension dynamo designed so that the potential when the speed is constant is not subject to appreciable variation. The one I have used was supplied by Messrs. Evershed and Vignolles, it gives steady voltages from 150 to 5,000 volts, and has proved very satisfactory.

I now pass on to consider the method of studying the character of the Röntgen radiation produced at different voltages. Two methods have been employed: (1) the absorption of the radiation by thin layers of different substances, such as tin, silver, gold, aluminium, celluloid, has been measured; (2) in this method the quantity determined was the velocity of the cathode particles which are emitted by a plate of metal when exposed in a vacuum to the radiation.

When Röntgen rays fall on a plate of metal it emits cathode particles and the speed of these particles, while independent of the kind of metal, increases with the hardness of the rays; indeed there is considerable evidence to show that the energy of the particles is directly proportional to the frequency of the rays, so that a measurement of this energy specifies in a very convenient way the nature of the Röntgen rays.

The apparatus I used for carrying out the first method is represented in Fig. 1. A is the hot tungsten cathode; G an anode made of gauze; T the target on which the rays strike. These rays pass through the diaphragm D into the ionizing chamber I, which is a brass cylinder about 40 centimetres long and 10 in diameter; this is filled with parallel gauze discs, the adjacent ones of which are insulated from each other, while alternate ones are connected together. One set of discs is connected with one quadrant of a Dolezalck electrometer, the other set to one of the poles of a battery of about 600 volts; the other pole of this battery is put to earth. If the quadrant of the Dolezalck is originally put to earth, then on making the connection the current flowing into the quadrant will measure the ionization in I, and therefore the energy and the rays. To determine the character of the rays, screens of metal foil are inserted between the diaphragm and the ionizing chamber by means of the arrangement T. This is a holder soldered on to the tube just behind the diaphragm, inside this a metal plate pierced with a series of holes, one of which is empty

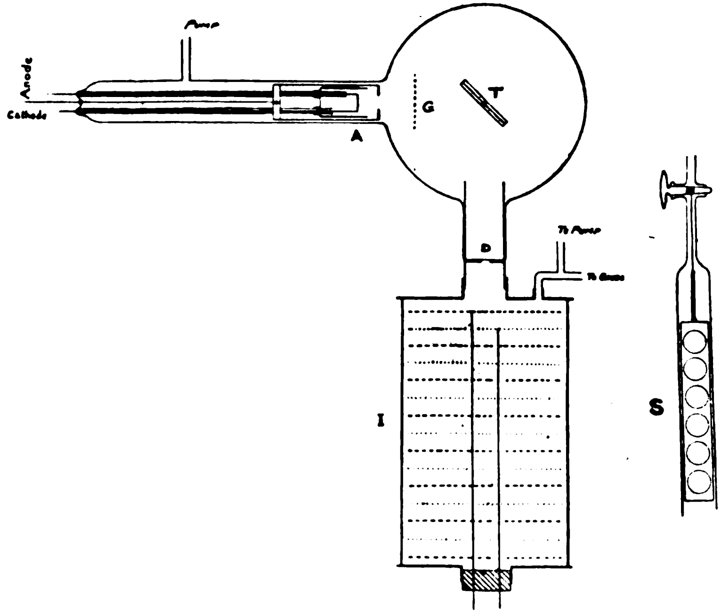

FIG. 1.

and the other filled with the different specimens of foil; this plate is raised or lowered by the windlass W, which winds up a piece of silk fastened to the plate. In some experiments the diaphragm itself was used as a target, the cathode being put opposite the diaphragm. This foil of various kinds was fixed in front of the diaphragm; and in this way the impact of cathode rays against different materials could be investigated.

In this method the energy of the rays is measured by the inonization they produce in the chamber. To absorb all the radiation it is necessary to have the gas in this chamber at a higher pressure than is admissible in the tube where the cathode rays are produced; the chambers must therefore be separated by a diaphragm which can stand a difference in pressure of some millimetres of mercury and yet be thin enough to allow the rays to pass through them. As these rays are extremely easily absorbed this necessitates the use of diaphragms which are less than a thousandth of a millimetre in thickness. Considerable difficulty was found in getting diaphragms to satisfy these conditions; the method finally adopted is due to my assistant, Mr. Everett, and consists in putting a drop of a solution of celluloid in amyl-acetate on the top of mercury in a shallow vessel. As the solution evaporates a thin film of celluloid is found on the mercury, and this is allowed to get thoroughly dry before being removed. By this device the boundary is free while the film is drying; if it is fixed the film tears from the shrinkage which takes place while it is drying.

I will now consider some of the results obtained by this method, and will first take the change in the absorption of the rays by a layer of aluminium, gold-leaf, or silver as the energy of the cathode rays producing the Röntgen radiation is gradually increased. We know that, speaking generally, hard rays correspond to large energy, soft rays to small, and it might be thought that as we increased the energy we should get a fairly constant rate of increase in the proportion of the radiation getting through the foil. When we try the experiment we find, however, that the increase in the transmissibility of the Röntgen rays with the energy of the parent cathode rays is not of this uniform character. The graph representing the relation between the absorption of the Röntgen rays and the energy of the cathode rays much more nearly resembles a series of steps with flat places between them than a uniform slope. Great changes in the energy of the cathode rays may give rise to very small ones in the absorption of the Röntgen rays they produce, as the absorption fixes the character of the Röntgen rays; this shows that this has altered but little in spite of the large alteration in the energy of the cathode rays. This is illustrated by the following Table, which gives the absorption produced by the same pieces of foil when the voltage of the cathode rays is steadily increased; the numbers given represent the ratio of the energy in the rays before and after they pass through the foil. The target struck by the cathode rays was silver in the first Table, aluminium in the second.

TABLE I.

Voltage of Cathode Rays	Absorption				
	Gold Leaf	Aluminium Foil	Copper Foil	Silver Foil	Celluloid
700	10	60	36	15	6
800	10	70	40	15	6·6
900	10	50	36	13·5	7·2
1100	11	54	42	16	7
1250	10·8	51	36	13	7·2
1500	10	30	34	12·4	7
2000	10	20	26	10	6
2500	8	15	22	8	6
3000	8·1	10	17	7	6·5
3500	4·4	4·5	7·8	3·7	4
4000	4	3·6	6	3·1	3

TABLE II.

Voltage of Cathode Rays	Gold Leaf	Aluminium Foil	Copper Foil	Silver	Celluloid
700	10	6	11	8	9
800	8	5	8	4·2	8
900	8	4	5	3·9	7
1000	6	3·3	4·5	3·1	8
1100	6	3	4	2·5	7
1300	4·8	..	4	..	5·6
1400	3	3·2	..	..	..
1500	4·8	..	3·7	1·85	5·6
2000	3	2·7	3·3	2	4·4
2200	..	1·9	..	..	..
2500	1·8	..	3·2	1·4	2·6
3000	1·8	1·7	2·4	1·2	1·7
3500	..	..	..	..	..
4000	1·75	1·6	2·6	1·1	1·9

Many other materials, such as lead, platinum, iron, quartz; and carbon, have been tried as targets; but whatever might be the nature of the target all the results showed the feature to which I have just drawn attention, the changes in the penetrating power of the Röntgen rays taking place for the most part by comparatively abrupt jumps at particular values of the energy of the cathode rays, while between these values the changes in the character of the

Röntgen rays produced by changes in the energy of the cathode rays are but slight.

This result has, I think, a very important bearing on the origin of Röntgen rays. If these rays were in the main produced directly by the stoppage of cathode rays, we should expect the quality of the Röntgen rays to vary *pari passu* with the energy of the cathode rays. We have seen that this is not the case; the bulk of the rays seem to keep of approximately constant quality, in spite of wide variations in the energy of the cathode rays. This result seems to me to prove that the bulk of the Röntgen rays are produced indirectly and not directly by the cathode rays. I take the view that the most important part played by the cathode rays for this purpose is to ionize some of the atoms in the target, expelling electrons from these atoms, and that it is the subsequent return of electrons to ionized atoms which gives rise to the greater part of the Röntgen radiation; as long as

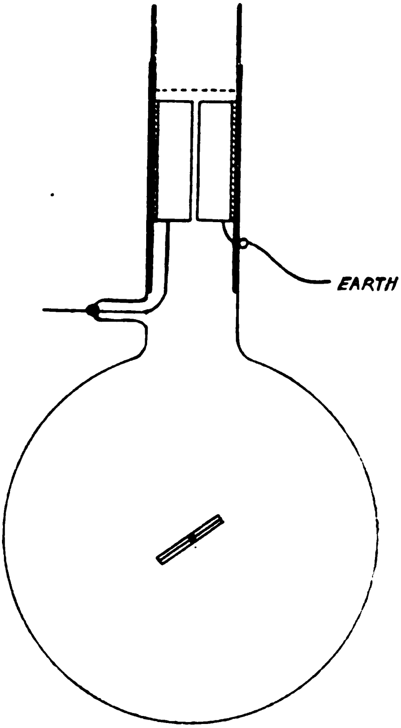

Fig. 2.

the type of ionization remains the same this process is independent of the energy in the cathode rays which produced the ionization. So that as long as a cathode ray has energy enough to ionize an atom in the target, it will produce Röntgen rays of the same type as those due to cathode rays with greater energy; it is only when the energy increases to such an extent that the cathode ray is able to produce a new type of ionization—to separate, for example, an electron from a ring of electrons nearer the centre of the atom, and not merely from the outside ring—that we should expect to find marked changes in the quality of the Röntgen radiation. When, however, the energy in the cathode ray reaches a critical value of this kind there will be a rapid change in the type of the Röntgen radiation. We should expect that these critical values would depend upon the material of which the target is made, and a reference to Tables I. and II. indicates that while the most marked change with a silver target takes place between 3000 and 3500 volts, with an aluminium target the change takes place at a smaller voltage, between 2000 and 2500. The type of radiation given out on this view is a "characteristic" radiation of the material of which the target is made. The voltages at which rapid changes in the absorption of the rays occur are those which are necessary to excite the various types of "characteristic radiation."

I have made a considerable number of experiments on the character of the Röntgen rays emitted by different substances when bombarded by cathode rays of different velocity, measuring the absorption of the rays produced by different numbers of layers of aluminium foil. The absorption produced by the first layer is always very much greater than that produced by a second layer behind it, showing that the main part of the radiation is so soft as to be completely absorbed by one layer of the foil; when this very soft radiation is eliminated constant values of the absorption are obtained for considerable variation in the velocities of the cathode rays. It is the absorption by one layer of aluminium foil after the soft rays have been abstracted which is given in the following table; the rays were generated by cathode rays striking against celluloid, lead and aluminium targets respectively.

Energy of Cathode Rays expressed in Volts	Absorption by 1 Thickness of Aluminium Foil Target		
	Celluloid	Aluminium	Lead
800	4	1·6	2
1000	3·2	1·45	1·5
1200	3	1·3	1·4
1500	2·8	1·4	1·4
2000	2·5	1·2	1·2

There is thus considerable difference between the quality of the rays from the celluloid target and those from the lead and aluminium ones. There is little difference, however, between those from lead and aluminium for the higher voltages, nor did I find any very marked difference in the quality of the Röntgen rays produced when the same number of cathode rays of the same speed struck lead and aluminium targets respectively.

With rays of this very soft type it is only a very thin layer close to the surface of the target which is operative, and so the cleanliness of the surface is a matter of great importance. If different substances give out characteristic radiation of the kind produced by these slowly moving cathodes, we should expect that the absorption by these substances would show anomalies when the rays passing through them approached in type to their characteristic radiation, and that the ratio of the absorption of different substances would show considerable variation. This was a very marked feature in these experiments. If, for example, we compare the absorption of the rays by gold and aluminium, the absorption by aluminium for very soft rays—say for those corresponding to voltages of 400 or 500—is in comparison with gold many times greater than for rays corresponding to more than 1500, so much so that a piece of aluminium foil which does not absorb nearly so many of these harder rays as a piece of gold-leaf may absorb three or four times as many of the softer rays. The following examples show how marked is this effect; the plates of the different metals were not of the same thickness, so that the results do not give the absolute values of the absorption but only the variation of the absorption with the voltage; the target was silver.

Voltage of Cathode Rays	Absorption			
	Gold	Aluminium	Copper	Silver
300	7	10	40	4
550	7	40	16	6
1000	3·7	1·8	5	3

Thus with the highest voltage the absorption of the gold is twice that of the aluminium, while with the lowest it is only about one-sixth; this points to the existence of a soft aluminium characteristic radiation, and also probably to one for copper.

The next question I shall consider is the variation in the energy of the Röntgen radiation with that of the cathode rays which produce them. We must remember that in the method we are considering at present the Röntgen rays have to pass through a thin film of celluloid before their energy is measured, so that any energy absorbed in the film would escape detection. In my experiments the Röntgen rays first became appreciable when the voltage on the cathode rays was about 150 volts—the limit depends on the thickness of the film, the magnitude of the stream of cathode rays, and the sensitiveness of the electrometer—it then increased comparatively slowly at first, but very rapidly when the voltage rose above a certain value. This value was in the neighbourhood of 350 to 400; it seemed to depend somewhat on the nature of the target. The energy in the Röntgen rays at different voltages, the number of cathode rays being kept constant, is shown below; the target was made of celluloid:—

Voltage	Energy in Rontgen Rays	Voltage	Energy in Rontgen Rays
150	1	350	45
200	6	400	180
250	15	450	360
300	30		

The energy in the Röntgen rays does not continue to increase at this rate with the potential; it attains a maximum at a certain voltage, and then, for a time at least, diminishes as the voltage increases. The potential for maximum voltage is subject to considerable variation; the cause for this I have not yet had time to investigate; the thickness of the film, the pressure in the cathode-ray tube seem both to affect this potential.

I now proceed to considering the other method of investigating the quality of the rays, i.e. by the velocity of the electrons ejected when the rays fall on a metal plate. To apply this method, the ionizing chamber I used in the previous one was replaced by a sphere silvered on the inside. At the centre of the sphere there was an insulated metal plate connected with one pair of quadrants of a Dolezalek electrometer. When the rays fall on this plate electrons are given off, and the plate acquires a positive charge which is registered by the electrometer. If the energy with which the electrons are ejected is measured by V volts, then if the silvered surface of the sphere is made negative to the plate by V volts or more, the electrons cannot escape from the plate and the electrometer will not be affected by the incidence of the rays. In this method it is desirable to have the pressure of the gas in the bulb as low as possible, so that no diaphragm between the cathode ray tube is necessary, and thus the rays which may be absorbed by the diaphragm and escape detection by the first method will be registered by this. On the other hand, since the diaphragm prevents charges of electricity which would vitiate the measurements from passing from the cathode ray tube to the parts connected with the electrometer, it is necessary to introduce some means for stopping these charges. This was effected by having in the tube, between the cathode ray tube and the silvered sphere, two half-cylinders insulated from each other and connected with the poles of a 600-volt battery; everything going from one tube to the other had to pass between these cylinders, and if it were electrified would be driven on to one or the other. The experiments with this method, which are not yet completed, give results which agree with those obtained by the other. They show that a potential difference of a few volts between the silvered sphere and the metal plate is sufficient to stop the vast majority of the electrons, even though the cathode rays had fallen through many hundred volts, and that as before the character of the Röntgen rays only changed slowly with the energy of the cathode rays. Thus when 1000 volts cathode rays fall on a target the Röntgen rays produced are for the most part no harder than might be produced by 100 volt rays. The proportion of rays corresponding to anything approaching 1000 volts is so small that I have not yet succeeded in finding by this method any trace of their existence. I hope, however, in less strenuous times, when research, which can now only be an occasional relaxation, may become again one's main occupation, to carry out more elaborate experiments on this and other points of interest in connection with these rays.

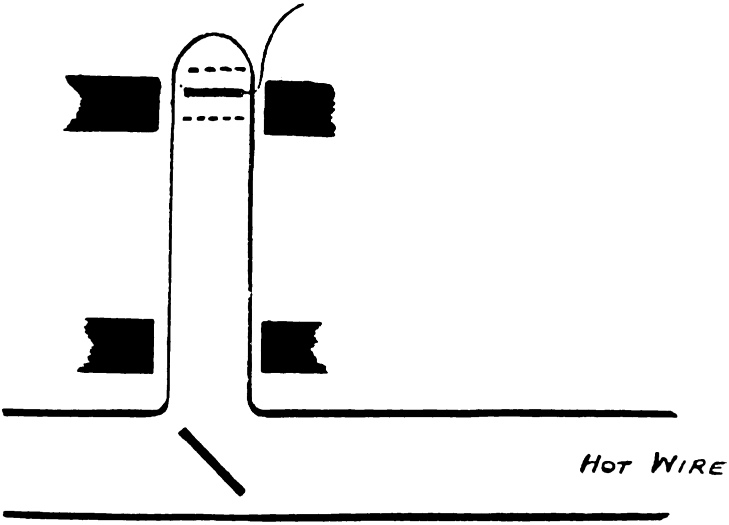

Fig. 3.

[J. J. T.]

HODGKINS TRUST.

ESSAY BY PROFESSOR HENRY E. ARMSTRONG.

Low-Temperature Research at the Royal Institution, 1908–1916.

THIS third instalment of the record, under the terms of the Hodgkins Trust, of the original inquiries pursued at the Royal Institution follows in natural sequence of subject the two earlier notices,* the work being for the most part the direct outcome and an integration of that done during the previous periods.

In my former account, I have termed the first septennate the *Hydrogen Period* but have pointed out that the Dewar vacuum vessel was first brought into use during the period. The second septennate was termed the *Charcoal Vacuum Period.* In the interval, not only has the *Coming of Age of the Vacuum Vessel* been celebrated but the Charcoal Vacuum process has been quietly appropriated, the world over, by scientific and industrial workers, as affording an otherwise unapproachable means of producing the highest vacua by effecting the all but complete removal of its gaseous contents from any vessel.

Although the liquefaction of most of the least coercible gases was achieved without the aid of the vacuum vessel, that of Hydrogen was not realized until a vessel of special type was made use of: probably the critical importance of the device is not recognized; it is doubtful whether the liquefaction of Helium could ever have been effected without the combined use of the vacuum vessel and the charcoal vacuum. The manipulation of liquid air and of liquid hydrogen and their use as refrigerating agents in experimental inquiries and on a large scale would have been impossible without it. The value of the vacuum or Thermos flask is now fully known to the public through usage, though probably, as a rule, without any appreciation either of the principle upon which it is founded or of the history of its development.

The use made of high vacua in the recent transcendental inquiries into the constitution of the atom is known only to the few and cannot be generally appreciated; the advance made is already so extraordinary and so unexpected that it may fairly be anticipated that this will outweigh in importance all previous applications.

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* Summaries of the work carried on with the aid of the Hodgkins Trust are, by the authority of the Managers, incorporated in the "Proceedings of the Royal Institution" every seven years.

Low Temperature Research at the Royal Institution, 1893–1900. Miss Agnes M. Clerke. "Proceedings," xvi. p. 699.

Ditto, 1900–1907. Prof. Henry E. Armstrong. "Proceedings," xix. p. 354.

The period being that during which the value of extreme exhaustion has been brought into such special prominence, as the Institution was the fountain-head of the means of attaining the highest vacua by means of refrigerated charcoal, I propose to term it the *High Vacuum Period.*

THE FULLERIAN PROFESSORSHIP.

The period is also noteworthy as that in which Sir James Dewar's tenure of the Fullerian Professorship of Chemistry has exceeded in length that of Faraday. Faraday was elected in 1833, Mr. Fuller having founded the Professorship in the earlier part of the year; the salary attached to the post was only about 100*l.* a year. Faraday was appointed for life, with the privilege of giving no lectures; he held the office until 1867; he was succeeded by Professor Odling (1868–73) and Dr. J. H. Gladstone (1874–77). Sir James Dewar came into office in 1877.

The work done by Faraday has been described and discussed on many occasions. Though mainly physical and entirely so during the later period of his activity, during the earlier period it was largely chemical; probably this side of his activity and the formative influence of the experience on his character is far too little appreciated; it was fraught with the most important consequences. The foundation stone of the Dyestuff Industry, which has loomed so large in the public eye of late, was laid by his discovery in 1824 of *bicarburet of hydrogen* or *benzol* (now *benzene*), as it was termed later on, by Liebig, when Mitscherlich, in 1833, prepared it from benzoic acid.*

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* In the latest edition of his "Coal-Tar and Ammonia" (5th edition, 1916, i. p. 223), Lunge makes the claim, that Faraday was anticipated in the discovery of benzene, in the following terms:—

"It is usually stated that benzene was discovered in 1825 by Faraday in the liquid separating from condensed oil gas, but Schelenz (Z. angew. Chem., 1908, p. 2577) has shown that the compound which we now term 'benzol' or more recently 'benzene' had been discovered in coal-tar forty years before Faraday, in the year 1825, reported 'On New Compounds of Carbon and Hydrogen and on certain other Products obtained during the Decomposition of Oil by Heat.' In Macquer-Leonhardi's 'Chymisches Wörterbuch,' published at Leipzig in 1783, vol. i. pp. 86, 709, a passage is found which in English translation runs as follows:—

"From coal we can obtain by distillation a nearly tasteless water, a brownish yellow, disagreeably balsamically smelling oil, and another empyreumatic tougher oil which can be rectified into naphthol (that is, mountain oil, in the old classic sense) like petroleum. In 1874 was published at Leipzig 'Demadys Laborant,' translated into German by Samuel Hahnemann, in which the distillation of coal on the large scale is described, enumerating the various fractions, one of which can be at once put into bottles in which a space of at least three or four fingers' breadth must be left. Even before this, in Caspar Neumann's 'Praelectiones Chemicae,' published at Schneeberg in

James Dewar

In 1826 he made another discovery of cardinal importance to the same industry, that of *sulphonaphthalic acid—naphthalenesulphonic acid* we now term it. Though chronicled by Bence Jones in his

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1740, it is stated that 'fossile carbons' on distillation yield a little water and 'a sharply tasting spirit,' which must have been benzol."

This disingenuous statement does not appear in the previous edition. Those who are acquainted with the behaviour of coal when distilled are well aware that the "spirituous" product is a very complex mixture from which it is by no means an easy matter to separate benzene. The quotations given obviously do not contain the faintest approach to an anticipation of Faraday's discovery. In his paper read before the Royal Society on June 16, 1825, he describes how he isolated benzene, by cooling the liquid with ice and salt, as a crystalline solid, of definite melting point; the liquefied substance boiled at a definite temperature. He determined the density of the liquid and also of its vapour; finally he made an exact analysis of the substance and deduced the composition of the hydrocarbon correctly, the formula he gave to it being that now used after translation into terms of the present-day molecular theory. He made a full chemical examination of the hydrocarbon and he nitrated it, though he did not isolate nitrobenzene; he even sulphonated it. Taking into account his training and the state of knowledge at the time, it is one of the most remarkable pieces of early work in organic chemistry with which we are acquainted. Having worked with a material practically identical with that used by Faraday, I can appreciate the extraordinary skill he displayed in making his discovery. It may be mentioned that the gaseous hydrocarbon noticed by Faraday accompanying the benzene is not, as is often stated, the hydrocarbon butylene, C_4H_8 but, in all probability, crotonylene C_4H_6 (cf. Chem. Soc. Journ., 1886, p. 74).

Another German, Prof. Otto N. Witt, in a communication to the Berlin Chemical Society made shortly before his death (Ber., 1915, p. 743), by implication rather seeks also to detract from the originality of Faraday's work with naphthalene by attributing to Brande the discovery that when the hydrocarbon is dissolved in sulphuric acid it gives "a soluble substance of acid nature." This statement is not in accordance with what Brande says.

The account of his work appeared in the Quarterly Journal of Science (1820, pp. 21, 181) under the title "On a Substance produced during the Distillation of Coal Tar." He was not even aware, at the time, what particular compound he was dealing with. He speaks of it only as a white crystalling substance and states that:—

"In sulphuric acid, when aided by heat, it dissolves in considerable abundance, forming a deep violet-coloured solution which bears diluting with water without decomposition."

Faraday, early in his memoir (Phil. Trans., 1826, p. 140), refers to Brande's work and reproduces this statement. But, at the outset, after speaking of his previous work on benzene referred to above, he goes on to state that he was led to study the action of sulphuric acid on "analogous" bodies—naphthalene among others—because he "had occasion to notice the peculiar action exerted on certain of these compounds by sulphuric acid. During my attempts to ascertain more minutely the general nature of this action, I was led to suspect the occasional combination of the hydrocarbonaceous matter with the acid and even its entrance into the constitution of the salt which the acid afterwards formed with bases."

Obviously, he was in possession already of a perfect method of reasoning. For its time, the work is altogether remarkable on account of its completeness. He recognized the existence of "a peculiar compound of sulphuric acid with the elements of the naphthalene which possesses acid properties." He

"Life and Letters of Faraday," these discoveries are not directly indexed in the volume.

Again, by his immortal electrolytic studies, the full philosophical value of which is only now being made apparent, as well as by those on the liquefaction of gases, Faraday laid the foundations of other great chemical industries.

No one else has done so much to establish the inseparability of chemistry and physics. Chemistry has ever since recognized the existence and strength of the bond of union; physics unfortunately has not reached the same breadth of development and physicists have paid but slight attention to the constitution of molecules grosser than atoms and to chemical properties; none has yet displayed any feeling for the higher walks of chemistry—for the organic side; latterly, however, physicists have taken the atoms under their charge and have made their dissection, the study of atomic constitution, their concern—though mainly in the mind's eye: it is possible, therefore, that there may be an approach of the two disciplines, although over a bridge of transcendentalism. It will always be a noteworthy fact, that Faraday gained his scientific training through self-education and through chemical work, in the first place; but Faraday was not a mathematician and his mind, if mathematical, was never under the constraint of formulæ; he was ever free to amend his views. Early in his life he defined his own attitude and character unconsciously in saying:—

"The philosopher should be a man willing to listen to every suggestion but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respector of persons but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature."

If such be the attributes of the philosopher, the development of science must ever remain in the hands of an aristocracy; Science proper cannot well be made a democratic occupation, any more than can Music or Art; at most we shall be able to enlarge the circle of admirers and of those who can find pleasure in contemplating its wonders; the scientific discoverer, in point of fact, is to be reckoned among artists. The value of an Aristocracy of Talent has been dwelt

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not only prepared a great variety of salts of the acid but analysed and established the composition of the barium salt correctly; he also showed that "half the sulphuric acid was neutralised by the hydrocarbon." He closes by stating that "Mr. Hennell, of Apothecaries' Hall, had made known to him, before he had completed the analysis of the acid and its salt, this very curious and interesting fact in chemical affinity as occurring in some other compounds of sulphuric acid and hydrocarbon." We must clearly credit Faraday with the discovery of the *sulphonic acids*, a class of compounds of extraordinary technical importance.

upon in the past, especially by Carlyle; at no time have his words of warning been of greater import than at the present period of our dire distress and peril. Of late years Science has fallen distinctly from its high estate and we have not been sufficiently mindful of the need of preserving it as an aristocracy—the work of artificers has been too often appraised as that of artists; there have been too many "false gods" among us and unreasoned worship of formulæ; a constrained mathematical habit of mind has been too prevalent: unless care be taken we may easily lose the freedom that was so hard to win and again fall a prey to a dogmatic "ecclesiasticism." Signs are not wanting. "The Aristocracy of Venice hath admitted so many abuses that the period of its duration seems to approach," wrote Swift; we must be on our guard, so that this may not be said of the Aristocracy of Science.

The spirit of the Royal Institution, the spirit of Davy and Faraday, is in no way sufficiently appreciated by the public; it is one, however, that should be made known more widely, so that it may be upheld and preserved at all costs. Feeling this as I do most strongly, I have endeavoured to make this essay, in some measure, a broad survey of the work accomplished during the tenure of the present holder of the Fullerian Professorship.

Sir James Dewar's inquiries, like those of his great predecessor, have been in part chemical, in part physical; they lie at the very opposite poles of the scale of temperature but have been mainly in borderland regions of the two sciences—hence their peculiar value. Those who have watched its progress and are conversant with the results agree in regarding the work as that of an artist gifted with exceptional powers of imagination and extraordinary command of technique.

Prior to his appointment in 1877, he had lectured on "The Temperature of the Sun and the Work of Sunlight" (March 7, 1873); also twice on the "Physiological Action of Light" (February 5, 1875; March 31, 1876). The second of these lectures is certainly the most memorable Friday evening lecture, in my recollection, he has delivered. I look back upon it as the most carefully considered and masterly discourse he has given; the model was then first displayed of the fascinating demonstrations to which Royal Institution audiences are grown so accustomed and now take as a matter of course—so much so, that few probably are aware how much is involved in their preparation.

Sir James Dewar's great predecessor was noted on account of his lucid and captivating style. Sir James will be remembered on account of his daring and of the infinite trouble he has taken to demonstrate the intricacies of modern low-temperature research and make the phenomena obvious to all: the degree of perfection to which he has brought his art is marvellous.

SPECTROSCOPIC HIGH TEMPERATURE STUDIES.

During the later seventies and several years afterwards many investigations were carried out, chiefly spectroscopic, to determine the behaviour of substances at high temperatures, so that the data might be obtained for the interpretation of the observations made by astronomers. In those days speculation was rife as to the nature of the elements; over and over again it was asserted that evidence had been obtained that they underwent dissociation—that is to say, reversible decomposition—at the transcendental temperatures prevailing in the sun and very hot stars. A variety of investigations were undertaken by Prof. Dewar in conjunction with Prof. Liveing, partly in the Institution laboratory and partly at Cambridge. The observations were of special value at the time on account of the fact that they were carried out with the most scrupulous care by men alive to the pitfalls which such work presents and always unwilling to draw conclusions until the facts were rigidly established.

Fortunately, the work of the two inquirers has been brought together in a volume of "Spectroscopic Studies," issued late in 1915 by the Cambridge University Press; unfortunately, no new comments are offered upon the papers (78) and no attempt is made to discuss the general bearing of the results in the light of modern discoveries; this is the more to be regretted, as the authors too often allow their observations to speak for themselves and refrain from all comment.

Such inquiry has almost lost interest for the present generation; the acuteness of the controversy which arose forty years or so ago is no longer realized, excepting by the few who watched the storm rage. Taken in conjunction with that of Huggins in particular, the work goes to show that however probable the view may be that the so-called elements may undergo decompositions at high temperatures, the evidence adduced could not be accepted as proof that such is the case.

The subject of "Spectroscopic Investigation" was first brought under notice by Prof. Dewar at a Friday evening, on June 6, 1879, in a lecture which was a summary of communications to the Royal Society during 1878–79 by Prof. Liveing and himself. This and other lectures involving the use of the spectroscope, which he gave at that period, will be remembered by all who were present, on account of the many brilliant and most beautiful demonstrations whereby phenomena were made visible to a large audience which, previously, only single observers had been in a position to witness. They were delivered at a time when the dynamo-electric machine was first coming into favour in place of the troublesome 50-cell Grove battery previously employed in giving lecture demonstrations; the systematic use of the electric-arc lamp in the lecture room was largely due to the example then set at the Institution, I believe.

The subject dealt with was that of the reversal of lines in the spectra of metals. It is well known that the foundation of solar and stellar spectroscopy, to which we owe our knowledge of the constitution of the sun and other hot stars, was laid by Kirchoff's discovery that bright lines in the spectra of certain metals could be reversed by passing their light through relatively cool vapours of the metals; the discovery gave the clue to the interpretation of the dark lines in the Solar Spectrum.

Bunsen and Kirchoff succeeded in reversing certain lines in the spectra of the alkali and alkaline-earth metals; the French observer Cornu extended the list to thalium, lead, silver, aluminium, magnesium, calcium, zinc and copper; Liveing and Dewar's investigations were of a far-reaching character and added much to our knowledge of the conditions under which reversal takes place. The inquiry was extended in a variety of directions and the spectroscope grew naturally to be a favoured instrument in the Institution laboratory and lecture room.

The introduction of powerful dynamo-electric machines into the laboratory, gifts of the President—the Duke of Northumberland—and of Mr. Siemens, gave the opportunity to study the chemical interactions taking place at the temperature of the electric arc. These were first referred to in a Friday evening lecture, on January 16, 1880, under the title "Investigations at High Temperatures."

The conclusion arrived at (by Plücker, Ångström and Thalén) that the so-called carbon spectrum is invariably associated with the formation of acetylene led to trials whether this gas could be extracted from the electric arc.

A long series of experiments were made in dry atmospheres, with purified carbon poles, which all led to the conclusion that hydrogen cyanide, HCN (and perhaps cyanogen, C_2N_2), was invariably formed and that it was impossible to deprive the carbons completely of combined hydrogen.

Hence the conclusion that "the inference drawn by Mr. Lockyer as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine could not be regarded as satisfactory, seeing that undoubtedly hydrogen was present in the carbon and in all probability nitrogen in the chlorine."

The subject was again dealt with in 1881 (Friday, June 10) in a lecture on the "Origin and Identity of Spectra." Stress was laid upon the imperfection of the evidence that carbon can take the state of vapour at the temperature of the electric arc. It appeared probable that the spectra attributed to carbon were more likely to be those given by compounds such as cyanogen and hydrogen cyanide, acetylene and carbonic oxide, all of which are formed under the conditions of practice.

In this connexion, attention may be called to the question I have raised, in my previous essay, as to the constitution of the black forms of carbon : it was there argued that probably the molecule is one of great complexity.* I would now add that it is in no way certain that any form of black "carbon" is an elemental substance and not a highly "condensed" hydrocarbon containing a very small atomic proportion of hydrogen. The practical impossibility of getting rid of hydrogen, on which Professor Dewar has laid such stress, is in favour of this view.

In the same lecture the multiple spectra of magnesium were considered and the conclusion arrived at that no satisfactory evidence of elementary dissociation could be deduced from the variations.

Finally, the apparent identity of spectral lines was discussed and a number of supposed coincidences shown to be non-existent, the final conclusion being that the few as yet unsolved coincidences either would yield to a higher dispersion or are merely accidental.

This lecture is reported at unusual length in the Proceedings.

The decay of interest in experimental spectroscopic inquiry of late years is due, probably, in large measure, to the fact that each instrumental improvement made possible by the use of higher dispersive power only serves to make any definite conclusion as to the coincidence of lines supposed to be characteristic of different elements more and more difficult to draw. Spectroscopy has now entered upon a new phase and has been made the subject of mathematical speculation, particularly since it has been held that even the atoms are of complex structure. In the mind of the physicist, the observed breakdown of materials such as thorium and uranium has added strength to the conviction that the so-called elements generally are composite materials, but the remarkable fact has to be taken into account that by no means known to us can we either assist or promote the breakdown. The speculations rife in earlier years as to the decomposition of the elements at high temperature, therefore, derive no support from recent researches and the one solid argument in favour of the view that the elements are compounds is still Mendeléeff's great generalization, *The Periodic Law*, so-called: the only explanation of the inter-relationship which this discloses has always been that some more or less close genetic connexion, such as that known to hold amongst carbon compounds, also exists among elements.

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* The problem of the constitution of the carbon molecule has acquired fresh interest, in the interval, owing to the discussion of the relationship between the forms of crystals and their molecular structure. Barlow and Pope have applied geometrical principles, whilst the Braggs have developed a refined method of analysis involving the use of X-rays. The structure assigned to the diamond is in accordance with chemists' views. The difference between the diamond and the other supposed forms of carbon is so great that proof that these are in reality hydrocarbons would be welcome.

In this connexion, special reference may be made to the lecture on "Recent Researches on Meteorites," delivered by Prof. Dewar on June 11, 1886. Attention was then directed to the considerable amounts of hydrogen and of the hydrocarbon methane, CH_4, given off not only from pieces of meteorite but also various graphites, including that prepared by the action of oxidizing agents on the cyanogen compounds in crude caustic soda, when heated in vacuo. The conditions were not those under which methane could well be formed from hydrogen and carbon.

For the present, as chemical methods of inquiry cannot be brought to bear, the subject must remain outside the ken of the chemist: he being unable to assist so long as a change in composition cannot be observed.

The lecture on April 1, 1887, on "Light as an Analytic Agent," also belongs to this section and may be referred to as a special illustration of the lecturer's genius as an experimenter. The observations described were made to ascertain whether the advance of an explosive wave could be detected by the shifting of lines in the spectrum of the flash corresponding to the displacements observed by astronomers in the case of stars, which have made it possible to deduce values for the rate of approach or recession of stars. The primary object was not realized. But on using a bent tube for the explosion and reflecting the light from either limb on to the slit of the spectroscope, so that the two images were superposed, it was observed that one of the two images of the lithium line was reversed. Such reversal is a proof that the temperature of the gas in the explosive wave does not reach its maximum immediately but is lower in the front of the wave.

Observations were described made with various metals and attention was specially drawn to the remarkable fact that, in the case of metals so slightly volatile as iron, nickel and cobalt, many lines are visible in the flash of the explosion, whilst few are seen when more volatile metals are present. No explanation is suggested. May it not be that the fact that these metals, in particular, are capable of forming volatile compounds, such as nickel carbonyl, $Ni(CO)_4$, is the cause of their special behaviour?

HIGH TEMPERATURE DISSOCIATION STUDIES.

The spectroscopic studies already discussed in part belong to this series. Two other Friday evening discourses, however, may be specially referred to in this connexion: that delivered on April 21, 1882, on the "Experimental Researches of Henri Ste. Claire Deville," to whom we owe the introduction of the term "dissociation" as connoting cases of chemical change which occur reversibly, to distinguish these from cases of irreversible decomposition; and that given on June 8, 1883, on "The Electric Arc and Chemical Synthesis." Of neither is an abstract to be found in the Proceedings; both were noticed, however, in the "Illustrated London News." In those days one of the most interesting items to not a few readers of this weekly journal was the column fairly regularly devoted to an account of the lectures delivered at the Royal Institution; these were written by the then librarian Mr. Vincent. Unfortunately science went out of fashion in the paper soon after this date.

The Rev. John Barlow had lectured at the Institution, on March 14, 1856, on Deville's discoveries relating to aluminium; and Faraday, on February 22, 1861, had dealt with his work on platinum. Professor Dewar, in his lecture, illustrated Deville's method of preparing aluminium from aluminium chloride by heating this with sodium and so withdrawing the chlorine; and spoke of his having produced both metals in large quantities. But in those days the two substances were still chemical curiosities: now they are manufactured on a huge scale, with the aid of water-power, by electrolysis. Sodium, it will be remembered, was one of Davy's greatest and most fundamental discoveries; and Faraday not only opened up the subject of electrolysis but is also to be counted as the father of the dynamo now used in generating electric currents.

Deville's methods of preparing boron and silicon were also illustrated in the lecture, as well as the improvements he had effected in the oxy-hydrogen blowpipe and in using this in melting platinum, on the large scale, in conjunction with Messrs. Johnson and Matthey.

Lastly, a fully illustrated demonstration was given of his Researches on Dissociation, to which, more than to any other part of his work, the great reputation of the French philosophical chemist is due.

The lecture was clearly an act of piety on the part of Professor Dewar, who may be reckoned as Deville's most devoted follower, the French chemist's influence being clearly traceable almost everywhere in his work.

The account given of the following lecture, in 1883, in the "Illustrated London News," may be reproduced almost verbally as showing how carefully the public were instructed by Mr. Vincent in the past; it is much to be wished that similar accounts could again be published. Excepting the "Engineer" no paper is now at the trouble to notice the lectures regularly.

"The lecturer began by exhibiting a brilliant electric arc produced by a new powerful gas engine and dynamo-electric machine. He pointed out the distinction between the positive and negative poles in the character of their light and their action upon the vapours of copper and silver, the effect of the reversal of the current resembling those of the voltaic battery. The distinctive action of different parts of the arc was shown by the spectroscope. The effect of the different temperatures of gases upon the electric charge was illustrated. The brilliant lines in the spectrum of iron were obliterated when hydrogen was introduced into the arc, although the iron was still present. Mr. De la Rue's photographs of the effects of the electric discharge upon the minute residue in rarefied tubes were exhibited and imitations of the Aurora Borealis were produced in tubes not so much exhausted. The polarization was as strikingly demonstrated in the electric arc as it is in a Grove's cell. The effects of the discharge at high and low pressure were next illustrated. Having exhibited the brilliant hydrocarbon gases, acetylene and olefiant gas, the chief illuminating constituents in ordinary coal gas, the Professor produced acetylene in the electric arc by the introduction of hydrogen; and in a similar manner he produced hydrocyanic acid gas by introducing nitrogen and hydrogen into the arc; he also exhibited the beautiful peach-coloured flame of cyanogen rendered more brilliant by the introduction of oxygen. Remarks were made on the important effects of the silent electric discharge in nature, in causing condensation instead of decomposition."

A noteworthy series of observations, foreshadowing the method now used in producing nitric acid from the atmosphere, was described in the lecture on January 16, 1880, showing the effects produced by subjecting air to the action of an electric arc within a cooled metallic vessel through which air was continuously circulated. The arc produced by a discharge of a high intensity, from a De Mériten's machine, between the poles of a Jablockoff candle from which the insulator had been removed, gave the best yield of nitrous compounds, a long Siemens arc being superior to the short. It was also shown to be advantageous to remove the products from the heated region by a rapid current of air, inferior results being obtained if the current were slow.

From radiation experiments referred to in the lecture, the temperature of the positive pole of the Siemens arc was estimated at about 6000° C.

A lecture which does not fit into the general scheme may be referred to here on account of the importance of the subject—"The Scientific Work of Joule"—and of the demonstration Prof. Dewar gave of the way in which Joule's determination of the value of the *mechanical equivalent of heat* was led up to by the early work of Count Rumford, the Founder of the Royal Institution, as well as later on by that of two of its Professors—Davy and Young—both, as the lecturer showed, "so near and yet so far from Joule."

An interesting feature of the lecture, which was delivered on January 24, 1895, is a memorable letter from Lord Playfair giving his recollections of Joule. This contains the following noteworthy passage:—

"When Joule first sent an account of his experiments to the Royal

Society, the paper was referred, among others, to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge but the discovery did not then recommend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend ; and I fear that then the Royal Society did not appreciate and publish the researches."

Unfortunately, even now the Royal Society has not improved its methods and still submits all communications to an often ineffective and sometimes intolerant criticism, even dictating alterations which are greatly resented by workers. The system is one which creates a maximum of friction and affords a minimum guarantee against the admission of matter which has no special value.

CHEMICAL ACTION OF LIGHT.

In the first of two lectures on the " Physiological Action of Light " which he delivered at the Institution on Friday, February 5, 1875, Mr. James Dewar described in detail a long series of refined experiments, made in conjunction with Dr. McKendrick, to determine the effect of light on the electromotive force of the retina and optic nerve. It was found to alter it to the extent of from 3 to 10 per cent. of the total amount of the natural current, the action on the compound eye of the Crustacea being the same as on the simple eye. The alteration of the electrical effect as the luminous intensity is varied appears to be closely in accordance with Fechner's law that the difference of our sensations is proportional to the logarithm of the luminous intensities by which they are provoked.

In a second lecture on the same subject, on Friday, March 31, 1876, Prof. Dewar showed that yellow light, which is known to have the greatest luminous effect, is also the most effective in producing the electrical alteration. A method was then demonstrated of working with live animals which involved merely a slight abrasion of the skin on the surface of the head and connexion of the electrodes with this and the eye surface. Warm and cold-blooded animals were shown to behave alike. Finally, it was proved that the current superadded or induced by the action of light is always in the one direction—being added to the primary current when the course is positive and the brain negative and subtracted from it when the course is negative and the brain positive. When two similar eyes are placed in reversed positions on the electrodes, the galvanometer deflection is to the right or left according to the eye affected.

On March 29, 1878, in the first Friday evening lecture delivered after his appointment as Fullerian Professor, Prof. Dewar dealt with the subject of electro-photometry on the chemical side, developing Edmond Becquerel's discovery (1839) that electric currents attend chemical interactions which are excited by light. The subject is in evident connexion with that of his two previous lectures. Experiments were described with cells having poles that were not acted upon chemically and containing a sensitive substance yielding only soluble products. Platinum poles and chlorous acid or chlorine peroxide were selected.

The amount and the direction of the current, in the case of chlorous acid, were found to be modified by the addition of certain salts and acids; electrical variations were produced resembling the effects observed when the eye is exposed to light. Salts such as potassium ferro- and ferri-cyanide and especially sodium nitroprusside are strongly affected. Uranium tartrate is one of the most active of organic compounds; a mixture of selenious and sulphurous acids in admixture with muriatic acid also yields strong currents when exposed to light. It is unfortunate that no further account of this work was published, the subject being one of great interest and importance.

It were time that the method developed by McKendrick and Dewar were again made use of and the phenomena of colour vision more fully investigated. The extension of the inquiry to colour-blind people is particularly desirable, in view of the work of Abney in recent years; also in relation to the question of the relation of colour to constitution, to which reference is made in my previous essay. It appears more than probable that the recognition and differentiation of colour is to be correlated with chemical changes in the eyes affecting systems which are in harmony with those of the colour-producing systems; the study of the electrical response of the eye to the light reflected from coloured substances from this point of view would be of peculiar interest.

The study of luminous effects has been continued, at intervals, in the Royal Institution laboratory with striking results. In the Friday evening lecture on June 8, 1888, Prof. Dewar drew attention to the phosphorescent phenomena attending the production of ozone. The appearances observed in high vacua have often puzzled spectroscopists, owing to the fact that the chemical changes which take place under such conditions have been unknown. Geissler was the first to discover that phosphorescence is sometimes set up in residual gases in vacuum tubes. Heat was found to negative such manifestations and several observers came to the conclusion that they were only produced by oxygen compounds. In the lecture, a most striking demonstration of the phenomenon was given by passing a discharge through a current of highly attenuated air streaming into a tall cylinder; the method is fully illustrated in the abstract of the lecture.

The appearance was only manifest in the case of oxygen compounds. In this lecture another striking demonstration was given showing that ozone could be produced by heating oxygen to about 1600° in a platinum tube. A diagram of this apparatus also will be found in the printed abstract.

The inquiry was resumed at a later date (p. 41).

THE LIQUEFACTION OF GASES.

Faraday's work on this subject ceased in the year 1845; he had carried the inquiry as far as the knowledge of the time permitted. It was not until Andrews, in 1864, had studied the behaviour of carbon dioxide under pressure at various temperatures and, as a consequence, had introduced a new theoretical conception—that of the critical temperature: the temperature above which liquefaction of a gas could not be effected, whatever the pressure—that the direction was indicated in which progress could be made. Between the close of 1877 and early in 1878, the scientific world learnt with the greatest surprise that two independent workers, M. Cailletet, a French ironmaster, and M. Pictet, a Swiss engineer, using very different methods, had both succeeded, it was affirmed, in liquefying not only oxygen but also hydrogen. The apparatus they used is figured and described under "Hydrogen," in a short article the writer contributed to the ninth edition of the "Encyclopædia Britannica" (vol. ix, p. 433).

On June 14, 1878, in a lecture on "The Liquefaction of Gases," Prof. Dewar gave a complete though condensed summary of the work done up to that date on the subject. He then described the remarkable achievements of MM. Cailletet and Pictet and gave demonstrations of the method adopted by the former with apparatus similar to that he had used: the method involved the sudden release of the pressure on a small quantity of highly compressed, cooled gas, liquefaction being due to the cooling caused by the sudden expansion; it was rendered obvious through the momentary formation of a mist in the tube.

The lecture ends on a note of optimism which was only justified after many years, very largely through work done in the Royal Institution. "The work of Faraday has been completed; every gas may be forced to appear as a liquid."

It is worth noting that the lecturer did not commit himself to saying—*has been* but merely *may be*. It was soon realized that, although correct as regards oxygen, Pictet's statement about hydrogen could not be accepted and that, under the conditions he developed, the liquefaction, let alone the solidification, of hydrogen was impossible. It is now known that the solid spoken of as solid hydrogen, projected, in Pictet's experiments, from the tube in which liquefaction was supposed to have taken place, was material "primed over" from the vessel in which the hydrogen was produced by heating sodium formate with caustic soda. I was informed in Geneva, a good many years ago, that when the apparatus was taken to pieces this tube was found to be choked with solid.

Prof. Dewar himself was the first to produce liquid and solid hydrogen.

The new field of work was first taken up by two Polish chemists, Wroblewski and Olzewski, who made notable progress.

But under the influence of the *genius loci*, fired by prophetic vision of the rich harvest in store, Prof. Dewar was early in the field. His singular ability as a manipulator and his engineering genius, combined with great imaginative power, have enabled him, in the cramped quarters of the Institution and with very inadequate resources, to overrun the field of inquiry to an extraordinary extent. The wide use that he has made of liquefied gases, for the purpose of studying the changes in the properties of matter brought about by lowering the temperature to the neighbourhood of the absolute zero, is the remarkable feature of his work, distinguishing it from that of all other inquirers.

His first original communication on the subject to an Institution audience was on June 13, 1884, when he gave an account of his own researches on liquefied gases and exhibited an apparatus more suitable for demonstration than the Cailletet apparatus. With this apparatus, all the phenomena brought to light by the Polish chemists above named were reproduced and the series of demonstrations inaugurated for which the Institution has since been famous. The apparatus is figured in the abstract.

Oxygen was liquefied in an inner tube surrounded by an outer tube filled with liquid ethylene; on reducing the pressure above the liquid to 25 mm., the temperature was reduced to about $-140°$ and the oxygen then liquefied at a pressure between 20 and 30 atmospheres.

The liquefaction of oxygen in this apparatus was also effected by using either solid carbon dioxide or liquid nitrous oxide as cooling agent. These gave the temperatures $-115°$ and $-125°$, both below the critical point of oxygen, then said to be $-113°$; it was necessary, however, to raise the pressure of the oxygen above the critical pressure of the gas (50 atmospheres) and liquefaction took place easily only at pressure of from 80 to 100 atmospheres.

The density of the liquefied gas was easily determined with the aid of the apparatus by noting the volume of the liquefied gas, then evaporating the liquid and measuring the volume of gas produced. A determination near the critical temperature of oxygen gave $d = 0 \cdot 65$.

In this lecture, the opinion was expressed that liquid methane would be the best cooling agent to use and Prof. Dewar pointed to the fact that he had suggested and used this substance a year in advance of MM. Cailletet and Wroblewski.

"Liquid Air and the Zero of Abolute Temperature" was the subject of the next discourse, on June 5, 1885. No abstract of this lecture was published.

The centenary of Faraday's birth was celebrated at the Royal Institution in 1891, on Wednesday, June 17, when the Prince of Wales opened the proceedings and Lord Rayleigh gave an address on Faraday's Physical Work; and again on Friday, June 26, when Prof. Dewar lectured on the Chemical Work of Faraday in relation to Modern Science. After brief references to the chemical work and a condensed historical account of his researches on the liquefaction of gases, Prof. Dewar gave further demonstrations of the properties of liquid oxygen, after liquefying it with the improved appliances then in the Institution. It was on this occasion, I think, that the startling demonstration was first given of the simple method of clearing liquefied gases by mere filtration through an ordinary fluted paper filter. The lowest temperature reached by Faraday was −110°. The lowest reached up to that time was −210°. The boiling-point of hydrogen was calculated to be −250°.

It is interesting that, on this occasion, Sir Lyon Playfair, after dwelling on the practical uses that were being made of liquefied gases, referred to uses that might be made of them in words which were a forecast of the awful purpose to which they have been put during the present war. "Sulphurous acid," he said, "will, I am certain, become most important in war, for if you took a brittle shell filled with liquid sulphurous acid and threw it between the decks of a ship it would produce such a stink that everybody would disappear in a moment. The time is coming when other gases will be used in this way." It came twenty-five years later, when the Germans introduced the use in warfare of liquid chlorine and liquid phosgene, $COCl_2$, irritant gases in comparison with which, though suffocating, sulphurous acid is almost harmless. The idea seems to have been a favourite one with Playfair, to judge from a passage in his Life, in which he suggests the use of arsenic compounds for military purposes.

Liquid air was first produced and its properties demonstrated at a Friday evening lecture on "Magnetic Properties of Liquid Oxygen" on June 10, 1892. From that time onwards it ceased to be a scientific curiosity and soon became the most familiar fluid at the Institution, where it has constantly been used as a weapon of research.

After showing that the nitrogen and oxygen in air liquefy together, the fact was demonstrated that the nitrogen boils off from the liquid before the oxygen. The separation of the two gases by fractional distillation has long been carried out on the large scale, both as a means of producing oxygen and also in preparing nitrogen for use in the production of cyanamide.

The most striking demonstration given in this lecture was that showing the attraction of liquid oxygen to the poles of a magnet. A cup made of rock-salt, containing some liquid oxygen, was placed between and a little below the poles of a large electro-magnet; whenever the electric circuit was completed the liquid rose from the cup and connected the two poles. To those of us who first witnessed the demonstration, though in accordance with our theoretical expectations, the way in which the liquefied oxygen suddenly leapt at and clung to the poles of the electromagnet was a very wonderful sight —a marvellous and fit culmination of advance in low-temperature technique.

The lecture on "Liquid Atmospheric Air" (January 20, 1893) is memorable as the occasion on which the *Thermos Flask* was first introduced to public notice and its efficiency demonstrated. The construction of the flask was explained and the principles underlying the production of high vacua were fully discussed, especially the vacua secured with the aid of mercury vapour. The period was also entered upon during which such remarkable use has been made at the Institution of liquid air, as a cooling agent, in the investigation of physical properties generally at low temperatures. On this occasion reference was made to the results obtained, in conjunction with Prof. Fleming, in an investigation of the electric conductivity of metals at very low temperatures; also to an investigation, carried out in conjunction with Prof. Liveing, of the optical properties of liquefied gases. The direct liquefaction of air was effected before the audience by means of liquid oxygen boiling under a pressure equal to that of about half an inch of mercury.

The subject of chemical action at low temperatures was also broached and, by one of those *tours de force* so characteristic of the lecturer, sulphur was shown to burn below liquid oxygen. Altogether, on account of the variety of topics considered and the striking novelty of the effects demonstrated, the lecture was one of the most remarkable of the unrivalled series of discourses given by Prof. Dewar in illustration of his pioneer low-temperature investigations, for which the Institution is so noted.

In the lecture delivered a year later (January 19, 1894) on "Scientific Uses of Liquid Air," in which again a very wide field was covered, many illustrations were given of further uses made of liquid air and of liquid oxygen, of which the latter was often obtained from the former by allowing the more volatile nitrogen to evaporate away.

The final conclusion deducible from the investigation carried out in conjunction with Professor Fleming was announced as being that "at the zero of absolute temperature all pure metals would be perfect conductors of electricity." A full account of this work was given by Professor Fleming on Friday, June 5, 1896. This was the first occasion on which the extraordinary change in properties conditioned by cooling to a very low temperature was brought home to an audience.

Liquid oxygen was shown to be nearly as transparent to high-temperature radiations as chloroform, one of the most transparent liquids next to carbon bisulphide. The advantage of a coating of mercury on the interior surface of the vacuum vessel as a means of preventing the access of heat from without to the cooled liquid within was commented upon. Then illustrations were given of the behaviour of a small residue of mercury vapour within exhausted vessels. Attention was called to the alteration in appearance of many coloured substances — mercury, uranium and platinum compounds — when cooled in liquid air. The change of indiarubber from the plastic condition into that of a glass-brittle substance was demonstrated for the first time. Attention was drawn to the possibility of dividing organic substances which solidify only at very low temperatures into two classes—those which crystallize and those which form glasses. Bisulphide of carbon belongs to the former, ordinary alcohol to the latter.

A method of determining latent and specific heats at low temperatures with the aid of the actual apparatus used was then demonstrated. The method involved the collection of the gas volatilized by the heat extracted from a substance when dropped into liquid oxygen.

The effect of very low temperatures in increasing cohesion in metals was then demonstrated and an account given of experiments to determine the breaking strain of metals cooled to − 180°. A spiral spring made of fusible metal, which is quickly drawn out into a straight wire at ordinary temperatures by an ounce weight, will support a couple of pounds at − 182° and vibrate like a steel spring. The following table illustrates the character of the increase in cohesive strength metals undergo when cooled :

BREAKING-STRESS IN POUNDS OF METALLIC WIRES
0·098 INCH IN DIAMETER.

——	15° C.	− 182° C.
Steel	420	700
Iron	320	670
Copper	200	300
Brass	310	440
German silver	470	600
Gold	255	340
Silver	330	420

The alteration of the magnetic moment of magnets on cooling was another subject considered.

A year later (January 18, 1895), the subject considered was that of "Phosphorescence and Photographic Action at the Temperature of Liquid Air." Those present on this occasion will remember the remarkable character of the demonstrations given and the beauty of the phenomena brought under notice. A great variety of substances were shown to become temporarily phosphorescent when they were exposed during a few seconds to the full light of an electric arc after being thoroughly cooled by immersion in liquid air. Among the most remarkable of these demonstrations were those given with substances such as eggshell and feathers; the eggshell shone brilliantly as a globule of blue light and the feather was equally brilliant, its outlines being clearly seen in the darkened room.

Water, when pure, is at most only faintly phosphorescent but remarkably so when impure. It is clear that the character of the phenomena exhibited are in close correlation with constitutional peculiarities. The two most remarkable classes of compound exhibiting phosphorescence at low temperatures are the platinocyanides among inorganic compounds and ketonic compounds such as acetophenone among organic. In this connexion, the observations made by Emerson Reynolds in the Davy Faraday Laboratory, on the changes in composition undergone by certain platinocyanides at low temperatures, are of interest as an indication that "constitutional changes" may well be involved in the manifestation of phosphorescence at low temperatures (comp. R. Soc. Proc., 1909, pp. 82, 380).

Attention was drawn in this lecture to the reduction of photographic activity by at least 80 per cent. at – 180°. This matter has been referred to in my previous essay (p. 37).

In a lecture on March 27, 1896, on "New Researches in Liquid Air," Prof. Dewar gave a summary of the advances made in engineering plant in the interval since Pictet made his momentous discovery in 1878. He expressed the opinion that, apart from important mechanical details and the development of laboratory technique, nothing new had been added by any investigator to the principles involved in the construction and use of low-temperature apparatus since the time of Pictet's discovery. Reference was made to the fantastic claim put forward by Prof. Olzewski that because, in 1890, he used a steel tube combined with a stop-cock to draw off liquid oxygen, he had taught the world "the method of getting large quantities of liquid gases." To this Prof. Dewar very properly remarks, "As to such claims, one can only wonder at the meagre additions to knowledge that in our time are unhesitatingly put forward as original and more especially that scientific men could be got to give them any currency in this country."

On this occasion, a small-scale laboratory apparatus for the liquefaction of oxygen and other gases was exhibited and explained. The value of vacuum vessels was also demonstrated and made obvious to all by means of a calorimeter consisting of three similar vacuum tubes joined to a common stem; in one of these the annular space contained air; in the other vessels it was exhausted but the walls of one were silvered in addition; ethylene was liquefied in each of the inner tubes and the rate at which evaporation took place in each tube was indicated by the height of the flame given when the escaping gas was fired at a tube inserted into the mouth of the vessel.

A variety of interesting demonstrations were given on this occasion, one being of the method of ascertaining the density of liquid oxygen and of liquid air by weighing various substances in the liquid. Probably the most significant was that showing that the gas nitric oxide might be condensed to a nearly white solid which became a blue liquid when melted, the colour being more pronounced at the melting than at the boiling-point, a fact which might be regarded as evidence that the gas had been entirely deprived of the higher oxide. The observation is of importance as an indication that, at low temperatures, the simple molecule NO—one of the most enigmatical compounds known to the chemist—is capable of combining with itself. It may be surmised that perhaps three such simple molecules are associated in the coloured liquid.

On this occasion, one of Prof. Dewar's most fascinating demonstrations was first displayed—that of burning a jet of hydrogen below the surface of liquid oxygen; graphite and diamond were also burnt in this way in face of the audience. The demonstrations were striking, not only as instances of daring and of dexterous manipulation but also on account of the proof that was given of the formation of ozone, under such conditions, by the gradual conversion of the colourless solvent into a blue liquid, owing to the condensation of the ozone and its dissolution in the liquid oxygen. It must be supposed that in all cases of combustion in oxygen, the molecule of the gas undergoes dissociation, thus:—

$$O_2 \rightleftharpoons O + O . \qquad O + O + O \rightleftharpoons O_3 .$$

It is permissible to lay stress on the fact that such observations do not really prove that chemical action may take place at the lowest temperature, nor that their occurrence is independent of the presence of a third factor; directly the hot body is introduced into the liquid it must create its own atmosphere and within this every grade of temperature may prevail.

The report of the lecture contains the all-significant statement:

"If liquid oxygen is absorbed in wood charcoal or cotton wool and a part of the body heated to redness, combustion can start with explosive violence." Liquid air has been extensively used of late years in conjunction with charcoal as an explosive mixture in blasting operations and there is little doubt that this most simple of all blasting agents may play an important part in such operations in future.

In this discourse, much was said of the cooling effect produced on discharging gases through fine apertures and of the use of cooling coils. This was preliminary to a demonstration of the liquefaction of hydrogen and its appearance in the form of a jet. With the appliances then available, it was not posssible to collect the liquid in quantity. When the liquid jet was allowed to impinge upon liquid air or oxygen, these were transformed into hard white solids.

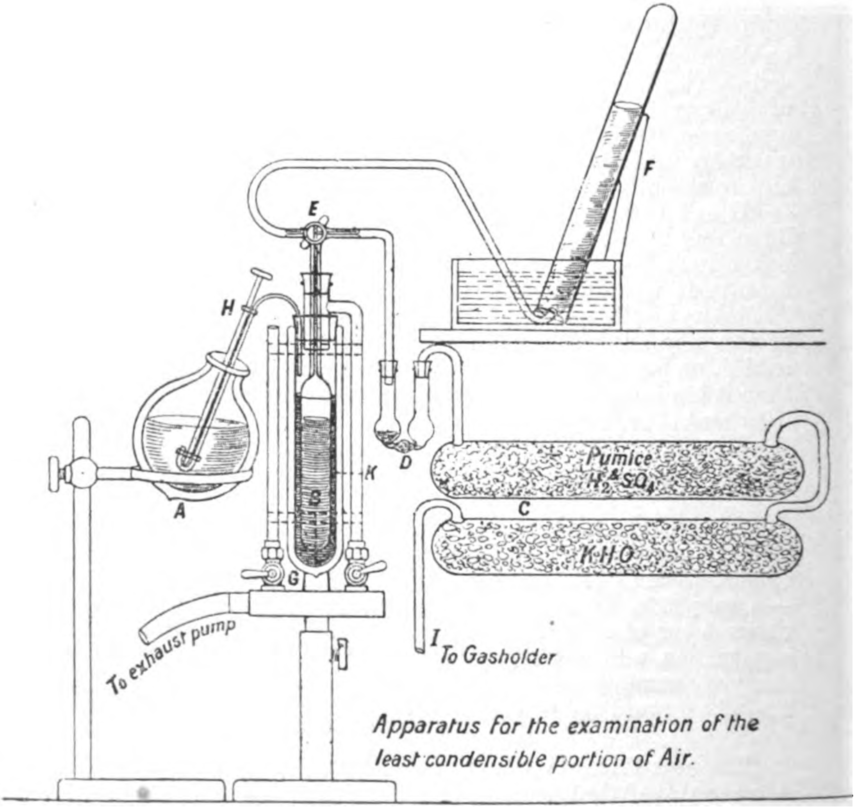
It had long been evident that even Cailletet had not succeeded in liquefying hydrogen. This was first effected by Wroblewski in 1898 but neither he nor Olzewski was able to deal with it in a tangible form. It was first collected as a liquid at the Royal Institution laboratory on May 10, 1898. At the commemoration of the Centenary of the Royal Institution on June 9, 1898, Prof. Dewar was able to exhibit no less than a litre of the liquid on the lecture table. Since then it has been nearly as familiar to us as liquid air and we have witnessed the most astonishing demonstrations of its cooling capacity.

Miss Agnes Clerke has given an account of the work in her Essay in this series.

A return was made to the "Properties of Liquid Oxygen" in the Friday evening lecture on January 22, 1897. The absorption spectra of gaseous and liquid oxygen and of liquid air were then considered —a subject of interest to all spectroscopic inquirers who observe through our atmosphere. The results of an inquiry carried out in conjunction with Prof. Fleming into the numerical values of the magnetic permeability and susceptibility of liquid oxygen were then brought forward and demonstrated.

The next Friday evening (April 1, 1898) was devoted to "Liquid Air as an Analytical Agent." A most daring demonstration was given on this occasion by pouring liquid air from a tin can—filled by dipping this into a 5-gallon jar full of the liquid—into a large red-hot silver basin; the liquid remained as quiescent at this high temperature as in cooler vessels and maintained a spheroidal condition—that is to say, it was suspended over an atmosphere of gas produced at the heated surface.

The apparatus shown in the figure on p. 816 of Vol. XV. of the Institution Proceedings was then explained and shown in action. The apparatus had been exhibited and the results obtained with it described at the Chemical Society, on November 4, 1897, in an address given by Prof. Dewar, then the President of the Society, under the title "Liquefaction of Air and the Detection of Impurities." There is no doubt that the information revealed in this paper gave Ramsay and Travers a clue to the presence of unknown gases in the air as well as to the method of isolating them and that it was the cause of their intrusion into this field of work. Prof. Dewar's course of action, up to this date, had been systematic and logical; he had endeavoured, as far as possible, with the very limited means at his disposal, to exploit the opportunities, which he was continually extending, of studying low-temperature phenomena; and he had long been engaged in collecting gas given off from the Bath springs with the



Apparatus for the examination of the least condensible portion of Air.

Fig. 1.

special object of studying its minor constituents. Had he been left to himself, the Royal Institution would undoubtedly have been the seat of the discovery of the inert gases in the atmosphere other than argon—Lord Rayleigh's discovery—and helium, the discovery of Ramsay's genius. The episode is of interest as a problem in scientific ethics and is one which merits more discussion than it has received, in view of the continued increase in the number of scientific workers, their average lack of originality and their ever-growing tendency to appropriate ideas which they have had no share in developing. It may be the modern system is one that favours advance—but it is a question if the gain due to such advance be sufficient to balance the attendant disadvantages: the extent to which premature publication has been developed, of late years, is one of its worst consequences but the moral effect upon the younger workers is perhaps the worst; the decay of the courtesy and consideration characteristic of a former generation is particularly lamentable.

The following statement from the report of the lecture may be reproduced here:—

"About 70 litres of the Bath gas were condensed, certainly the largest quantity of this gas ever subjected to chemical examination. This was boiled off and, as by accident too much nitrogen had volatilized along with the gas, oxygen was added and the mixture sparked over alkali, to get rid of the excess of nitrogen. The sample of gas directly collected from the liquid nitrogen contained about 50 per cent. of helium. During the sparking, helium lines were well marked (along with the others, the origin of which must be settled later) and a vacuum tube filled with the product of the sparking gave a splendid spectrum of the gas. The recorded unknown lines in the Bath helium were subsequently detected along with helium in the more volatile portion of liquid air. Eight months after my paper to the Chemical Society and some two months after this address was delivered the same material was found by Professors Ramsay and Travers to exist in argon and has been recognized and named by them Neon, a new element."

In this lecture an interesting application of the vacuum vessel was demonstrated showing that Pictet's conclusion was incorrect that at low temperatures all substances had practically the same transparency and that a non-conducting body became ineffective as a shield against influx of heat.

Sets of three double-walled glass vacuum tubes, as nearly identical in size and shape as possible, were mounted on a common stem and the hollow space in two out of the three was filled in with the substances to be studied. The tubes were then thoroughly exhausted —a work of great labour.

The rate of influx of heat was measured by determining the rate of evaporation of equal volumes of liquid air contained in the vessels.

The results showed that the presence of certain finely divided solids —silica, charcoal, lampblack—increased the heat insulating power of the vessel, although in presence of air they facilitate the transference of heat. In no case was the solid so effective as a high vacuum alone, the glass surfaces being silvered.

Photographic action at low temperatures was again referred to in these lectures and shown to be greatly reduced but the caveat at the end of the report should be noted :—

"It would seem probable that if the plate could be developed at these low temperatures no action would be apparent and that it is during the heating up after the low-temperature exposure that the photographic action on the film takes place through an internal phosphorescence. This possibility must make us cautious in drawing inferences as to possible chemical action at low temperatures."

Liquid hydrogen was considered in the lecture on January 20, 1899. The report is of importance on account of the historical matter it contains, particularly the denial of the accuracy of statements put forward by Prof. Ramsay as to the work of the Polish observer, Olszewski.

In his Commemoration lecture on June 7, 1899, after quoting Davy's remark :—

"Nothing tends so much to the advancement of knowledge as the application of a new instrument. The native intellectual powers of man in different times are not so much the causes of the different success of their labours as the peculiar nature of the means and artificial resources in their possession,"

Prof. Dewar said :—

"The new instrument of research, which, for the first time, we have to experiment with before an audience, is the liquid form of the old inflammable air of Cavendish."

A wonderful series of demonstrations was given in this memorable lecture, which Lord Kelvin subsequently spoke of as "brilliant, beautiful and splendidly interesting." The liquefaction of the gas, as the lecturer said, was the culmination of ten years of devotion to low-temperature research. By it all doubt was laid at rest as to the true nature of hydrogen: whether it should be ranked with the metals, as Faraday, Dumas and Graham had anticipated it would be eventually—though it must be confessed that the properties of the hydrocarbons are in no way to be taken as in harmony with such a view.

Solid hydrogen was the subject of the next lecture, on April 6, 1900. It was seen to have the appearance of pure ice. Miss Clerke has dealt with this achievement.

A broad outline of the progress made during the century in studying the properties of gases was given on January 18, 1910

("Gases at the Beginning and End of the Century"). In the latter part of the lecture, vacuum vessels were again referred to specially and various types shown.

The most important matter touched on in this discourse was the method of separating the more volatile constituents of air which has since been of great service in preparing neon.

"The Problems of the Atmosphere," discussed in the lecture on April 11, 1902, were mainly those relating to gases of low density present in minute proportions in ordinary air—such as hydrogen and helium—and to the probable increase in the amount of these as the atmosphere is ascended. A method was described of liquefying air and at the same time separating the more volatile gases, without the use of liquid hydrogen, uncontaminated by compression pumps. Reference is briefly made to this lecture in my previous essay.

The ever-perennial subject of "Low Temperature Investigation" again came under notice on January 16, 1903. The work described in this lecture is discussed in the section on "Modifications in the Properties of Matter at Low Temperatures," in my previous essay. Perhaps the most striking case brought forward was that of india-rubber, which at very low temperatures becomes a rigid, resilient, brittle solid through which oxygen does not diffuse.

"Liquid Hydrogen Calorimetry," the subject of the lecture on March 25, 1904, also has been discussed in my previous essay.

The lecture on "New Low Temperature Phenomena," on January 20, 1905, was the first of a series dealing with Sir James Dewar's momentous discovery of the extraordinary absorptive power of charcoal at low temperatures. That the discovery was not made at an earlier date, as soon as liquid air became available as a cooling agent (1892), may appear remarkable, but the frequency with which the obvious has remained unseen is well known. Sir James appears to have had his attention diverted mainly by the extraordinary reduction of chemical action at low temperatures—through finding that substances such as phosphorus and potassium could not be oxidized, and that no action was traceable even through the agency of electric currents. He was thus led to suppose that physical action would be similarly affected, the more as in his earlier work on hydrogenenium he had found that the absorption of hydrogen by finely-divided palladium was not promoted by cooling.

The subject was further developed on June 8, 1906. The use of charcoal in rendering possible the use of metallic vessels as perfect vacuum flasks was first disclosed in this lecture.

Again the subject was dealt with on June 7, 1907. The early part of the lecture was devoted to an account of the difficulties met with in attempting to liquefy helium, including the loss of the store of this gas laboriously accumulated over a period of two years, owing to the collapse of a glass vacuum vessel and the consequent rupture of the regenerator coil of the apparatus.

On this occasion a very striking illustration was given of the separation by diffusion of gases differing in density. A vacuum tube containing helium and neon, showing the intense red glow of neon when a discharge was passed through it, was partially immersed in liquid hydrogen; the more condensible and heavier neon at once sank to the lower end, as revealed by the orange-red glow, the upper part of the tube showing the yellow colour of the helium discharge. This demonstration is as good an example, perhaps, as could be given of the surprises Sir James Dewar has constantly afforded his hearers in his lectures, by his extraordinary faculty of giving apt and original illustration of points of importance on which he has wished to lay emphasis.

The lecture on the "Nadir of Temperature and Allied Problems" on June 5, 1908, was again a mass of apt illustrations of low-temperature phenomena made manifest with the aid of highly cooled charcoal. On this occasion hydrogen was solidified merely by connecting a vessel containing liquid hydrogen and surrounded with liquid air with an exhausted vessel containing charcoal also cooled by liquid air.

In view of the extraordinary difficulty originally met with in liquefying and solidifying hydrogen, the simplicity of the means adopted in this demonstration was altogether astonishing—and eminently characteristic of its author.

The main subject considered under the title "Problems of Helium and Radium" on June 11, 1909, was the amount of the former generated spontaneously from radium over a given period. But the phosphorescence of gases again came under discussion.

On January 21, 1910, in a lecture on "Light Reactions at Low Temperatures," after demonstrating the chemical efforts produced by light, in various ways, mostly involving the exhibition anew of demonstrations given in former lectures on the action of light, Sir James Dewar dealt at length with the action of ultra-violet rays on phosphorescent micro-organisms at low temperatures.

In this lecture several light cells such as were referred to in the lecture on "The Physiological Action of Light," on March 28, 1878, were shown in action; these are figured in the published abstract (xix, 928). The most striking demonstration given in the lecture was one showing that ozone is produced by the action of ultra-violet rays on oxygen, not only when liquid oxygen is exposed to their influence through the walls of a quartz vacuum vessel but even when solid oxygen surrounded by liquid hydrogen is subjected to their impact.

In the Friday evening lecture on January 20, 1911, on "Chemical and Physical Change at Low Temperatures," the production of ozone by the impact of ultra-violet rays on solid oxygen at the temperature of boiling hydrogen was rightly referred to as "perhaps the most remarkable of all low-temperature reactions observed."

The solidification of oxygen was first demonstrated, on this occasion. The main subject of the lecture, however, was the remarkable gas, carbon monosulphide, CS, discovered as the outcome of experiments made in conjunction with the late Dr. H. O. Jones.

The "Heat Problems," brought under consideration in the Friday evening on January 19, 1912, were, as usual, cold problems. On this occasion a very remarkable demonstration was given of the persistence of drops of liquid hydrogen falling a considerable distance through the air.

Owing to Sir James Dewar's state of health he did not lecture in 1913.

In 1914, on January 23, the discourse delivered was on "The Coming of Age of the Vacuum Flask;" but the real subject was an account of a detailed inquiry into the specific heat of the elements between the boiling-point of liquid nitrogen and hydrogen, by means of an improved form of liquid-hydrogen-calorimeter. The amount of hydrogen present in the atmosphere and respired air was also brought under notice.

On January 22, 1915, this last subject was still further developed. The preferential condensation of gases by charcoal was next discussed; then illustrations were given of the diffusion of hydrogen through heated platinum and of helium and neon through quartz. The method followed in an inquiry into diffusion through india-rubber was also referred to.

SURFACE TENSION PHENOMENA.

Thus far, since 1883, low-temperature research had occupied Sir James Dewar's attention almost exclusively. But the beginning of a new septennate has been marked by his entry upon a new line of inquiry. Having done his best, in the past, to still intermolecular forces, he has now passed to the study of their freest interplay in water-soap films. His ingenuity and dexterity of hand have never been more obvious. Under his magic care, the soap-bubble has ceased to be an ephermal plaything of the moment. Horizontal *black* films feet across, huge *black* spheres, 16 inches or more in diameter—their blackness is proof of their infinite tenuity—have been preserved during over a year and might well last for ever if the conditions of equilibrium could be maintained. The last three Friday evenings have been devoted to their display. Let us hope that the complete story may soon be told of their genesis and that the lessons derived from their study may be adequately described.

Soap is all but absent from the giant films and bubbles Sir James can now call into being at will, so that they are essentially water films. That water has wondrous properties—that it is the most wonderful of all liquids—we already know ; but the magic affinities of its molecules, the individuality of their behaviour, were never so clearly displayed as they are in these tenuous films of a new era. That they have been a solace to their progenitor during a period of indescribable stress is not merely matter for congratulation but proof that man is able to rise above his surroundings : a sign of hope we may well lay to heart.

Here my record ends—in wonder, at the wealth of performance I have been able to chronicle. It has been said :—" There is a myth among some Eastern nations that at the birth of Genius an unkind fairy marred all the good gifts of the other fairies by depriving it of the power of knowing where to stop." The story is no myth, it seems. It is clear some such Fairy has been active throughout the period of the present Fullerian Fellowship, these last years especially.

THE HIGH VACUUM SEPTENNATE, 1907–1914.

The Dewar Vacuum Vessel.

The coming of age of the introduction of the vacuum jacketed vessel into practice was proclaimed by Sir James Dewar in his lecture on Friday, January 23, 1914. As he then remarked :—

" The insulating properties of a vacuum in preventing the influx of heat by the elimination of convection currents had been known to physicists ever since the investigations of Dulong and Petit on the laws of radiation, in the early part of the last century, proved the important part played by the gas particles surrounding a body in dissipating heat otherwise than by pure radiation."

In 1873, he had himself used a highly exhausted, annular, brass vessel for calorimetric purposes, when determining the specific heat of *Hydrogenium*—that is to say, hydrogen condensed in palladium.

The apparatus is both described and figured in the Transactions of the Royal Society of Edinburgh for that year (vol. xxvii, p. 167).

Proceeding from the same principle, the French physicist Violle appears to have been the first, in 1882, to construct and use for a similar purpose—in determining specific heats by the method of cooling—a vessel made of glass. In a note "Sur un Calorimètre par Refroidissement," he describes (C.R. xciv, 1510) his method in the following terms:—

"Je prends une petite bouteille en verre mince, à col étroite et à double enveloppe, entre les deux enveloppes de laquelle on a fait un bon vide avant de former l'enveloppe extérieure. L'appareil porte ainsi avec lui son enceinte et les conditions de refroidissement resteront toujours indentiquement les mêmes."

Violle was alive to the value of a silver coating but he applied this only to the outer surface of the interior chamber containing the liquid, to regularize the emission of heat. In the Dewar vessel, the entire surface of the vacuous chamber is silvered. To quote Violle:—

"J'ai fait construire différentes bouteilles de diverse capacités; certains d'entre elles ont la surface extérieure de l'enveloppe intérieure argentée, de façon à ralentir et régulariser encore la rayonnement."

Claude, the French engineer, who is so well known on account of his important contributions to the development of machinery for the liquefaction of gases, in his book on the subject ("Liquid Air, Oxygen and Nitrogen," p. 210, Eng. ed., J. & A. Churchill, London, 1913) gives the credit of the invention of the double-walled glass vacuum vessel to D'Arsonval, in 1887, overlooking Violle's earlier communication of 1882 to the French Academy.

D'Arsonval's claim is based on a communication made on February 11, 1888, to the Société de Biologie (Series 8, v, 136), under the title "Action thermo-isolatrice du vide sec." He appears to have devised the vessel for the purpose of storing methyl-chloride, then coming into use as an anæsthetic:—

"Dans bien des circonstances il importe de réaliser un milieu aussi imperméable que possible à la chaleur et qui soit néanmoins parfaitement transparent. Tel est le cas qui s'est présenté dans certaines expériences de calorimétrie où je fais usage des gaz liquéfiés.

"Après bien des essais, dans le détail desquels je ne saurais entrer ici, j'ai trouvé que le vide sec, fait dans un vase en verre, constitue une excellente enveloppe pour arrêter la chaleur obscure.

"L'appareil se compose en principe de deux vases en verre, entrant l'un dans l'autre et soudés de facon à former un vase unique.

"Cet ensemble constitue un récipient que présente deux cavités concentriques: l'une intérieure, qui est le réservoir proprement dit; l'autre annulaire, où l'on fait le vide sec à l'aide de la pompe à mercure ou de la machine Carré à faire la glace. La transparence des parois de ce récipient permet de voir tout ce qui se passe dans son intérieur. L'enveloppe annulaire, où le vide sec existe, constitue la paroi thermo-isolatrice.—J'ai pu constater dans certaines cas qu'un liquide chaud placé dans le vase intérieur se réfroidit de quinz à vingt fois plus lentement que si l'enveloppe isolatrice n'existait pas."

Such vessels apparently could not then be made in France (cf. Claude, p. 216) any more than in England! But had they been made as described by D'Arsonval, they would have been far inferior, through lack of the highly-polished silvered interior which is an essential feature of the Dewar vessel.

Sir James Dewar, who was not aware of these French publications on the subject when I brought them under his notice a year or so ago, was able to develop the construction of glass vacuum vessels but slowly owing to the difficulties of glass-blowing and the absence from this country of competent makers willing to undertake their construction. The use of such vessels only became possible from 1898 onwards, after he had succeeded in getting them made in Germany. Meanwhile, the manufacture of such vessels has been developed into an important industry in that country, especially since their introduction in the form of the all-familiar *Thermos Flask*. Having enjoyed a world monopoly, even up to the outbreak of war, the Germans have profited greatly from the manufacture.

Sir James Dewar alone has the credit of having made them the perfect and indispensable weapons of low-temperature research they now are; without them the more serious and important part of the work could not have been accomplished. In the 1914 lecture, it was pointed out that they were equally useful in storing hot liquids. When the production of vacuum vessels from quartz instead of glass is perfected, it is to be expected that they will come largely into use in preventing the escape of heat. No advantage arises from silvering such vessels, but if coated with nickel deposited from nickel carbonyl they can be exposed to high temperatures.

The later invention of the metallic vacuum vessel—which has been rendered possible only through the discovery of the marvellous condensing power of charcoal at very low temperatures—is one that eventually, in all probability, will rank even beyond that of the fragile glass vessel in importance, in view of its many practical applications. In this vessel, the promise of the brass vacuum calorimeter of 1873 is entirely fulfilled. One use to which it is being put in the war, primarily by the Germans, is as a means of providing artificial respiration for aeroplane pilots and observers when flying at great heights.

The various forms given to the vacuum vessel are described and figured in lectures delivered on January 18, 1901, and January 23, 1914.

But from his many inventions, the inventor himself has had no profit: as Samuel Butler remarks, "If genius cannot be bought with money, still less can it sell what it produces. . . ."

The work of Low-Temperature Research, however, like that of Davy and Faraday, has brought repute to the Institution and its author has had the great satisfaction of seeing it already bear fruit in many practical directions—that others reap where he has sown. The purpose of the Institution has thus been fulfilled!

ABASEMENT OF TEMPERATURE.

SOLIDIFICATION OF HYDROGEN AND OXYGEN.

The solidification of hydrogen, on exhausting the gas by an air-pump from above the liquid, was first demonstrated in the Institution Theatre on April 6, 1900. Later it was shown that another method is to bubble the more volatile helium through the liquid. Far the most telling method, however, which is also a most effective demonstration of the extraordinary gaseous avidity of cooled charcoal, is that given on June 5, 1908, with the apparatus figured in vol. xix, p. 422, of the Proceedings of the Institution (Fig. 2).

Owing to the small vapour pressure of oxygen at its melting point, liquid oxygen cannot be solidified even with the aid of a powerful pump of the ordinary kind. The production of solid oxygen, as a transparent jelly, was first demonstrated on January 20, 1911, with the aid of charcoal cooled in liquid oxygen; the apparatus is figured at p. 261 of the Institution Proceedings (1911, vol. xx, Fig. 3). Subsequently, it was shown (Jan. 23, 1914) that liquid oxygen, cooled only by liquid air, can be solidified directly by using the Gaede pump, by means of which a very high degree of exhaustion may be effected rapidly. As soon as the vacuum is no longer maintained, the jelly melts. The very different pressures at which the various solidified gases melt are shown in the following table:—

——	Melting Point Pressure	Absolute Temperature
Helium	Below 2 mm.	Below 2·5°
Neon	350	35°
Argon	628	85°
Krypton	158	104°
Xenon	86	133°
Hydrogen	55	14°
Nitrogen	90	59°
Oxygen	0·9	46°

The advantage of using liquid hydrogen in place of liquid air or oxygen has been demonstrated in many ways in the course of Sir

James Dewar's investigations and it is obvious that still more remarkable effects would be obtained if the absolute zero were more nearly approached—but this appears to be a very difficult matter. The boiling-point of hydrogen is 20° absolute, that of helium is about 4·5°. By evaporating liquid helium under reduced pressure, Kammerling Onnes has not been able to go below 3° absolute. Sir James Dewar estimates that, if a gas were available as much more volatile than helium as this latter is more volatile than hydrogen, possibly the temperature might be lowered to within 0·5 of the absolute zero but not to zero.

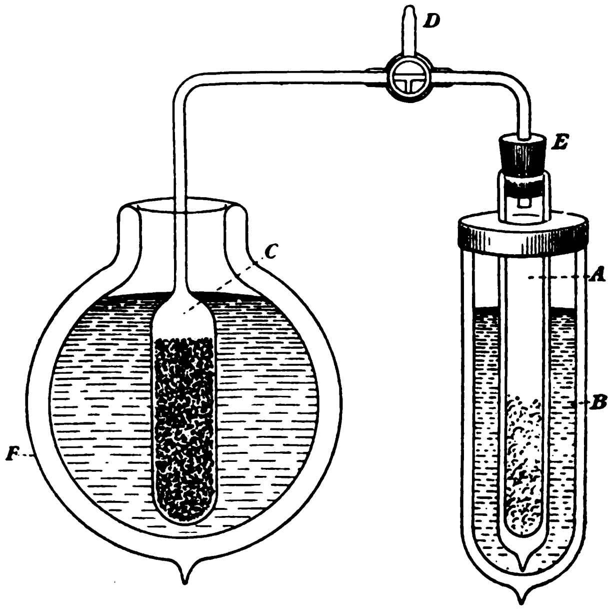


Fig. 2.

A. Liquid hydrogen. B. Liquid air.
C. Charcoal cooled in liquid air.
D. Connexion with air pump.

It is surprising, however, how low a temperature may be reached even by the evaporation of ice alone, which can be shown to vaporize at as low a temperature as − 78° C. A striking demonstration of Guy Lussac's statement, that it is possible to freeze mercury by the evaporation of ice, was given by Sir James Dewar in his lecture on January 19, 1912 (Proceedings, xx, 581).

In this connexion, it is desirable to note the superior value of the gas thermometer as a means of measuring very low temperatures. The practical limits of temperature reached by the volatilization at low pressures of various substances are shown in the following table; it is to be noted that the maximum cooling effect is, in all cases, one-third of the critical temperature of the substance volatilized :—

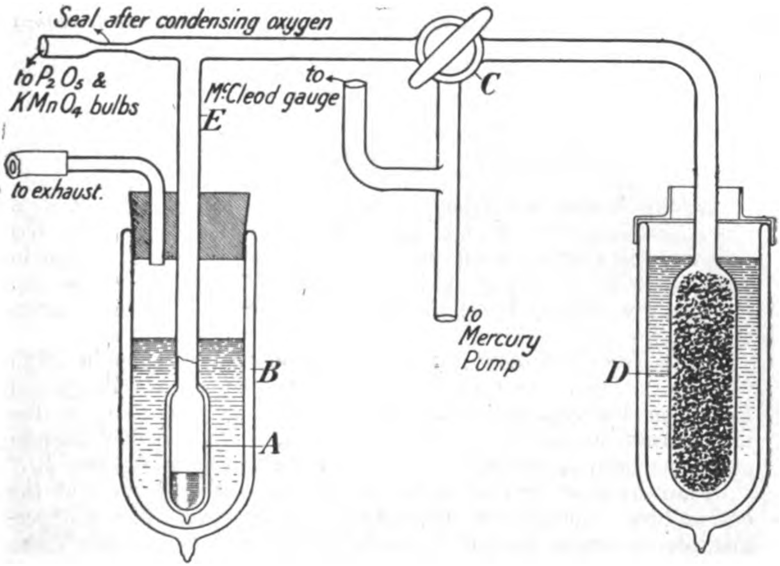


Fig. 3.

A. Silvered vacuum-jacketed tube, with an uncoated slit, containing 5–10 cc. oxygen. **B.** Silvered vacuum tube, with clear slit, containing liquid air under an exhaust less than 20 mm. **D.** Charcoal in boiling oxygen.

LIMITS OF TEMPERATURE OBTAINABLE WITH VARIOUS COOLING AGENTS.

Solid	Absolute Temperature attainable. Degrees	Pressure in mm. of Mercury	Fraction of T_c
Ice	203	0·001	0·32
Carbon dioxide . .	132	0·63	0·43
Oxygen	51	0·19	0·33
Nitrogen	37	0·7	0·30
Hydrogen	9	0·59	0·29
Helium	1·8	0·26	0·33
"X".	0·4	..	..

Owing to the self-cooling effect exercised through surface evaporation, drops of the liquefied gases, even of hydrogen, are of remarkable persistence. Drops of liquid air will retain this state during a fall of 30 to 100 feet through the air. Even in the case of hydrogen, small drops are traceable through a fall of 8 feet by the trail of frozen air caused by their passage. (January 19, 1912.)

CHEMICAL INTERACTIONS AT LOW TEMPERATURES.

As Sir James Dewar has pointed out (Proc. xx, 256), perhaps the most remarkable of all the low-temperature changes he has discovered is the fact, mentioned in the Friday evening discourse in 1910, that solid oxygen at the temperature of boiling hydrogen (20° abs.) is partially transformed into ozone by the impact of ultra-violet rays.

In the previous essay, in the section on "Conduction in High Charcoal Vacua in relation to the Theory of Chemical Change and Induction Phenomena," I have ventured to suggest that, in the various cases in which the occurrence of change at very low temperatures has been observed, the conditions *locally* may be "*spheroidal.*"

I am the more inclined to favour this explanation, in view of the extraordinary behaviour of oxygen referred to above. It seems permissible to argue that if ultra-violet rays can "penetrate" the molecules of oxygen and cause their disruption, they can equally well penetrate a mass of either liquid or solid oxygen and cause local liquefactions, so that complex systems might arise within which action could take place. I question if the problem be not one of those we must now hold to be beyond the range of experimental inquiry; indeed, whether the ultimate decision can well be arrived at otherwise than by a purely logical process.

THE PHENOMENA OF PHOSPHORESCENCE.

Luminous phenomena have always been Sir James Dewar's favourite study from the time of his first appearance as a lecturer at the Institution, when he dealt with the physiological action of light. During many years, subsequently, he devoted himself almost entirely to spectroscopic inquiry. Then came the period during which low-temperature research all but monopolized his attention but it is clear that the old interest only lay dormant. First came observations on the chemical action of light, especially at low temperatures; these were followed by an inquiry into the excitation of phosphorescence by light at low temperatures. The phosphorescence of ozone was also brought under investigation.

These inquiries have reached their culmination during the septenate under discussion. In this period a further study has been made of the phosphorescent phenomena attending the production of ozone; in addition *photogenic bacteria* have been thoroughly investigated.

The phenomena of phosphorescence are familiar; for example, the delicate haze of blue light seen when paper brushed over with a solution of quinine sulphate is exposed to invisible ultra-violet radiations. A number of dye-stuffs fluoresce in a most wonderful manner—the eosines in particular. In all these cases, as in that of quinine, rays of higher refrangibility are absorbed and rays of lower refrangibility emitted.

In the discussion of the phenomena of colour, in my previous essay, in the section on the properties and structure of carbon, attention is called to the close connexion of colour with constitution. The eosin dye-stuffs are peculiar as existing in a coloured form and in a colourless form of different but closely allied structure: it is probable that, in solution, the one form constantly passes into the other. It may well be that fluorescence is a consequence of this oscillatory change and it is not improbable that the phenomena of phosphorescence generally are of a similar order (cp. R. Soc. Proc., pp. 70, 99).

In the remarkable series of demonstrations which he gave in 1895, Sir James Dewar was able to show that a great number of ordinary materials become brilliantly phosphorescent when exposed to light at a low temperature. Further illustrations were given, in again alluding to these phenomena, in his lecture on January 21, 1910. Thus, an ivory paper-knife, cooled by liquid air, after exposure to light from an arc lamp, phosphoresces brilliantly; a slip of gelatin, even an ordinary paraffin candle, behaves in a similar manner. The so-called luminous paint, calcium sulphide, on the other hand, which shines after exposure to light at ordinary temperatures, when cooled in liquid air ceases to phosphoresce; but if it be cooled while in the non-luminous state and then exposed to ultra-violet light it emits light when allowed to become warm. Such cases may be explained as due to changes of molecular structure conditioned by the action of light and followed by an oscillatory change from one form to another when the temperature conditions are favourable.

An extreme case of the action of ultra-violet light being causative of disruptive action is the altogether remarkable illustration given by Sir James Dewar, in the lecture under consideration, showing that not only is ozone produced if a beam from an electric arc be focused upon liquid oxygen by means of a quartz lens (through the walls of a quartz vacuum vessel) but that even *solid oxygen* is so affected.

Ultra-violet rays, it is well known, are the cause of sunburn. The effect on the eyes produced by sun-glare and by the powerful illuminants now used so widely is also traceable to these rays; it is no longer felt when these rays are cut off by means of the colourless glasses now in use, which absorb these rays.

Sir James Dewar has used the organism *Photobacterium phosphorescens* in demonstrating the activity of these rays, as emitted by a mercury lamp, even at the low temperature of liquid air (- 185°).

The effect is to kill the bacteria rapidly, even when the culture medium in which they are present is cooled down to - 185°. To quote from the abstract:—

"Five minutes' exposure, at a distance of a few inches from the mercury lamp, is usually sufficient to kill the bacteria when they are located on the surface layer of the culture medium. The exposed culture is taken out and allowed to warm up in the air. No brightness supervenes even after the culture is kept for several hours."

Other (non-phosphorescent) organisms are equally affected by the rays.

The death of the phosphorescent organism is also brought about by a number of metals—zinc, copper, silver and especially mercury—when these are introduced into the culture medium. The luminous effect, it may be mentioned, is dependent on the presence of oxygen.

In referring to this subject, in my previous essay, I remarked, "it is clear that the luminosity of phosphorescent organisms is due to a vital process, as the effect is no longer noticeable after disintegrating the organisms at the temperature of liquid air." It may be pointed out that this passage should not be read too widely. Whilst it is to be supposed that the phosphorescing substance is a product of vital activity, it is not to be assumed that the phenomenon itself is dependent upon any "vital" process other than that involved in the production of the material. During my voyage to Australia in the summer of 1914, I made such experiments as I could with the luminous organisms in sea water. When these were collected in a flat dish and observed in the dark, flashes were rarely seen so long as the liquid was still; but on agitating the contents of the dish, luminous streaks frequently crossed the field, either because the organisms received some mechanical stimulus or because they were brought into contact with oxygen.

I found that on adding a little toluene or chloroform to the sea water, the flashing took place spontaneously and regularly, even in the still liquid, long after the organisms had been killed by the treatment.

When in Ceylon later on, I caught a number of fireflies and shut them up in a tube with a few drops of toulene, by which they were soon killed. Instead of being intermittently luminous, they then became permanently luminous and the luminous effect was still obvious three to four hours after death. The impression I gained was that the "luminous material" might well be the product of a slowly-decaying enzymic change.

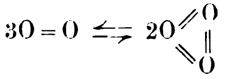
It is conceivable that the luminous appearance is the consequence of an oxidative change of an oscillatory character—even that it is conditioned by the formation of ozone. In this connexion, I may refer to the mention made in my previous essay (p. 385) of Sir James Dewar's fascinating demonstration of the luminous effect produced by the oxidation of phosphorus under certain conditions of low pressure. I there remarked: "It may be supposed either that some particular form of the phosphorous molecule is the origin of the glow or that it marks a stage at which some special oxide is formed." I have since been led to think that the "special oxide" may not be an oxide of phosphorus but *ozone*, which is a well-known "product" of the oxidation of phosphorous.

I am thus led to the demonstration given by Sir James Dewar on June 11, 1909, of the phosphorescent glow developed on passing an electric discharge through air or oxygen and allowing this to stream into a tall cylinder, placed on the plate of an air-pump, from which the contents are rapidly removed, so as to reduce the pressure to from 1 to 5 mm. of mercury, the air current rushing down at a velocity of about 1000 feet per second (Fig. 4).

A steady brilliant phosphorescent stream is seen down the whole length of the cylinder. Hydrogen and all volatile organic compounds containing hydrogen either stop or diminish the phosphorence.

Carbon dioxide gives a marked but much less intense glow. It is known from Sir Benjamin Brodie's experiments that ozone is produced on subjecting this gas to the electric discharge. Cyanogen and carbon tetrachloride vapour do not become luminous but both sulphur dioxide and carbon disulphide vapour give good phosphorescent streams at suitable tensions. The glow in highly-purified oxygen is short but distinctly more brilliant than that given by air.

There can be little doubt that the phosphorescent glow in oxygen or its compounds is due to the "production" of ozone. This gas, it may be added, is a coloured substance and reversibly convertible into oxygen :—



$$3\mathrm{O}=\mathrm{O} \rightleftarrows 2\mathrm{O}\begin{matrix}\nearrow\!\!\!\nearrow\mathrm{O}\\ \qquad\|\\ \searrow\!\!\!\searrow\mathrm{O}\end{matrix}$$

It is conceivable that phosphorescence may arise, as in the case of the fluorescent dye-stuffs, as a consequence of the repeated occurrence

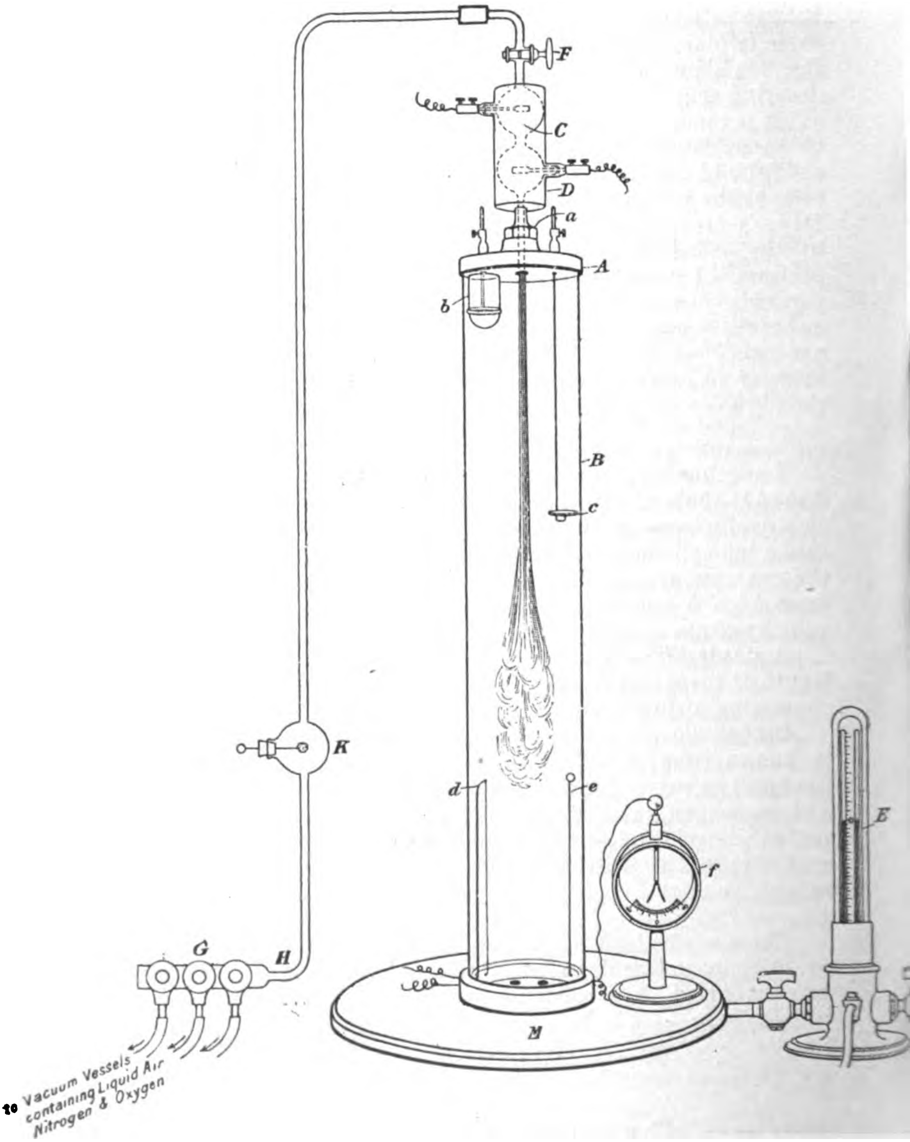

F
C
D
a
A
b
B
c
K
d
e
f
E
G
H
M
to Vacuum Vessels containing Liquid Air Nitrogen & Oxygen

FIG. 4.

of change in the two directions—from the coloured to the colourless systems and vice versa.

Sulphur being an element allied to oxygen, it is also conceivable that a like change may occur in its vapour. So far as I am aware, all known cases of chemical change in which luminosity is developed are cases of oxidative change.

The sequel to Sir James Dewar's lecture is remarkable. At the close of the abstract, it is stated that:—"Nitrogen obtained from the liquefied gas gives only a feeble and very diffused glow. This gas contains oxygen, a few per cent. of which causes the glow, as pure nitrogen gives no visible luminosity with this kind of electric discharge."

Further experiments with nitrogen, made by Professor Strutt in the course of 1912, led him to discover that nitrogen, like oxygen, is converted into a highly active form when subjected to an electric discharge of a peculiar character. In principle the apparatus he used does not differ from Sir James Dewar's but he differs from Sir James in his account in asserting that the gas becomes brilliantly luminous even in absence of oxygen. This conclusion was called in question by certain German chemists but was long maintained by Professor Strutt; latterly he has been inclined to retreat from this position and to admit that the glow may be dependent upon the presence of oxygen. Professor Strutt has not ascertained what proportion of the active form of nitrogen is produced nor whether the change be associated with any change in volume, as in the case of ozone; the volume should be doubled if change take place in accordance with the equation

$$N_2 \rightleftharpoons 2N$$

If the appearance of phosphorescence be in any way associated with reversible change in a system containing a coloured component, there is no reason to expect the production of a glow in nitrogen under the circumstances considered.

Prof. Strutt has stated that on cooling the activated gas, in a spherical bulb provided with an elongated, narrow tubular neck, by plunging the neck into liquid air, the period of luminosity is shortened. Hence he concludes that "the reunion of nitrogen atoms occurs more quickly the lower the temperature," discarding the obvious explanation that the more rapid change is due to the more local concentration of the gas in the cooled limb of the tube. Finally, he adds, "this is a unique instance of a chemical action being quickened by cooling": as a matter of fact, not a few gaseous interactions are quickened, within certain limits, by cooling; the case is in no way peculiar.

CALORIMETRIC STUDIES.

Reference was made, under this heading, in my previous essay, to liquid air and liquid hydrogen calorimeters and several examples were given of the use that had been made of them. In the present septennate, Sir James Dewar has made the liquid hydrogen calorimeter an instrument of precision and has used it in determining the specific heats, at about 50° absolute, of most of the elementary substances and of a large number of organic compounds. The instrument is fully described and figured in the abstract of the discourse delivered on January 23, 1914. In using it, the weighed substance is cooled in boiling nitrogen and afterwards dropped into an insulated calorimeter containing liquid hydrogen; the evaporated hydrogen is then collected and measured.

The results obtained with "elementary substances" are of an unexpected character. When the heat capacities of quantities corresponding to the atomic weights—in other words, the *atomic heats*—of the elements are compared, in the case of the metals, a nearly constant value approximating to six is found; the solid non-metallic "elements" alone give irregular and far lower values.

On the other hand, when the volumes occupied by atomic proportions—the *atomic volumes*—are contrasted, again at ordinary temperatures, the values rise and fall in periods in a most striking way, corresponding "elements" of the several families occupying corresponding positions in successive periods.

Sir James Dewar's atomic heat values at very low temperatures (50° abs.) are related in this latter manner, that is to say, the relationship resembles that in which the elementary atomic volumes determined at atmospheric temperatures are related. This remarkable relationship is shown in the accompanying curve (Fig. 5) :—

Such a result, it may be mentioned, had been in part foreshadowed as a consequence of certain modern speculations on the structure of atoms. On this account, the observations are of singular importance.

A detailed study of the specific heat of sodium in particular, at various temperatures, has been made by Principal Griffiths, which is of special interest in view of Sir James Dewar's results. The recent lecture by Mr. Jeans should also be read in this connexion.

The so-called elements, it is well known, are now no longer regarded as simple, since the discovery of radium and other radioactive materials.

It may be noted that chemists have long looked forward to such a development.

Thus I may quote the following statement made by myself, in submitting a new scheme which I have suggested for the "Classification of the Elements," as recently as March 1902, though I have had the conception in mind since I became aware of the great

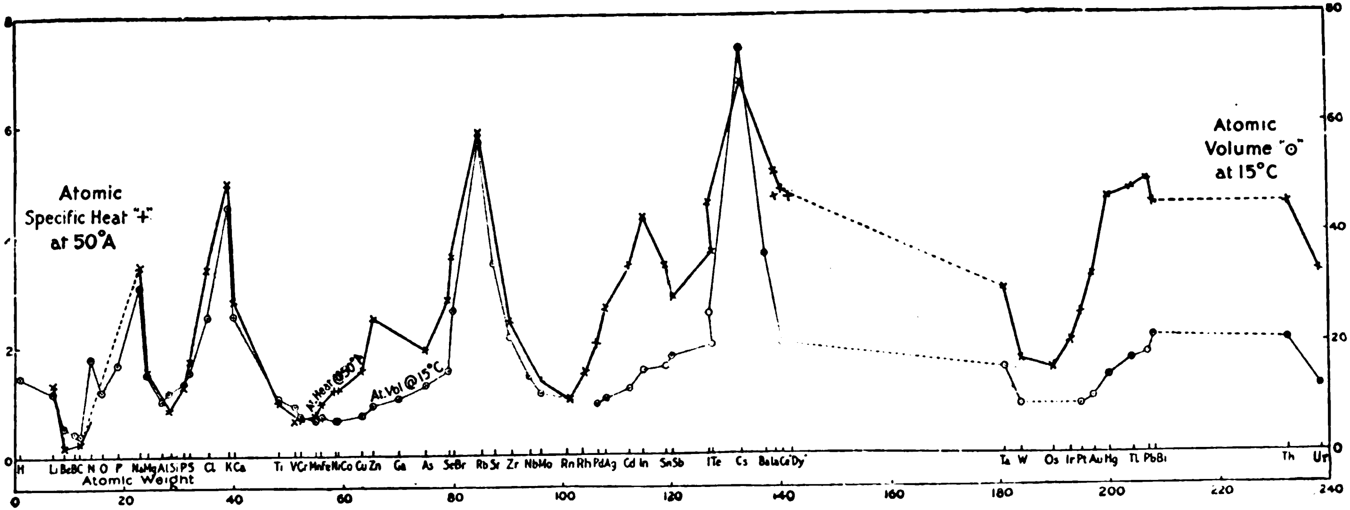

Atomic
Specific Heat "+"
at 50°A
Atomic
Volume "o"
at 15°C
At. Heat @ 50°A
At. Vol @ 15°C
Atomic Weight
H Li Be B C N O F Na Mg Al Si P S Cl K Ca Ti V Cr Mn Fe Ni Co Cu Zn Ga As Se Br Rb Sr Zr Nb Mo Rn Rh Pd Ag Cd In Sn Sb I Te Cs Ba La Ce Dy Ta W Os Ir Pt Au Hg Tl Pb Bi Th Ur
0
2
6
8
0
20
40
60
80
0
20
40
60
80
100
120
140
160
180
200
220
240

FIG. 5.

Russian's generalizations, having always been struck by the resemblance brought out by the "periodic" arrangement of elements with those presented by carbon compounds when classified according to their homologous and isologous relationships :—

"Although no direct evidence acceptable to chemists has been adduced which in any way justifies the belief that the elements are decomposable, it is impossible to resist the conclusion that they are genetically related—so closely in many respects do they resemble a series of related compounds, especially when regarded from the point of view of the organic chemist. The generalization known as the *periodic law* is in itself a justification of this view, the manner in which inter-relationship becomes manifest when they are classified in accordance with its canons being probably the strongest of all the arguments which can be cited as tending to show that the elements are compounds—but compounds very different from those with which we are accustomed to deal" (R. Soc. Proc., vol. 70, 86 ; vol. 87, 204.)

The subject was further developed in the Graham Memorial Lecture in 1912 (Glasgow Philosophical Society, Proceedings; cp. Science Progress, vi. 584).

Sir James Dewar has not yet placed on record the results he has obtained with organic compounds but I know these are of a remarkable and most suggestive character. The study of such compounds is of altogether special importance, now that the simplicity of the primary materials from which they are formed is suspect.

The most remarkable feature of all the cases of spontaneous decomposition of radio-active materials hitherto studied is the production of *helium* gas as one of the products. In so far, therefore, as known primary materials have been shown to be of a complex nature, they are proved to be compounds of helium. They may, in fact, be contrasted with the hydrocarbons. As helium is an entirely inert gas, it is difficult to believe that it is present "as such" ; I am still inclined to think that the molecule will ultimately be found to be complex, just as hydrogen, as we know it, is a complex molecule (H_2), the constituent radicle of the molecule (H) being the combining unit present in hydrogen compounds. Indeed, while accepting most of the conclusions based upon the study of radio-active change, chemists cannot well adopt forthwith all the views put forward by the distinguished school which has added so wonderful a new chapter to our knowledge and has so revolutionized our views of nature.

Sir James Dewar's latest determinations of the rate at which helium is given off from radium have been made known during the septennate. They show it to be slightly under one-half of a cubic millimetre per day. So far as is known, the decomposition of radium and similar materials is an entirely spontaneous process dictated by some inner necessity—one which cannot be either assisted or controlled by any means at our disposal. No sufficient explanation has yet been given of this astounding behaviour; chemical precedent would lead us to think that the decomposition may be akin to that of substances which undergo endothermic change, because the rearrangement of the constituents is one involving a degradation of energy, as in the case of acetylene:—

$$x\,C_2H_2 = 2\,x\,C + 2\,x\,H = 2\,C_x + x\,H_2.$$

But in so far as the production of helium is concerned, this is denied by physicists.

The occurrence of such changes is known to be conditioned by "impurity." Highly purified nitrogen chloride is non-explosive; it may be distilled. If radio-active change be unconditioned, it again is a departure from all other forms of chemical change.

The problems opened up by the discovery of helium are indeed transcendental. Both chemically and physically it is the least responsive of gases: one in comparison with which hydrogen is eminently active, though below it in molecular weight and volatility. The temperature difference between boiling-points of the two materials is relatively small and yet an interval is included within which marvellous changes in the properties of other materials are likely to occur. It is deeply to be regretted that helium is so scarce and that the expense and difficulties attending its use as a refrigerating agent must be so great. So much having been accomplished with hydrogen in the Institution laboratories, it may be hoped, however, that means will yet be found of extending the inquiry into a field of the unknown wherein miracles may well be realized.

Prof. Soddy has so fully discussed the problem of radio-active change in his recent fascinating and masterly lecture on "The Complexity of the Chemical Elements" that little more can be said with advantage here. We are placed in a difficulty. Since the discovery of radium the term "element" has been deprived of its verbal significance; so also has the term "atom." If our language is to be an accurate expression of our thought, either the definition of these terms must be altered or we must devise new ones.

Apparently the decomposition of radio-active materials may give rise to substances which are indistinguishable in chemical behaviour, although they differ in their combining proportions and some physical aspects. Materials so related are termed *isotopes* by Prof. Soddy: these correspond to the related or homologous members of a family contemplated in my scheme, which even provides for the existence of isomeric terms in different families. They are comparable also with the homologous terms in a series of hydrocarbons, such as the paraffins. I have suggested that tellurium is the inferior homologue or isotope of a missing "element" bearing the weight-relationship to iodine that a material having the properties of tellurium may be expected to present in the light of Mendeléeff's generalization. There are several places available in my scheme for lead isotopes.

Prof. Soddy contemplates the possible rectification of the accepted combining proportions through the discovery that primary materials now regarded as homogeneous are in reality mixtures of isotopes—as in the case of lead. I have already made a like suggestion in the paper referred to above :—

"It is perhaps not beyond the region of possibility, however, even in the case of an element with so low an atomic weight (as that of beryllium), that isomorphous mixtures may have passed as pure substances ; this possibility has not been sufficiently taken into account in preparing the materials for determinations of atomic weight."

But on general grounds and in the light of a scheme such as that I have ventured to put forward, it is difficult to believe that all the primaries other than isotopes have been discovered—in other words, that the list of elements is practically complete and that we know the possible number. Many years ago, Sir Benjamin Brodie drew attention to the fact that in the case of carbon compounds certain types appeared to prevail; this may well be true of the primary materials. But as in the case of radio-active substances, discovery may still wait upon the evolution of suitable methods of detection and a chance discovery of some highly localized material, as in the case of germanium, is in no way precluded.

GASES OF THE ATMOSPHERE.

Gases such as those given off from the Bath Spring are found to contain helium and other rare gases in unusually large proportions.

In the course of the examination of the rare gases derived from these Springs, unidentified lines have been observed in the spectra the origin of which cannot at present be explained. Astronomical observation has also involved the detection of lines which cannot be assigned to any known source. On these grounds continued study of natural gases is desirable. The methods of separating minute quantities of difficultly-condensable gases, whether by mere cooling or with the aid of charcoal, have been materially improved of late and specially described in the lectures given in 1914 and 1915.

One result of the inquiry has been to show that the proportion of hydrogen in the atmosphere has been much exaggerated, apparently owing to the use of metal tubes in collecting and conveying the air examined.

Sir James Dewar finds less than one part of this gas in a million of atmospheric air. On the other hand, he has shown that respired air may contain a considerable amount of hydrogen, the amount varying from one person to another ; in one case he rates the amount exhaled to have been about 250 c.cm. per day but only 100 c.cm. in his own case. Incidentally, it was noticed that all the methods in use for removing oxygen from air by means of reducing agents give rise to the production of a small proportion of hydrogen.

As many organisms incite fermentations which give rise to hydrogen, it is only to be expected that this gas should be constantly produced under natural conditions.

In studying the composition of the gases condensed from air by charcoal at liquid-air and liquid-hydrogen temperatures, a series of significant observations have been made on alterations in the equilibrium following variations in the composition of the condensed material; what appear to be new conditions of relative volatility have been noticed which are indicative of some sort of molecular aggregation, for example, between hydrogen and nitrogen or carbonic oxide.

DIFFUSION PHENOMENA.

In a recent lecture (1915) reminiscent of Delville and also of Graham, a series of striking illustrations were given of the diffusion of hydrogen through heated platinum and palladium, showing also the different extents to which this gas is present in the flame of a Meker burner, at different heights. The diffusion of helium and somewhat less readily of neon and scarcely of hydrogen through heated quartz was also demonstrated.

An auspicious foundation has also been laid of a method—of course involving the use of liquid air—of studying diffusion through a film of indiarubber, only about 0·01 mm. thick, under a pressure difference of one atmosphere between the two sides. Hydrogen passes through such a film at four times the rate of air and carbon dioxide at least ten times as fast. It is also permeable by water vapour, which passes through about three times as rapidly as air. Ammonia and the vapours of chloroform and ether permeate the membrane very quickly.

CARBON MONOSULPHIDE.

A chemical inquiry of great importance, only possible with the assistance of liquid air, is that relating to carbon monosulphide, CS, the sulphur compound corresponding to carbon monoxide, CO. Many unsuccessful attempts have been made in the past to prepare the compound. All ordinary methods have led to the production, at most, of a solid substance which is clearly a condensed or polymerized form $(CS)_x$ of the true monosulphide.

The boiling-point of carbon monoxide being − 190° and its melting-point − 210°, whilst carbon dioxide boils at − 78° and melts at − 57°, carbon disulphide boiling at 40°, it was to be anticipated that the monosulphide would also be a highly volatile gas.

The method adopted has been to subject the vapour of carbon disulphide to the action of an electric discharge in an ozonizer and then to pass the modified vapour into a vessel cooled by means of liquid air. The product condenses as a white solid, which rapidly becomes brown even at the low temperature of liquid air; on removing this latter, the transformation into the polymeride is effected so rapidly that detonation takes place and the vessel is shattered. Apparently the monosulphide condenses only below − 120°, though above − 185°. The most striking chemical property of the gas is its power of reducing sulphuric acid.

The discovery of carbon monosulphide is a most welcome addition to our knowledge of the simple carbon compounds. Its properties are such as might fairly have been anticipated, in view of the remarkable tendency of sulphur to unite with itself.

In the light of this discovery, however, the properties of carbon monoxide must be regarded as more than ever remarkable. That a single atom of carbon should suffice to neutralize, and be itself so nearly neutralized by, a single atom of oxygen is probably one of the most surprising facts in organic chemistry. It is especially remarkable that such a compound should show no tendency to polymerize and be generally so inert, whilst its sulphur analogue cannot be prevented from falling a sacrifice to the instinct of self-combination. But chemical science is full of riddles and whilst many have been guessed in the Royal Institution laboratories, not a few have been propounded to which answers will not easily be found.

THE FUTURE OF ORIGINAL EXPERIMENTAL RESEARCH.

The privilege of surveying a long course of lectures such as those on which I have commented all too superficially, especially after having had the good fortune to attend, I believe, the entire series, is an opportunity such as few can have. I wish I could say that I had shared my opportunities with a far larger number directly interested in physical-chemical inquiry than I know have enjoyed them with me.

In looking back across the interval of time during which I have known the Royal Institution, when I consider the benefit I have had from attendance at the discourses and from watching the advance of the inquiries carried on within its walls, what I am most concerned to know is :—how scientific inquiry may best be promoted, how the educational advantages so lavishly dispensed by the Institution may be sufficiently appreciated in days to come.

At a time when the value of science to the State is so much insisted upon and gradually but all too slowly and imperfectly realized, the part the Institution has played in advancing scientific knowledge should be taken specially into account and more widely known. The educational facilities it offers have yet to be appreciated in a proper manner. I can speak from a long experience and can bear personal witness of the great value the lectures were to me even as a student. My connexion with the Institution dates back to 1867, when I was working at the Royal College of Chemistry, in Oxford Street, under Frankland, who was also Professor of Chemistry at the Royal Institution. We were then engaged in developing a new method of water analysis, which involved much gas analysis. A very beautiful instrument for such work, devised by Frankland and Ward, was mounted in one of the windows of the present store-room on the ground floor of the Institution; during the latter part of 1866 and the earlier part of 1867, I almost daily carried tubes in which gas had been collected over mercury from Oxford Street down Bond Street to the Institution to analyse them with the aid of this apparatus. This led to my frequent attendance at the lectures. In those days Tyndall was at the height of his career as a public lecturer: his lucidity was wonderful, the perfection of his demonstrations marvellous. The educational value of such lectures cannot be exaggerated: we need, in some way, to recover the art which Tyndall practised so successfully—perhaps this may come when we rescue some of the intelligence of the country from the bar and divert it into science, at least there is an opportunity for some wholehearted attempt to reproduce an atmosphere such as Faraday created, that of simple wonderment and reverent inquiry into the marvels of Nature.

But Faraday and Tyndall were at pains to be explicit and clear. The modern lecturer often lacks breadth of outlook and is too rapid, too special and technical in his treatment of his theme, therefore above his audience. Far too often results are made known but no inkling given of the method followed in obtaining them.

Undoubtedly, lectures on scientific subjects do not attract in the way they did formerly—perhaps this is because science has been admitted into society and is no longer a Bohemian occupation but almost one of respectability: or because it has vanquished ecclesiasticism and freedom of opinion is no longer denied us—this fight being over, public attention is not aroused by our disputes.

Faraday's address, entitled "Observations on the Inertia of the Mind," read at the City Philosophical Society in 1818, may well be studied in this connexion. In it he says that—

"Man is an improving animal. . . . In knowledge, that man only is to be contemned and despised who is not in a state of transition. We are by our nature progressive."

Modern education apparently is not based upon such a view. The failure of our "public schools" to develop a desire for knowledge and to inculcate the habit of inquiry has been absolute: nor is this surprising, seeing that almost everywhere they have been in the hands of men whose whole training has taught them to reverence the past rather than to gaze into the future. As a nation we are proved to be strangely lacking in foresight and most narrow in our outlook. But this may be an acquired state, not congenital. And as, in the past, our schools have been in the hands of men who have little desire for progress, we must at least make the experiment of placing them under the control of persons who will seek to develop a system more in harmony with the times, whereby our innate disabilities may be minimised, if not overcome. There will be no progress so long as the clerical head-master is retained: being pledged to accept certain dogmas, his mind is necessarily unscientific and cannot be open. The advocate of classical study, who regards these as all sufficient and superior to every other, is in a very similar position—even worse, because he is under no professional obligation and the mere creature of habit.

The case for immediate reform is absolute. The development of scientific appliances during the war, the many directions in which scientific knowledge has been of avail, have led the public suddenly to become alive to the need of developing scientific habits of thought and of utilizing scientific knowledge to the full. It is realized that the industrial if not the social future of the country is largely a question of the extent to which we can make practical use of scientific ability. Experience seems to prove, however, that this type of ability is a very special and rare form: on this account, it is imperative that those who are gifted with scientific attributes should, as far as possible, be recognized at an early age: so that their talents may be developed, not obscured; that they may be directed into, not diverted from, the path of inquiry and their apathy overcome.

Faraday, a century ago, with wonderful perspicacity, considering his youth, cautioned his fellow-members of the Philosophical Society against allowing apathy to prevail: our cry to-day must be the same; apathy is clearly at the root of most of our difficulties.

The youthful philosopher, however, though professing optimism, could not but be logical and recognize the rocks ahead; for after saying—

"Whatever be the reason, the melancholy truth is evident, that we are fit for the noblest purposes but that we fulfil them not,"

he ends on a note of pessimism:—

"In pursuing the analogy in my own mind of this general influence [of inertia] to which both matter and mind are subjected, I was led to a conclusion respecting mental inertia, which, though I have no reason to doubt, I should be fearful of uttering on my own authority alone. I will therefore put it in the form of a query; . . . Inertia has a sway as absolute in natural philosophy over moving bodies as over those at rest. It therefore does not retard motion or change but is as frequently active in continuing that state as in opposing it. Now, is this the case with mental inertia?

"That I may ask the question more distinctly, I will preface it by two others, which, if disallowed, will give rise to conversation; if allowed, will prepare for the third. Are there not *more passive* than active minds in the world? Is mental inertia as puissant in active as in idle cases? Then, What is the cause of the state implied by the first question? or what is the reason why, unlike the material world there is so much more of inanimation than of activity in the intellectual world?"

Our reply to his long since uttered query unfortunately cannot be very different from the answer of which he was apprehensive.

It is clear that we have to bear in mind the conditions under which we are forced to work—the more as so much virility is now being sacrificed. The demand for scientific workers in years to come is bound to be excessive: in this very fact lurks danger. It may lead to a great shortening of the course of training; it may bring far too many into an early state of subservience; it may involve an undue attention to the field of applied science: consequently to a diminution of effort to widen the boundaries of knowledge and disregard of the pursuit of truth as the primary objective.

A laboratory such as that of the Royal Institution has been a pearl of great price in the past; in the future, it may well be a glowing gem in an otherwise featureless casket, if the policy now foreshadowed and advocated of coupling University instruction directly with practical aims be brought into operation; not only will the outlook of the schools be materialized and commercialized but their efficiency will also be greatly lowered. I have myself been a witness of the decadence of science, not here but in Germany, through the operation of such a policy: the while I have seen it grow in usefulness—but by continual encroachment upon capital.

In my student days, before the war of 1870, "free trade" prevailed in the laboratory: no two of us were working at the same subject; a multiplicity of problems were under investigation; we were all eager to learn each from the others; we told one another openly our hopes and kept one another constantly informed as to the progress we were making. The value of the training, the experience gained, was proportional, consequently, to the number of students engaged in research work.

Kolbe's laboratory discovery in 1874 of a practical method of manufacturing salicylic acid and its rapid translation into practice, more than any other work, perhaps, brought confusion into the chemical camp. It became obvious that there was money in chemistry and manufacturers saw that the University Professor might be of value to industry: so they retained him and his assistants and subsidized them, on condition that the works should have the first offer of their results and that publication should be arrested until it was clear that they were not of immediate practical value. The atmosphere of the laboratory was entirely changed in consequence: it became materialized; instead of being one of light and learning and of breadth and freedom, it became one of secrecy and essentially narrow. Each assistant had his set of pupils and these were pledged not to talk freely, as of old, of their results; the commercial attitude of mind took the place of the old belief in science for its own sake.

Our schools have been, in the main, free from this taint, because manufacturers have made no systematic use of them. This may have been bad for our manufacturers; it may have had the result that too few have been attracted into science, because it did not pay; but it has been good for science, in the sense that we have been free to work and develop in whatever directions we chose. The English chemical school, of late years, small as it may have been, influenced to an excess as it has been by the German school of thought, has none the less been far broader and more independent than any of the others.

Our difficulty will be, while forwarding the aims of industry, to maintain our independence and freedom—so that we may continue in the path of discovery and extend the bounds of knowledge. The danger that we may become the slaves of industry is far from remote. The temptation will be the harder to resist in consequence of the State patronage and interference with which we are now threatened. A department of Government has been called into being, with a round million sterling at its command, to promote industrial research. The immediate progress of industry may conceivably be promoted, in great degree, by such a body, under present conditions; though ultimately, unless German and American example be followed and scientific research be regarded as a necessary adjunct of the factory, little real progress will be made, because a scientific atmosphere will not have been created within the works. Moreover the results of effective industrial research must necessarily be kept private and cannot even be made known throughout a trade—unless competition be altogether done away with, an unlikely event.

To the lay mind, the organization of research may seem to offer not a few advantages, especially in view of the complaint made so frequently of late by scientific workers that original inquiry has not been sufficiently promoted in the past. But it must always be under the condition that grants are made to workers as a right rather than as a favour. The scientific fraternity will sign its own death warrant if it consent to the introduction of any system of bureaucratic control which might affect the very limited class who alone are capable of truly original work, such as were Rumford, Young, Davy and Faraday. Whatever system of organization be developed, the one necessary condition of success will be that it rest in the hands of those who have intimate understanding of the work and are therefore fully alive to its requirements and difficulties.

The experience of ages goes to show that Art of every kind is a matter of individual expression and that it cannot brook control. All original scientific inquiry worthy of the name is subject to this same condition. Money must be given to competent inquirers almost unconditionally to be of service.

The Royal Institution hitherto has been the privileged home of scientific activity: all attempts to rival it have been unsuccessful, because elsewhere control has been exercised and imagination has been wanting: work such as has been carried on within its walls cannot be done to order. Even the maintenance of the favourable conditions of the past may be very difficult: it is all the more necessary, therefore, that the endeavour be made to understand what these have been, that at least they may be sustained and if possible improved, so that the example first set by Davy and Faraday may ever be a living influence and that the laboratories may be devoted to reflective study and research, the home of men without desire for advertisement such as Carlyle would have included among heroes had he dealt with science.

"The great *silent* men! Looking round on the noisy inanity of the world, words with little meaning, actions with little worth, one loves to reflect on the great Empire of *silence*. The noble silent men, scattered here and there, each in his department; silently thinking, silently working; whom no Morning Newspaper makes mention of! They are the salt of the Earth. A country that has none or few of these is in a bad way."

There is grave danger that our country may gain few of these in the future, unless greater care be taken to establish conditions conducive to their special welfare.

WEEKLY EVENING MEETING,

Friday, January 21, 1916.

His Grace the Duke of Northumberland, P.C., K.G., LL.D., F.R.S., President, in the Chair.

Professor Sir James Dewar, M.A., LL.D., D.Sc., F.R.S., M.R.I., Fullerian Professor of Chemistry.

Problems in Capillarity.

Studies in gas diffusion described in last year's discourse have lead to the examination of films of great tenuity for such enquiries. These films can be got in the solid state through the use of solutions of nitro-cotton in certain solvents, or in the liquid state by employing Plateau's soap solution. The object of the present Discourse is to describe the result of some attempts to produce apparatus suitable for the study of the properties of such films, along with an account of the results so far obtained.

Liquid films thin enough to show colours are produced when drops of a large number of organic substances are allowed to touch the surface of pure water. The apparatus adapted for demonstrating the colour bands and also for making solid films is shown in Fig. 1. A copper funnel A of 10 cm. diameter, blackened by coating with nitrate of copper and subsequent heating to redness, is connected to the Institution water supply in order to obtain an easily renewable and well-defined clean water surface. The funnel is fixed in a blackened hollow metal conical stand B shown in section; this stands on a shelf over a sink. A parallel beam of light is directed down at about 40° inclination, and, after reflection from the liquid surface in the funnel, is focused on the screen. The best definition is obtained by allowing the water surface to fall a little below the rim of the funnel, for which purpose a two-way tap is connected below. It is essential that a fresh surface be produced by flooding with water between each experiment. A small drop of a suitable liquid on the end of a glass rod is made to touch the water in the funnel near the centre of the surface. The difference of surface tension between the water and the liquid drop results in the latter being spread to a circular film thin enough to produce colour bands in the same definite order exhibited by soap films. The production and development of the colouring is generally very rapid, so that the final thinnest stage of blackness is quickly reached. Pinene, turpentine, chlor-benzene, tetra-hydro-quinoline and olive oil are a set of bodies that show marked contrasts.

SOLID COLLODION FILMS.

A solution of nitrated cotton can be readily made in amyl acetate. If a drop of this solution is allowed to touch the water surface, a circular film showing the rings of colour steadily spreads out. The amyl acetate quickly evaporates, as shown by the regular disappearance of the colours, and an extremely thin black solid film of the nitro-cotton is left. This can be folded over, showing the stages of colour produced by increasing thickness. If a flat ring support is carefully slipped under the film it can, by careful handling, be completely removed by a drawing motion at a steadily increasing elevation. Black films 15 cm. in diameter can be so obtained. They are easily dried, and quite permanent with careful handling. They can be weighed, and their thickness thereby estimated. For example, the mean weight of several films 11 cm. in diameter was found to be 0·432 mgms.; the density of the collodion cotton being 1·60, it follows that the thickness of the black film, if uniform, will be of the order of 3 × 10^{-6} cm., i.e. 30 $\mu\mu$. The average thickness of a film can also be found by knowing the weight of a drop of a collodion solution in a known concentration, and

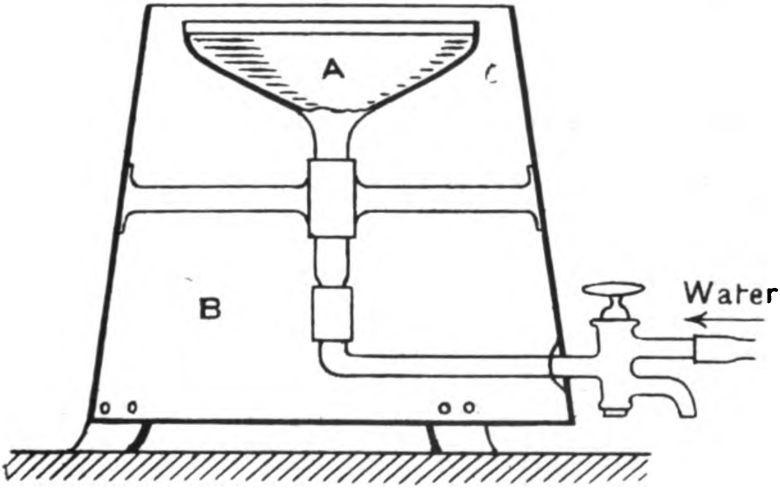


FIG. 1.

subsequently increasing the diameter of the large circular film produced.

For studying the transit of gases films about 5 mm. in diameter were used. They were mounted on a thick-walled glass tube ground flat and polished at one end. The tube was fixed vertically and centrally under the water surface before making the film. The water is afterwards allowed to flow slowly out, carrying the film down, thereby folding it securely and neatly over the ground end of the tube. The water level inside and outside the tube must be kept the same by a suitable air inlet, otherwise the suction may break the film. By this means two, three or more black films can be overlaid, and different coloured films obtained.

In order to investigate the passage of air through such films a small pressure was introduced into the tube supporting the film, and the diminution with time measured on a sensitive alcohol displacement manometer. A soap bubble was used to supply a small regulated pressure.

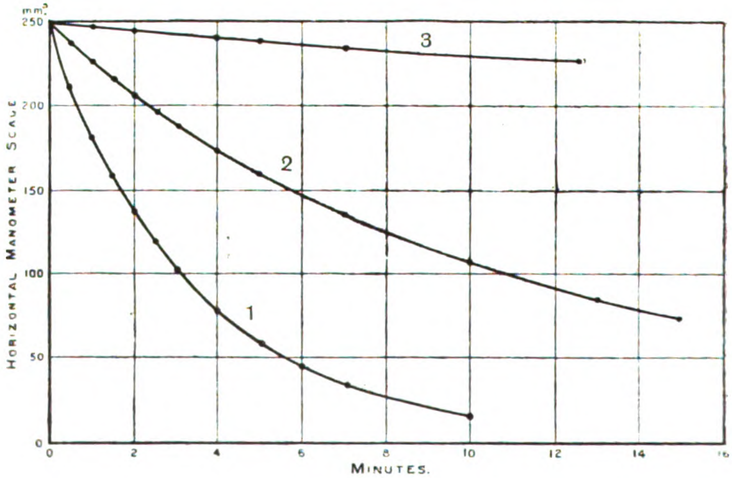


Fig. 2.

The above graph (Fig. 2) shows the observed fall of pressure (p) as ordinates, with time (t) as abscissæ, got from observations from three separate films freshly dried in a warm oven before measurement.

Curve No. 1 is the graph of one film; Nos. 2 and 3 of a double and triple film. In all cases the fall of pressure was logarithmic. That is to say, when log p is plotted with t a straight line graph is obtained: from whence we deduce that the rate of diminution of pressure is proportional to the pressure.

The actual quantities dealt with in a typical instance are as follows. 50 mm. of the alcohol manometer scale are equivalent to 1 mm. water pressure:—

Observations of pressure with time were:

P (mm. H_2O)	4·4	3·15	2·4	1·31	0·82	0·44
t (minutes)	0	½	1	2	3	4

Volume of enclosed space = 30 c.c.

Area of membrane = 0·049 sq. cm. (0·25 cm. diameter).

Fall of pressure in first ½ minute = 1·25 mm. water = 0·00012 atmosphere.

∴ Volume of air passed in first ½ minute = 30 × 0·00012 = 0·0036 c.c.
= 10·45 cc. per day.

∴ Volume passed through 1 sq. cm. in 1 day = $\frac{10·45}{0·049}$ = 209 c.c.

The mean water pressure during experiment was 3·8 mm., or 0·00037 atmosphere.

On the assumption that the rate of diffusion remains proportional to the pressure up to one atmosphere, then the extrapolated value of the rate per sq. cm. per day at atmospheric pressure would be 565 litres.

A similar determination from the fall of pressure in the second whole minute gives a value of 930 c.c. per day per sq. cm. at a mean pressure of 1·86 mm. of water. If this be extrapolated to one atmosphere, the rate per sq. cm. becomes 520 litres.

Compared with this, the rubber membrane 0·001 cm. thick had a rate of 2 c.c. of air per sq. cm. per day. The mean thickness of the black collodion film is 0·000003 cm. or $\frac{3}{1000}$ths of the rubber. The rate of air transit through the collodion at one atmosphere appears thus to be 260,000 times that of rubber. The square of the ratio of the thicknesses is roughly of the same order as the inverse ratio of the diffusion rates.

The corresponding extrapolated values at one atmosphere were 592 for a single black film, 36 for the double film (whose colour appeared very pale golden), and just under 1 for the triple film (reflecting a brown-magenta colour). The extraordinary diminution of gas percolation thus observed is very remarkable.

COLLODION FILMS: MICROSCOPIC EXAMINATION.

A microscopic examination seems to throw some light on this question of gas transit. A regular distribution of minute bubbles is revealed, which occur largely in colonies composed of one or two larger bubbles with ten or twenty very small ones surrounding them like small patches of froth. These patches seldom exceed 0·025 mm.

in width; the larger bubbles are not often over 0·008 mm. diam. The appearance of the colonies are roughly represented in Fig. 3.

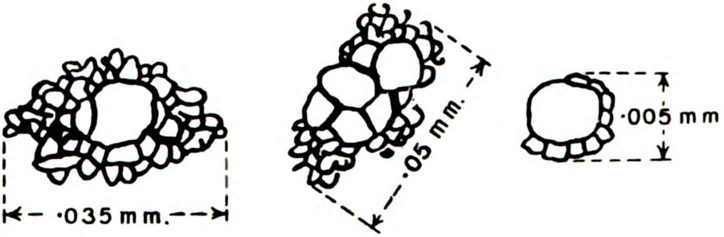


FIG. 3.

Between the fairly equally spaced colonies there was a regular distribution of smaller bubbles of from 0·005 to 0·001 mm. Among these a small proportion were colonies, but of a low order of aggregation.

In one straight line of length, 1·53 mm. across a film, there were:—

10	colonies	of from 0·01	to 0·02	mm. ...	Total = 0·15
15	,,	,, 0·005	,, 0·01	,, ...	,, = 0·11
about 50	,,	about 0·001	,, 0·003	,, ...	,, = 0·10
					0·36 mm.

The proportionate length of bubbles = 0·36 mm. in 1·53 mm. of film, or 23½ per cent, so that the total relative area of bubbles to continuous film = $(0\cdot235)^2 = 0\cdot055$, or 5½ per cent.

The appearance of double and triple films under the microscope was simply that of a more thickly sown appearance of the bubbles. One perfectly black film 1½ cm. diam. showed much smaller bubbles, and much more thinly spread; the most numerous were 0·004 mm. diam. In a slightly thicker film the bubbles are somewhat larger.

From the dimensions of these bubbles and colonies it is evident that they must be very flattened in shape in the film, because its thickness is at most one-hundredth of the length of the bubbles. The upper and lower bounding surfaces of these bubbles must, therefore, be extremely thin relatively to the mean film thickness. This suggests that the main diffusion most likely occurs through these extremely thin portions of the film. When two films are superposed, the bubbles will not be likely to coincide to any great extent, as they only occupy about 5½ per cent of each film. The exaggerated diffusion through the thin places will thus be largely checked. The addition of a third film would further reduce this by a similar factor, so that the diffusion would diminish fairly geometrically. In the example given above the relative rate of the double film was $\frac{1}{17}$th that of the single film, while the addition of a third film again reduced this by $\frac{1}{36}$th.

If these bubbles were actual holes, then the gas transit would be that of simple transpiration. This would give a curve of an entirely different character to those shown in Fig. 2. The plots of log. p and t would no longer give a straight line, as the rate of gas transit is then proportional to the square root of the pressure. This was actually tried on a capillary tube, when the appropriate curve of diminution of pressure with time was obtained.

The bursting pressure of a collodion black film of double thickness was found by connecting the open end of the polished glass tube on which it was mounted, as described above, to a mercury manometer. Air pressure was gently introduced, and the height of the mercury pressure required to burst the film observed. As an example, a film of double thickness, 3½ mm. in diam., burst at 3 cm. pressure of mercury, corresponding to a load of 40 grms. weight per sq. cm.

SOAP FILMS IN EXHAUSTED VESSELS.

Films of soap solution can readily be obtained of the same order of thickness as those from collodion. When produced in exhausted vessels they are freed from many disturbing agencies. The deleterious effects of suspended matter and other air impurities are avoided; convection currents and air friction disappear; and the motions in the film are rendered more definite. The solution being free from dissolved gases, the absence of oxygen and carbonic acid stabilises the soap solution, which, however, may in time be damaged by attack on the glass. Any froth that may be produced rapidly subsides, because the bubbles are formed only of water vapour, which condenses quickly owing to the higher pressure inside the bubbles.

The three methods of film production in exhausted vessels most frequently used are:—

(*a*) Production of bubbles of water vapour from the slightly warmed soap solution, or by violently shaking the liquid until it becomes a mass of froth and allowing to stand.

(*b*) "Throwing" the film by a smooth and rapid circular swish of the soap solution over and round a constricted neck; or across a cylindrical vessel.

(*c*) Immersing, or otherwise covering, a wire or glass framework, which is thus left holding a film when the soap solution is carefully withdrawn from it.

The first method is applicable to an ordinary distilling flask, or plain exhausted tube, such as are shown in Fig. 4. The flask has a capacity of about a litre, and the straight tube was about 60 cm. in length and 4 cm. in diameter. Both are sealed after exhausting to a fraction of a mm., the part containing the soap solution being meanwhile cooled to about -20° C. When the bulb A is then gently warmed on either side by the finger and thumb, a regular succession of bubbles rise from two opposite points of A, which are thus at a slightly higher temperature. The growing bubbles pass in their expansion to and fro across the liquid surface almost with

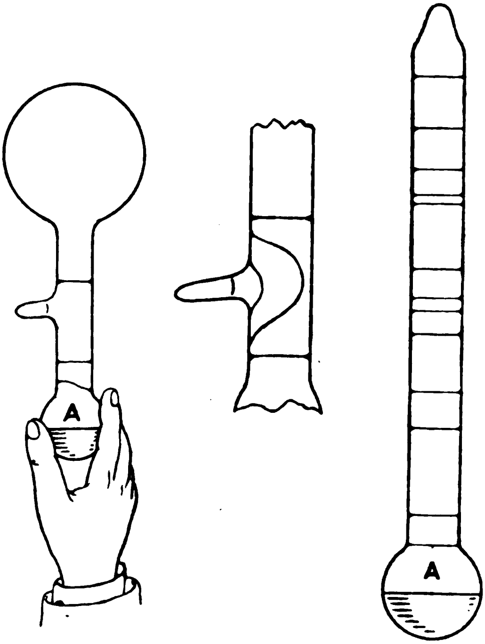


FIG. 4.

the regularity of a pendulum, and rise up the tube as a series of flat films. In a similar manner, if the side tube (filled with the solution) be warmed by the fingers, saddle-backed films emerge and divide into flat films up and down the straight neck (see Fig. 9). An alternative method more applicable to the straight tube form is to agitate the liquid until it is largely a mass of froth, the bulb A being warmed in the hand during the shaking. When the tube is then set vertical, the slight warmth imparted to the liquid mass in A causes a steady and extensive evolution of interlacing bubbles to pass up the tube. These bubbles soon settle to a succession of flat films, thirty being frequently obtained in such a tube. The films thin down to the black stage in a few hours at most, and slowly descend in the tube during many days, provided the temperature

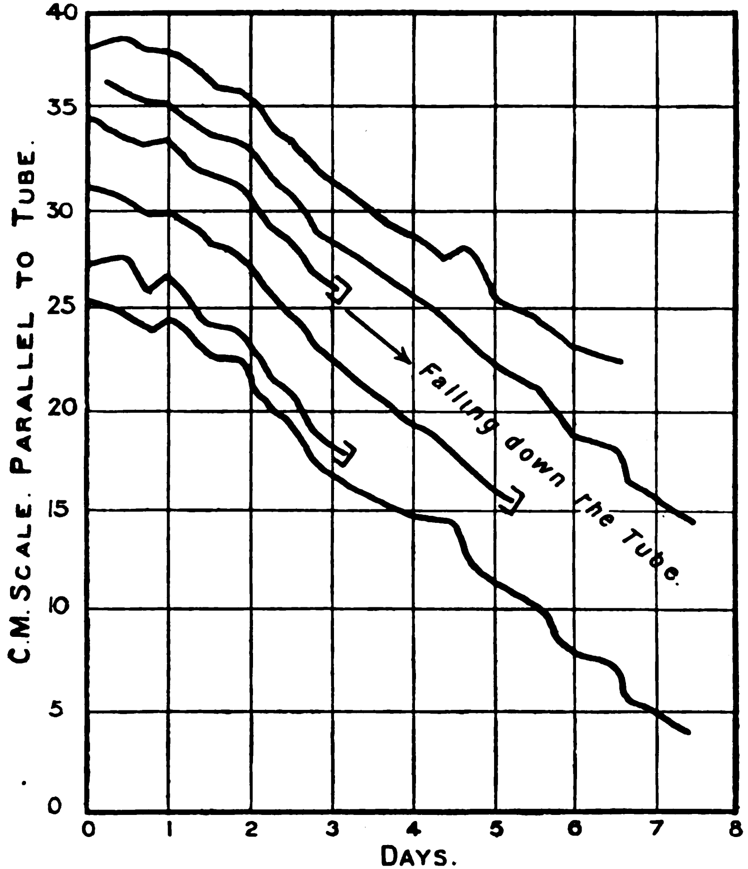


Fig. 5.

remains constant. They are instantly responsive to any slight alteration of temperature, being so sensitive that the approach of the hand is sufficient to cause movement, from the minute alteration of vapour pressure produced by the small increase of heat radiation.

If one film be broken, the remainder rearrange their positions, those nearest the disturbance being most affected.

Examination of the manner of the fall of the films in the tube shows that the rate is approximately steady through the greater part of the tube, when not too many films are present. As the films become more crowded in the lower part, the rate of fall decreases. If the tube is not strictly cylindrical, then, provided the narrower part is above, the films may either remain almost steady, or if the difference of diameter is sufficient, the films will steadily rise; they can thus indicate differences from uniformity of section, too small to be perceptible to the eye. In a similar way small irregularities or slight unevenness of surface are indicated by a proportionate distortion in the film itself from a perfect plane, because the film sets perpendicular to the supporting surface. Variations of temperature are indicated by an interruption to the smooth progress of the films through the tube. Thus, in a quiet room, where the temperature is falling, the mass of liquid will cool more slowly than the thin glass walls and supporting films; and a slight evolution of water vapour rising from the liquid will drive the films up the tube. Such variations are well shown in a graph of continuous records of such a set of films. (Fig. 5.)

The draining of the liquid from the tube walls is well exhibited by the steady decrease of thickness of the liquid rings of contact around each film. The interactions, in these films, of the surface tension and other forces, such as gravity, have been very thoroughly investigated by the American mathematician, Willard Gibbs, and this ring of contact is therefore appropriately termed the Gibbs ring or layer. It is seen that the films remain all black, i.e. of uniform thickness, while the various Gibb layers are variable according to the excess of liquid on the neighbouring glass surface. The amount of liquid they contain will vary from two or three times that present in the film up to a considerable amount. This relative amount can also be varied by small temperature variations imposed on the Gibb layer, which can cause a stream of liquid to pass out therefrom into the film which thereby becomes thickened. A similar result is obtained by differences of vapour pressure on the two sides of a film caused by small alterations of temperature, condensation on the film then taking place.

A special form of exhausted vessel constructed for the purpose of keeping the film stationary is shown in Fig. 6. It has two equal bulbs A and B with a slight, but rather sharp constriction between them, forming a neck on which the film is thrown. The two bulbs are drawn out on opposite sides of the neck to a tubular bend to form an enclosed space freely open to each side of the film, and the equality of pressure thus secured prevents the film from being disturbed by variations of vapour tension with temperature. A small bulbed reservoir is sealed in the centre of the tubular bend on the side farthest from the double bulb. The diameters of the films so obtained were varied in the different vessels used from 1 to 8 cm. After filling with the requisite amount of liquid, the whole can be exhausted through the tubular extension of the reservoir. In order to obtain a film the liquid is tilted over into one of the double bulbs and swirled with a single gentle sweeping motion round and over the neck, thus leaving a film across the opening. With a suitably shaped neck, provided there is a sufficient amount of liquid, very little practice is necessary. It may be added that films in the double bulb of Fig. 6 remain indefinitely. Even without any neck, a film can be obtained by practice in a plain cylindrical vessel of considerable diameter. One such film 19 cm. in diam. in a closed bottle is on the table, having become perfectly black since it was thrown more than a week ago.

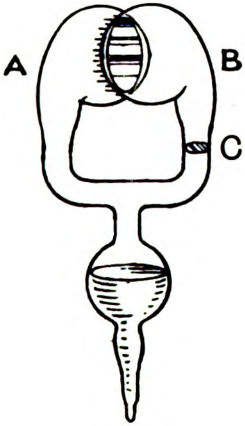


Fig. 6.

The double bulbs have proved to be very convenient for systematic study of plane films, from the ease with which they can be manipulated. In addition to the film at the neck, other films across the tube as at C can be obtained. Such a film acts as a stopcock, isolating the opposite sides of the main film on the neck. This can then be subjected to slight differences of pressure by gentle warmth from the hand. Instead of a plane film varying degrees of curvature can be obtained. Many effects on the character and appearance of the films can thus be studied, and have proved interesting.

The illumination, either for direct visual study or projection, is easily arranged without much disturbance from the glass surfaces. A slightly condensed beam of light from an arc lamp, passing through a plane-sided water cell to prevent heating, is directed at a moderate inclination on to the film, and the reflected light is focused on the screen by means of a suitable lens. For visual examination more diffuse lighting is preferable, and this can be obtained by placing a ground glass in the path of the incident beam at a little distance from the film, or for large films a plain white screen adjacent to the film should be suitably illuminated either by diffuse daylight, or beam from a lamp. In either case a black background must be provided to the film, to absorb the transmitted light.

With a smoothly blown bulb, very little distortion is produced, so that a clear elliptical image is seen. The appearance of the film at two stages of development is shown in the accompanying plate (Plate I.) A portable form is shown in Fig. 7. The bulb is mounted in a black box provided with a window opposite and parallel to the film. The window is central with the film of the same width, but about twice the height. The lower half of the window is ground glass, illuminated by an ordinary electric pocket lamp fixed in a small box. The film is viewed through the upper clear part of the window.

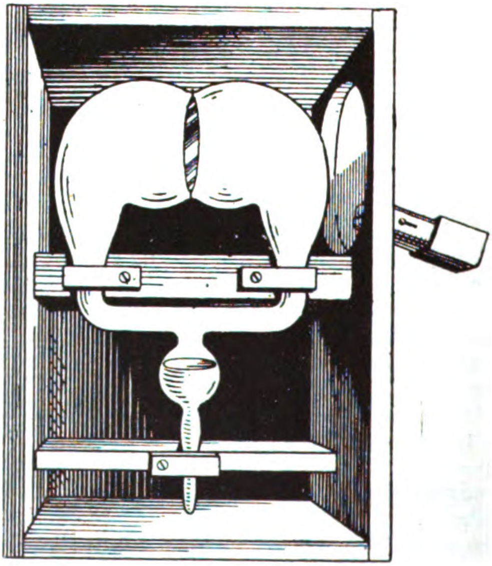


Fig. 7.

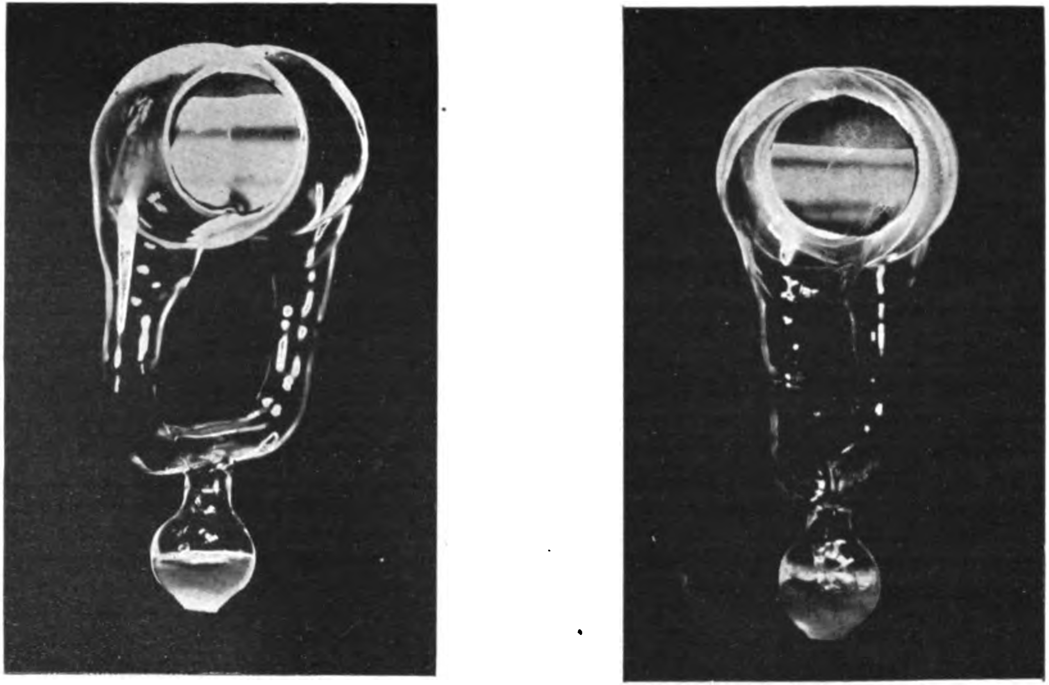


PLATE I.

The main features of the colour grading to be seen in such a film, apart from the black stage which is obtained later, are two purple to blue bands, more or less wide and separated according to the amount of thinning which has occurred. A less distinct third band is a redder shade of purple and is not so strikingly contrasted. This banding is well adapted for showing the mobility of the film, as a freely moving disc of liquid. When rotated in its own plane, the thickest and heaviest part will of course remain at the bottom, while the remainder of the film will be relatively placed according to its decreasing mass, and the surface tension will retard any relative motion of the parts. The colour bands, therefore, will keep horizontal when the film bulb is rocked in the plane of the film. To show the movement, some uniformly spaced black and white bands are marked on a card surrounding the film and in the same plane (see Fig. 8). The card is fixed to the film bulb and moves with it when rocked, while the bands are seen to remain practically stationary. With a suitable scale replacing the banded card, the amount of displacement from the horizontal can be read. The film can thus be used as a level, and from its permanence and easy production finds a ready application to such a purpose. A half black film in which the lower half is too thick to show colour is a better index than the varying colour bands, and gives instead a bright contrast in a very mobile condition. This is readily obtained from the totally black film, usually found in a bulb which has been left for a short time. The liquid for providing the thicker uncoloured part comes from the excess present in the Gibb ring. A quick rocking motion or oscillation in the plane of the film soon causes enough of the excess liquid to be transferred to the film, and instead of becoming uniformly distributed, it steadily accumulates upwards over the black film to any desired amount. Finally, of

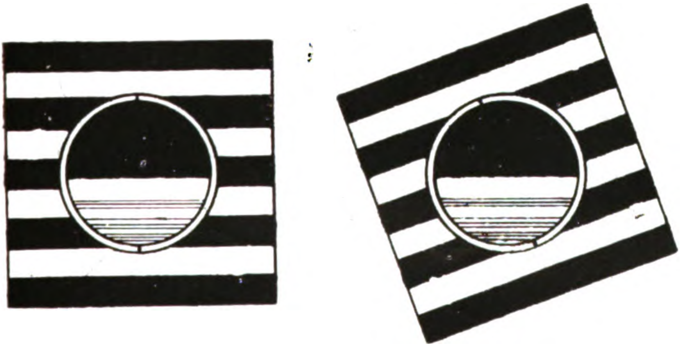


FIG. 8.

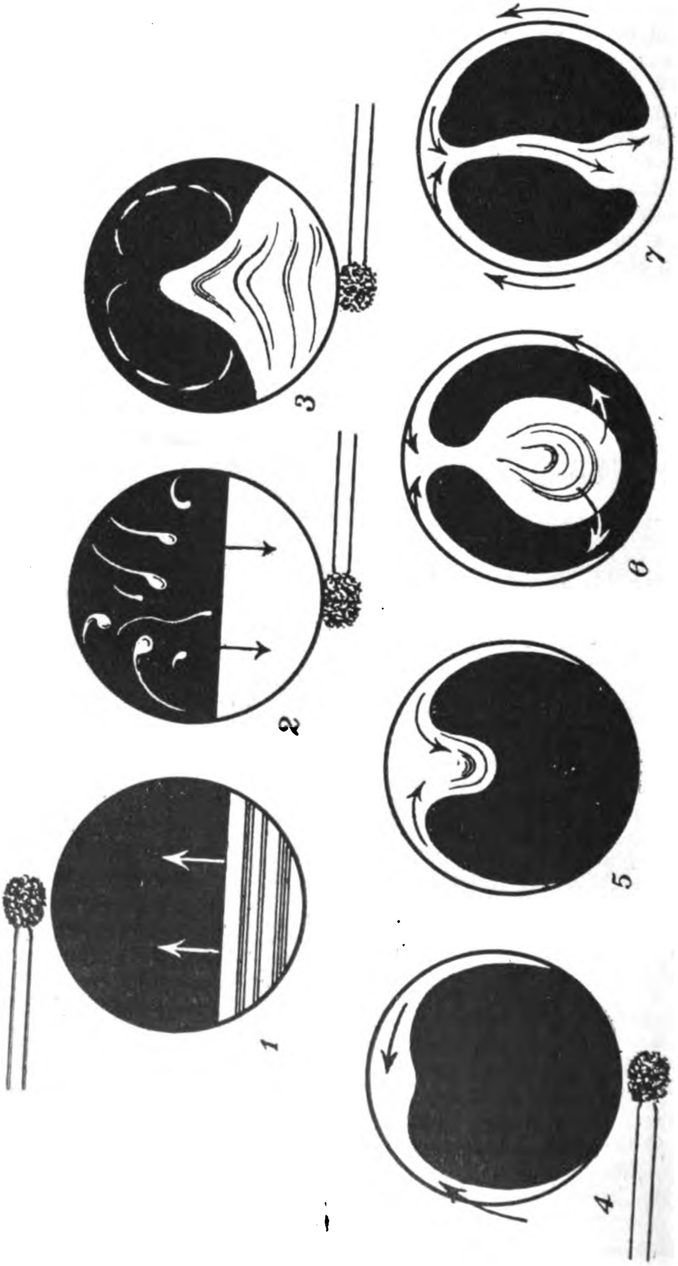


Fig. 9.

course, the whole film can be obtained in a condition too thick to show colouring. Such a film, however, when once more left quiet, thins out again at a much greater rate than a newly thrown film, which of course has an excess of undrained liquid on the glass surfaces in the neighbourhood of the Gibb layer. In fact it has been found that a film some 3 or 4 cm. diam. thins with extraordinary slowness if formed, e.g., on a cylindrical ring of fine gauze wrapped outside with bibulous paper or worsted wetted with the soap solution. In such a case only feeble colouring is seen even after some hours, showing how thick the film has remained.

To illustrate the stability of the film the portable box form was placed in an ordinary laboratory shaking machine oscillating at about three or four times a second, and the film remained intact for hours. If the movement was in the plane of the film, it became thickened as described above; but if in the perpendicular plane, very little alteration was produced in the distribution of thickness of the bands.

The results produced by difference of temperature in the Gibb ring are well seen in the double bulb. Here again an interchange of liquid between film and Gibb ring takes place. A black or partly black film shows the effects in the most striking manner. If a pad of wool soaked in water about 30 C. to 40 C. is held on the top of a black film (see 1, Fig. 9), in a few seconds a reflecting streak appears at the bottom, and steadily extends up to a broad segment which would cover the whole film surface in a short time. Now place the warm pad at the bottom of the half black film. At once there begins a movement in the surrounding Gibb ring: increasing streams moving up show coloured streaks mounting the arch of the film, breaking out as brilliantly coloured starlike discs, often leaving a streak curving after them through the black, like a stream of meteors across a dark sky. Meanwhile the coloured lower segment of the film thins out and passes through the appropriate stages of colour, while being steadily absorbed downwards, as if to feed the upward moving streams in the Gibb ring.

In an unexhausted bulb the convection currents caused by this treatment give rise to quite different appearances. For example, starting with a film about two-thirds black, while the lower third is banded with colour, and applying the warm pad below, a current of warm air then streams up across the film displacing the liquid against the gravitational and surface tensional forces which tend to maintain it in its initial condition. A coloured peak is steadily elevated into the dark upper region, and a thin stream of coloured particles begins to rise from the part so elevated, and fall again round the periphery on to the disturbed coloured part below. The original parallel bands have now the appearance of strata bent into finely curved patterns; a very brilliant appearance occurring from the contrast of the silvery and steel-blue portions of the film, the effect being very much like that of some Japanese lacquer, the simulation of a volcano enhancing this suggestion (see 3, Fig. 9).

In an horizontal film many similar movements may be produced; the various stages observed in one case are figured at 4 to 7, Fig. 9, the convection finally established passing in a coloured stream across the black film, and back round either side through the Gibb ring.

Variation in the amount of liquid near the Gibb ring has been referred to as being a disadvantage for certain purposes. With the object of avoiding this a ring support was arranged for the film. Platinum wire about 0·2 mm. thick was used, and was sealed in at the neck of the double bulb. The film was thrown as usual; the peripheral part between the wire ring and the glass was then destroyed either by touching with a glowing pencil of carbon, or more conveniently with a small sponge saturated with liquid air. With such an arrangement there is no actual necessity for an open way on both sides of the film to equalise the pressure, because the film hangs free of the glass. A simpler form of double bulb can then be used, with the result that there is greater freedom in illuminating and viewing the film. This was found to be especially desirable with the use of ultra-violet light, for which purpose a quartz bulb was obtained of the shape shown in Fig. 10. For some purposes the convex front of the bulb may be replaced by a plane window set at a small angle to the film in order to avoid subsidiary reflections. This form of support is well adapted for showing the permanence of the film under vibration, the wire ring being then sealed to the bulb neck at one point only, and thus behaves as a sensitive index to any vibration communicated to the bulb. A spot of light reflected from the film shows the movement. It can be seen that the film is not destroyed even though strongly vibrating when the ring is projected in profile;

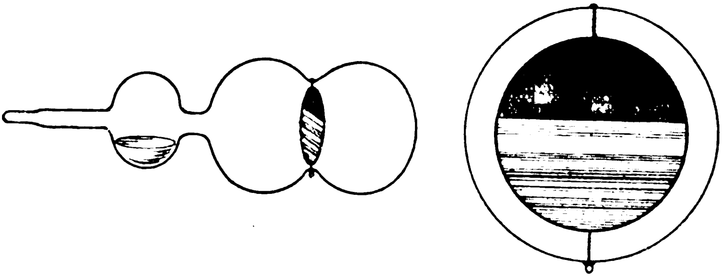


Fig. 10.

the amount of the amplitude of the vibrations is seen to exceed 2 cm., in a ring of 3 cm. diameter.

In another form of exhausted vessel, shown in Fig. 11, the film is obtained on a rectangular glass frame. This frame was made of glass rod 0·2 mm. diam., and must be symmetrically placed in the tube. To obtain the film the liquid has simply to be smoothly decanted out of the bulb to the opposite end and carefully returned, taking care that the frame emerges at right angles to the surface of the retreating liquid. The shape of the bulb permits the film to be set up for examination with either its long or short side vertical, while retaining the liquid in the bulb.

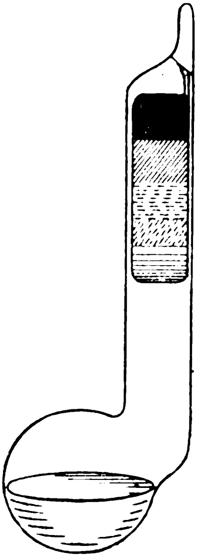


Fig. 11.

For the measurement of the rate of extension of the black the wire ring or some similar supporting frame is very convenient, as the film is thus completely contained and isolated from excess liquid. The line of demarcation between the black and the remainder of the film is very clearly marked, and with the proper illumination the fall can easily be followed on a cathetometer. Different soap solutions give different results, and temperature effects are very marked. The next table includes some typical results obtained at ordinary temperature with solutions of potassium and sodium oleates approximating in composition to that given by Plateau.

RATE OF EXTENSION OF BLACK ACROSS FILMS OF SOAP SOLUTION.

	mm per minute.
Exhausted Vessels.	
Film 3 cm. diameter, in vertical plane.	
On glass neck	0·20 to 0·05
On wire ring	0·33 to 0·16
On glass frame, 6 cm. × 1 cm.	
Long side vertical	4·0 to 0·4
Short side vertical	0·2 to 0·016
Film 3 cm. diameter, in horizontal plane	0·09
Unexhausted Vessels.	
Films in vertical plane, across glass bottle.	
19 cm. diameter	0·24 to 0·16
3 ,, ,,	0·06 to 0·02
Film in horizontal plane, across glass bottle.	
3 cm. diameter	0·12 to 0·16
Alteration of Inclination of Plane of Film.	
Film on rectangular frame in exhausted vessel.	
Inclined 10° to horizontal	0·04 to 0·01 after 7 hours.
,, 20° ,,	0·15
,, 40° ,,	0·305

In an horizontal film the black circle increased in diameter from 8 to 32 mm. in about 4 hours, the actual rate being 0·11 mm. per minute. It follows that the gravitational effect is considerable.

A 3 cm. film of 2 per cent potass oleate in 50 per cent glycerin, after being freshly thrown, gave a rate of 0·05 mm. in a vertical plane. After the same film had become completely black and drained, it was re-thickened from the remaining Gibb ring by rotation. The rate then measured was 0·2 mm. through the upper third, 0·5 mm. in the equatorial part, and 0·3 in the lower one-third. This was again repeated, when the corresponding figures were 1·6, 5·0 and 1·6. It would thus appear that when the Gibb ring is forcibly deprived of liquid it exerts a very powerful attraction to any liquid in the neighbouring film, quickly draining it to the black condition when the neighbouring surfaces are well drained.

Generally speaking, solutions of potassium soaps thin rather more slowly than sodium soaps of the same composition, and both are much slower than comparable concentrations of ammonium soaps. These last are very sensitive to small variations of ammonia from neutrality, becoming slow when the ammonia is in defect, and vice versa. As regards the percentage of glycerin, as would be expected, increasing concentration reduces the mobility and rate of development of the black, which rate, however, is then more sensitive to changes of temperature than are those solutions having less glycerin. A plane vertical circular film 19 cm. diameter, containing 5 to 10 per cent excess of ammonia over neutral oleate in 50 per cent glycerin, gave

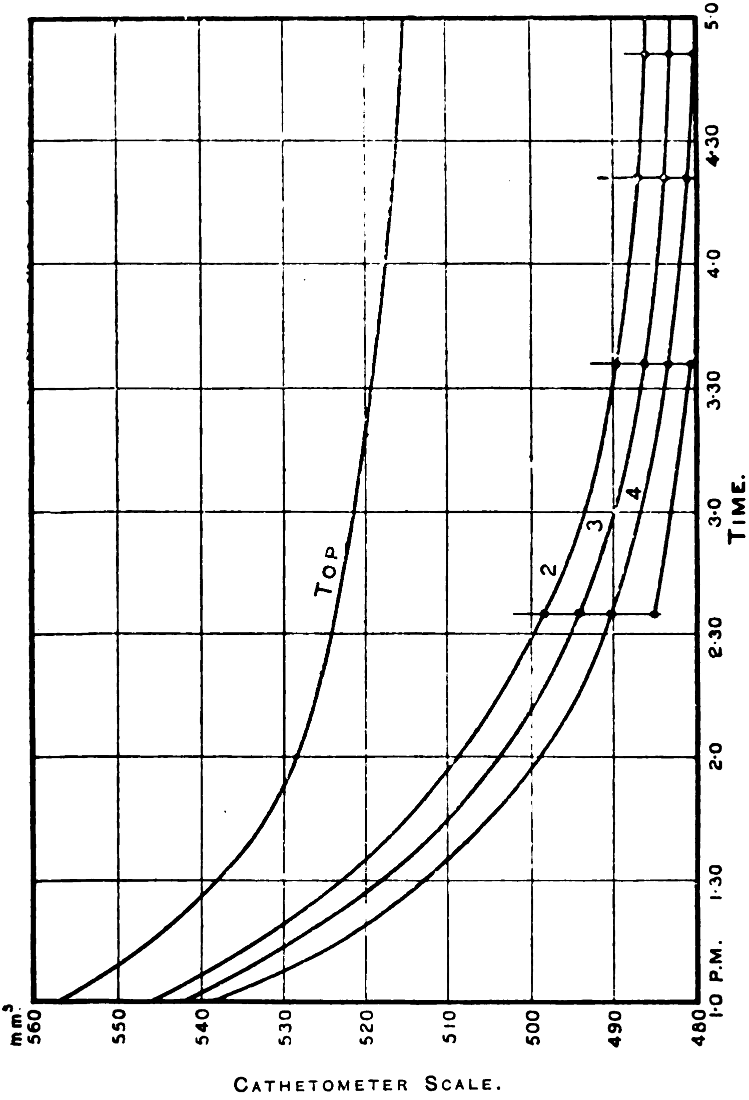

mm³
560
550
540
530
520
510
500
490
480
1·0 P.M.
1·30
2·0
2·30
3·0
3·30
4·0
4·30
5·0
TIME.
TOP
2
3
4
CATHETOMETER SCALE.

Fig. 12.

a rate of 1½ cm. per hour for vertical fall of black boundary. The black does not generally extend at a steady rate across a circular film. Thus a film on a thin platinum wire ring 3 cm. in diameter, using 1¼ per cent potass oleate in 25 per cent glycerin, gave in successive half-hours rates of 0·3, 0·23, 0·18 mm. per minute. This slowing down is to be expected as the black approaches the lowest part, where the ordinary drainage from the film accumulates to form a permanent drop. It follows that a more uniform rate is obtained in a large film than in a small one. Thus in a film on a rectangular frame 8 cm. long vertically by 2 cm. across, a uniform rate of 0·45 mm. per minute was registered through 5 cm.

This last measure was obtained by a solution of 3⅓ per cent potass oleate in 33 per cent glycerin in the evacuated tube of the form shown in Fig. 11.

In this apparatus the film can be produced under such undisturbed conditions that several grades of black frequently appear between the ordinary deep velvety black and the silvery film which usually directly follows it in order of thickness. As many as five distinct grades have been seen, each perfectly defined in regular proportion by a sharp horizontal boundary line. In the development of the film these extend downwards at different rates, as shown in the accompanying graphs (Figs. 12, 13, 14.)

The values of the observed rates in mm. per min. for a 5 per cent ammonium oleate solution in 50 per cent glycerin, in successive periods of 20 min., were:—

						Finally	
Top black	0·6	0·5	0·3	0·18	...	0·06	mm. per min.
2nd ,,	0·8	0·55	0·45	0·35	...	0·05	,,
3rd ,,	0·9	0·60	0·43	0·34	...	0·05	,,
4th ,,	0·95	0·65	0·425	0·33	...	0·05	,,

Comparable observations with a 3⅓ per cent potass oleate in 33 per cent glycerin gave the following values. Here five grades were detected:—

Top.	Uniform rate of 0·45 mm. per min.			
2nd	0·55	0·375	0·26	,,
3rd	0·55	0·38	0·40	,,
4th	0·50	0·375	0·26	,,
5th	0·40	0·28	0·20	,,

Preceding these observations, the 4th and 5th grades had not very definitely separated. The values observed during this initial period for the 2nd and 3rd grades were 1·3 mm. and 1·0 mm. in succeeding 10 min.; the two separating lines being only a few mm. apart and parallel. The form of the graph was thus:—

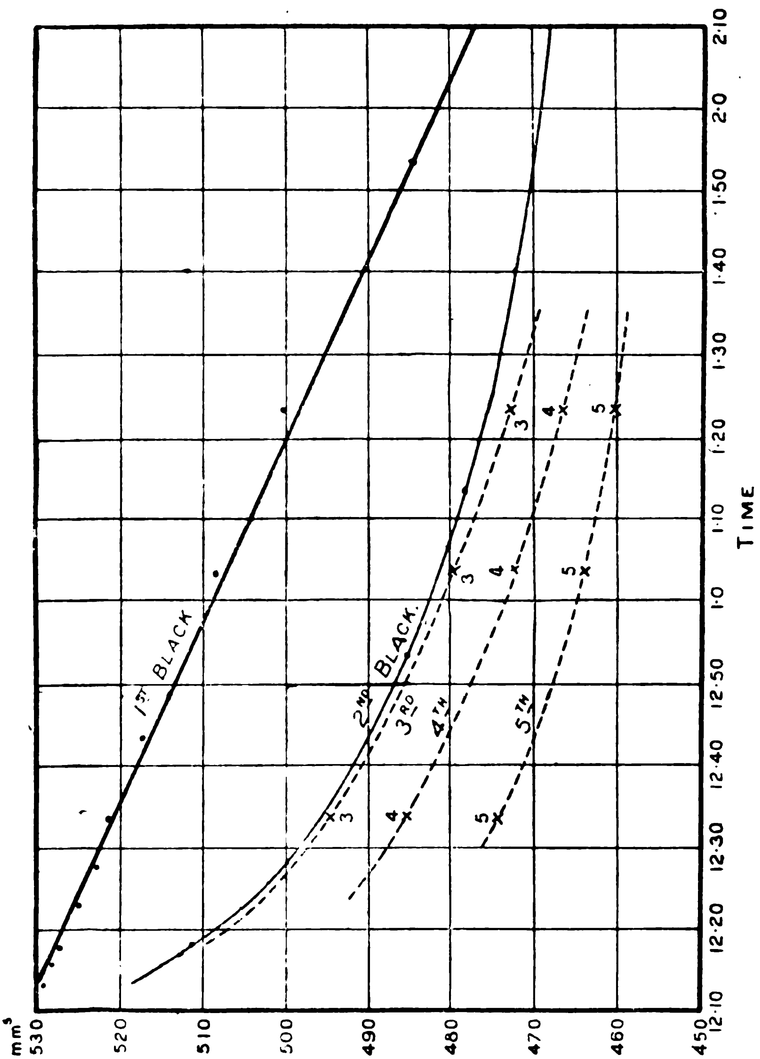


Fig. 13.

Instead of taking the measures with the film vertical, observations were made at an inclination of 40° to the horizontal, with the following result :—

Inclined 40°.	0·305 mm.	1st black
,,	0·65 ,,	2nd and 3rd blacks
,,	0·90 ,,	4th black

For a more extended study longer films must be used. These graded blacks appear to be rather unstable. At times they become grained and indefinite under slight stimulus of vibration or change of temperature. A very striking effect is produced by cautiously flowing a little solution through the lower part of the graded film; the various grades then become mottled, and coalesce with rapid aggregation to the stable silvery boundary. This effect sometimes occurs apparently spontaneously. With solutions of ammonium oleate, slight excess of tension of ammonia seems to be a predisposing cause, easily brought about by a small elevation of temperature.

The form of graph observed in such a case is next shown. The film started with development of three grades of black, which suddenly coalesced to the common single stage, which then extended at a fairly uniform rate.

Rate before coalescence	... 1st	0·40	mm. per min.
,, ,,	... 2nd	1·50	,,
,, ,,	... 3rd	1·45	,,
Rate after coalescence	steady at	0·8	,,

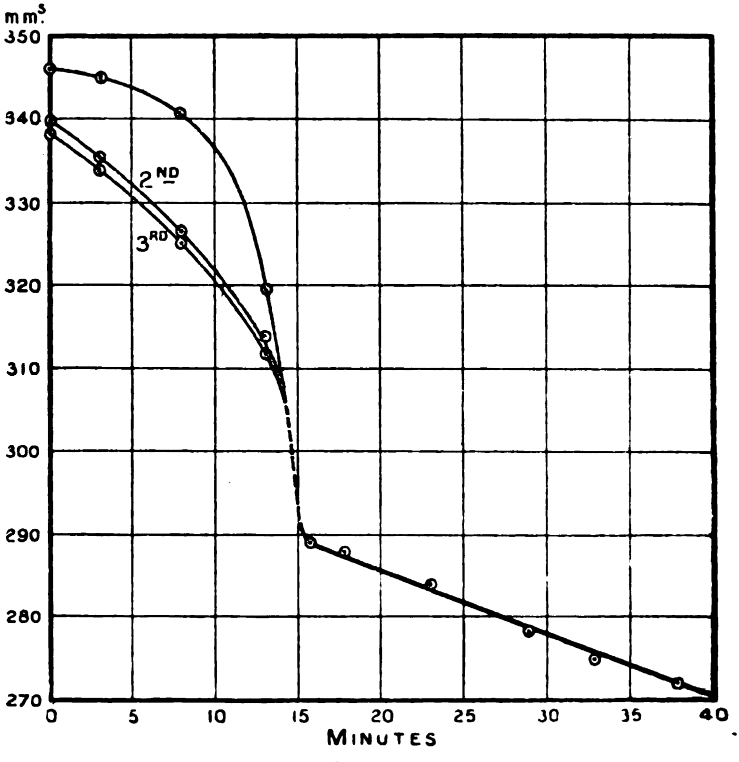


FIG. 14.

Colour Variations.

The colours shown by liquid films are the index of their thickness, becoming especially definite as the greatest tenuity is reached. The limit in this direction is the so-called black film, by which is meant a film too thin to cause colour by interference.

A liquid film has two reflecting surfaces, so that the light coming from the film is due to reflection from the upper and the lower surface. This latter portion will have travelled a slightly longer path than the first, and in a denser medium, where its velocity is lower. The vibrations will thus emerge out of step or beat with the part reflected at the upper surface, which has travelled only in air, and interference between the two portions must follow. This will depend in amount on the length of the path in the liquid, that is, on the thickness of the liquid film and the inclination of the path of the light to the reflecting surfaces. When the film becomes very thin, the length of this path in the liquid approaches the order of a few wave lengths of light, and the interference effect will then become appreciable. If the light be monochromatic, that is, composed of one wave length only, alternate light and dark bands will be seen varying in character with the specific monochromatic light. When composite light is used, as in the case of ordinary white light, various wave lengths will be successively suppressed as the thickness of the film decreases, and the reflected light being deprived of these will accordingly appear coloured.

The limiting case of the "black" film is obtained when the thickness of the film is very small compared to the wave length of ordinary light; no sensible interference can then be caused in the film because of the extreme shortness of the path therein. However, all the rays that have been reflected *in* the liquid—i.e., from the lower surface—suffer no modification; those which are reflected *on* the liquid are retarded or held back half a beat, and will thus be completely out of step with those coming out from the lower surface, with the result that an apparent blackness results. Such feeble light as there is appears neutral and faint on a dark ground, and represents, so to speak, no more than the difference in general intensity suffered by the internally reflected light, due to the slight absorption in the film.

In the same way the light passing through the film becomes coloured. The two portions now producing colour by interference may be regarded, on the one hand, as that which has simply been bent or refracted slightly in passing through the film; and, on the other hand, as that which instead of passing straight through is reflected up from the lower surface and down again from the upper surface before following the course of the unreflected beam. The amount of retardation in the film is the same as was the case between the two reflected portions of light previously considered. But none of the transmitted light is reflected on the liquid, and therefore it does not suffer the phase change of half a beat already referred to. The resulting colours will therefore be complementary to those produced at the same time in the reflected light, because the amount of interference will be uniformly different by this half beat, the vibrations suppressed in the reflected illumination being present in that transmitted, and vice versa. The limiting case of the very thin film appears bright by the transmitted light, since no appreciable retardation occurs in the passage through the film ; and the phase change of a half beat giving the black film by suppression of the reflected light does not happen in the transmitted light.

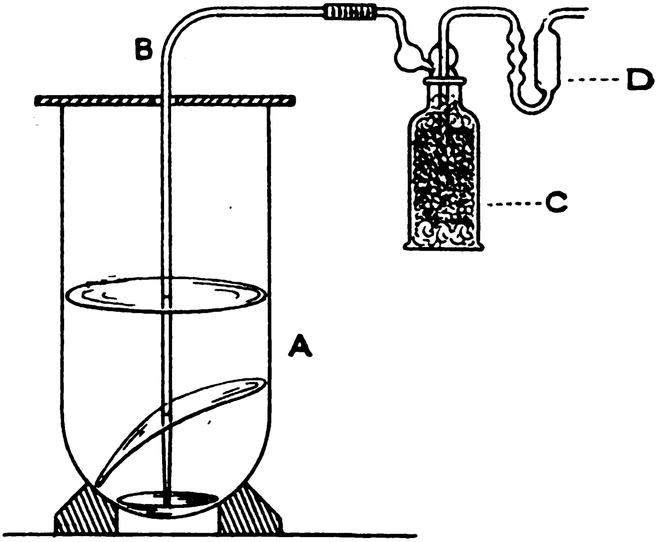


Fig. 15.

A simple method of obtaining a soap film of about 25 cm. diam. is illustrated in Fig. 15. An inverted cylindrical glass shade A containing slight depths of soap solution has its inside surface thoroughly moistened by careful tilting and rotating. A glass cover pierced with a hole large enough for a convenient length of quill tube B to pass was bent at right angles and connected to the outlet of a gas washing bottle C charged with granular soda lime to purify the air from carbonic gas and cotton wool to remove all suspended matter. A bubbler U-tube D attached to the inlet of C serves as a convenient mouthpiece and condenses the major part of the water vapour from the breath, but when large volumes of air have to be used it must come from a gas holder or water pump or compressed air cylinder. After the impurities of the vessel have been removed, dipping B into the solution in A, a small bubble can be blown on the liquid, and this can be expanded until it passes up the wet sides of the cylinder as a flat circular film. The film becomes thin enough to show brilliant rings of colour in less than a minute, these being made brightly visible by holding an illuminated white sheet of cardboard behind and just above the film, while also arranging a black background below the white card. The plane of the film is at right angles to the cylindrical sides, and will thus be level when the cylinder is vertical. In this condition the centre of the film becomes thin enough in a few minutes to show a black circle which steadily extends in diameter until, after some hours, the whole film is black. Such black films under these circumstances may be kept for a year.

Around the black circle as first seen there is a ring of silvery white very sharply marked off from the black. The silvery region then grades very smoothly into pale and deep golden, becoming orange and deep amber, then a brownish madder and full purple, clearing into a bright steel blue. This grades into greenish blue and fades into a light gamboge, developing into a more greenish yellow, and then to a brilliant green. After becoming again yellow, a crimson tint develops and intensifies to a full purple, followed by an extremely intense grass green. This ends the most striking part of the colouring, the increasing thickness being then denoted by alternate red purple or pink and green bands of diminishing depth of colour until they fade into the transparent colourless thick film.

These flat films have a position of equilibrium relative to the supporting vessel, tending to set at right angles to the plane of the walls at the place of contact; thus the film in the cylindrical glass shade remains flat, because that is the plane perpendicular to the supporting walls. If a film be similarly blown across the centre of a spherical flask, then at any inclination this condition will be fulfilled, and a uniform film should be able to remain in any position if it has the full diameter of the sphere. When there is the minimum of liquid and the film is well drained, and consequently nearly uniform in thickness, it is seen to remain practically quiet in all positions. When, however, a few c.c. of soap solution are left in the flask, the film is much thicker in the lower part, and, if, further, it is well moistened where it comes in contact with the flask, is more mobile and tends to assume a vertical plane, because the thicker part will sink to the lowest position, whence it will quickly revert when tilted. The appearance when projected on the screen shows that, however the flask be rotated, the film remains very nearly vertical. It will, however, remain fairly steady in any position if it is slightly less than the full diameter of the spherical flask, and is blown up clear of the excess of liquid. When this modification is employed a striking demonstration of the colour effects can be made by using the film as a mirror for an intense parallel beam of light condensed from an arc lamp. The reflected light is focused by a simple lens combination, and after reflection at a plane mirror can be seen in full detail on the screen. For this purpose a film is blown across the centre of a flask about 5 in. in diameter. The Epidiascope affords a convenient means for the illumination and projection. The film is kept inclined at about 10° or 15° to the horizontal; in a short time the black shows as a segment of the circular film, steadily increasing in extent from the top. The sharply defined line separating this black segment from the rest of the film makes it appear as if cut off. The continual movements in the film can be clearly seen. These are extremely complex, and consist largely of streams of liquids of varying thickness. Therefore they appear in a beautiful variegation of colours well contrasted from the general tone of the region of the film across which they move.

Every local disturbance of the delicate equilibrium is shown by coloured streams or wandering coloured areas of various sizes, usually tending to a circular shape. These, however, by collision or mutual attraction, while moving at different speeds in neighbouring paths, are frequently drawn out into threads and chains which cross the path of another moving circle, and are thus, or otherwise, broken, when a separation takes place into a line of small particles in the manner of the beads on a spider-web. These motions are most strikingly seen when they cross the black area of a film; a condition which is readily produced by small local differences of temperature in the Gibbs ring. Then a stream of coloured starlike discs breaks out from the Gibbs layer, and pass like rockets through the black region, giving a brilliant variety of effects as their many-coloured trails cut and coalesce.

Mr. W. J. Green, B.Sc., of the Davy Faraday Laboratory, has rendered valuable aid in the Research, and Mr. J. W. Heath, F.C.S., of the Royal Institution, has also assisted in the experiments.

Records of Experiments omitted from the Lecture on "Problems of Hydrogen and the Rare Gases."

Friday, January 22, 1915.

The behaviour of hydrogen and the Rare Gases in their passage through metals and other permeable materials have stimulated further investigation, since Graham published his classical researches in 1866. Some of the problems of gaseous transmission at high temperature through quartz, platinum and various metals were discussed in the Discourse of last year.*

Transmission through Indiarubber.

In the method adopted by Graham "a thin film of rubber from a small balloon" was stretched over "a thin plate of stucco" closing the upper end of a glass tube 1 metre in length, filled and inverted in mercury to give a Torricellian vacuum. Over this tube he fixed a "hood of vulcanised rubber, provided with a small entrance and exit tube" whereby different gases were introduced over the rubber, and their rate of transference into the vacuous space was determined. The values of the relative rates determined by Graham were as follows:—

Air	1·0	Oxygen	2·23
Nitrogen	0·87	Hydrogen	4·79
Carbonic Oxide ...	0·98	Carbonic Acid ...	11·83

The absolute rate of air at three different temperatures, through a thin coat of indiarubber on silk, were also given. Expressed in c.c. per day through each square cm. of membrane surface, the values are:

Tempr. 0° C.	4	14	60
Rate	0·07	0·28	0·83

Wroblewski† in 1876 examined the diffusion of gases through absorptive substances. In the use of caoutchouc he found that a membrane 0·034 mm. thick was almost completely impervious to air. Carbonic acid diffused through at a rate proportional to its pressure on the membrane, and independently of the pressure of air on the other side when this was free from carbonic acid. The connection between this result and Henry's Law of Absorption was pointed out. Hydrogen he found took 3·6 times as long to diffuse as an equal volume of carbonic acid; and in a mixture of the two, each diffused independently at a rate proportional to its partial pressure.

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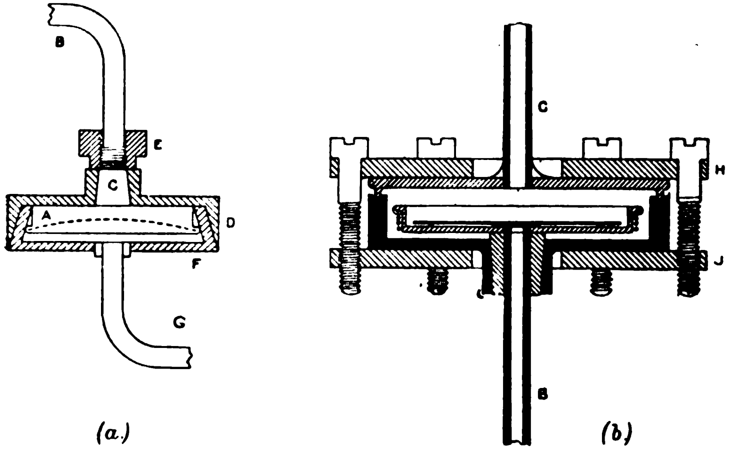
* Problems of Hydrogen and the Rare Gases. Proc. Roy. Inst., XXI., p. 543.

† Ann. Phys. Chem. 158, 1876, pp. 539-568; Rep. der. Physik. 12, 1876, pp. 423-453.

APPARATUS FOR STUDY OF RUBBER DIFFUSION.

For the purpose of extending Graham's investigation, membranes about 0·01 mm. thick were used for demonstrating some aspects of the diffusion of gases and vapours. The thickness after use was determined by immersing the membrane in liquid air, mounted on its brass-drum support, thereby changing it into a rigid body that could be cut off at the low temperature and subsequently weighed; knowing the specific gravity and the area the thickness is found.

Sheet Para rubber, about 1 mm. thick was gently warmed and carefully pulled in all directions by the hands, before being stretched on the brass support; the outside edge being thinly coated with melted rubber. It was found most convenient for one person to stretch and hold the membrane down in position while a second person carefully wrapped and tied on the securing thread. Much thinner films could be obtained from the material employed by dentists, giving three times the percolation shown by the stretched Para, but they were often faulty through small leaks, and had to be abandoned.



(a.) (b)

FIG. 16.

For maintaining different conditions of temperature and pressure, the mounted membranes were enclosed in boxes which were gas-tight. The two types employed are shown in Fig. 16—

(a) for pressures at or below one atmosphere;
(b) for pressures above one atmosphere.

In (a) the membrane is stretched across the open end of the shallow metal drum A. Just inside this a copper gauze disc is fixed to support the film against suction from within, communicated along the lead tube B, which connects by a ground joint to the highly exhausted manometer* or McLeod gauge. A charcoal liquid air vacuum arrangement serves for conveniently producing and renewing the exhaust in the manometer: but in the case of the McLeod gauge the molecular pump is more suitable. The tube B is soldered gas-tight into a coned and threaded boss C forming the centre of the closed end of A. The hollow brass cylinder D completely encloses A, on to the boss of which it fits tightly by its coned neck. A nut E secures the arrangement against subsequent differences of pressure. The rim of D is slightly coned inside to a similar hollow cylinder F, which thus completes a gas-tight box enclosing the membrane. A second lead tube G similar to B opens into F and leads to a two-way stop-cock, whereby the box is exhausted and filled to the required pressure with the dried and purified gas to be examined. The tube G is bent in a half circle and brought up parallel to B, so that the whole arrangement can be enclosed in a bath at any required temperature. For the use of higher pressures the two halves of the gas box seen in (*b*) fit together with a spigot joint filled with a thin lead washer, and are securely bound by peripheral screws on the external ring-plates H and J. Five or six discs of coarse dried filter paper are placed under the indiarubber membrane and form a readily permeable support. The 2 mm. opening of B is covered by very fine mesh copper gauze under the filter paper. The drum supporting the membrane is only 2 or 3 mm. deep. This arrangement was tested to forty atmospheres pressure. The pressure was regulated by a screw valve attached to the bottles containing the compressed gases. The gas passing through the membrane under high pressure appeared as a slow stream of bubbles issuing from B, and could be collected and measured in the ordinary way. At low temperatures and pressures the gas transference was measured, in this case also, either by the increase of pressure shown on an exhausted U-tube manometer of known volume connected to B, or by a McLeod gauge when necessary.

The rates of diffusion measured at a pressure of one atmosphere with the apparatus of Fig. 1 gave the following values at 15° C.:—

	Relative Rates	Rate expressed in c.c. per day per sq. cm.
Air	1·0	2·0
Nitrogen	0·69	1·38
Carbonic Oxide	0·94	1·88
Helium	1·75	3·5
Argon	1·28	2·56
Oxygen	2·0	4·0
Hydrogen	5·6	11·2
Carbonic Acid	14·0	28·0

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* See Fig. 9: Problems of Hydrogen and the Rare Gases. Proc. Roy. Inst. XXI., p. 558.

The relative rates of diffusion given in the table between the different gases is not preserved at all temperatures. Thus at 0° C. carbonic acid has 16 times the rate of air, instead of 14 as given above for 15° C. In another case one film at 2° C. gave 1, 4 and 1·65 for the relative rates of air, hydrogen and helium. Further, the absolute rates, given in the second column, are mean values, the films differing slightly in thickness. Extreme variations obtained were 1·2 and 3·2.

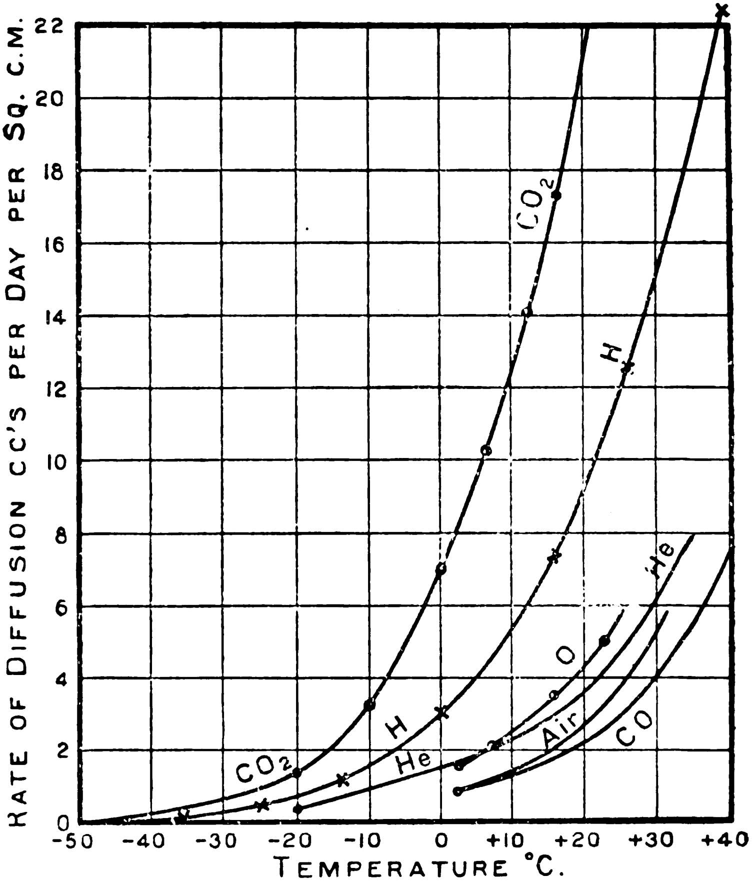

Fig. 17.

The order of diffusibility shown in the above table is difficult to associate with any chemical or physical property of the gases concerned. Especially is this the case with helium, the most volatile of all gases, having an atomic weight of 4, which is only one-eleventh of the weight of the carbonic acid molecule, whose rate of diffusion is nevertheless eight times that of the lighter helium. Also the relatively slow rate of helium compared to hydrogen is remarkable in view of the diffusion of these gases through quartz, in which case helium has much the greater rate.

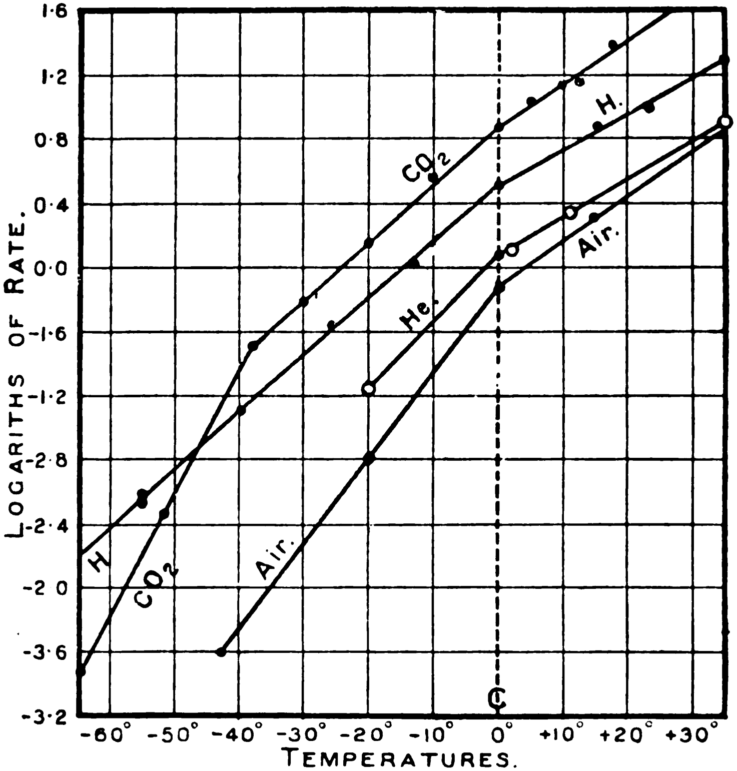

FIG. 18.

RELATION OF TEMPERATURE TO RATE OF GAS DIFFUSION THROUGH INDIARUBBER.

Fig. 17 gives the curves, showing the observed variation of the rate of gas transference with temperature in the case of six typical gases. In Fig. 18 the logarithms of the rates are plotted vertically, instead of the actual rates, with temperature horizontally as before; and in every case straight line graphs are thus obtained. These straight lines all show a marked deviation at 0° C., which naturally suggests that water in some condition is the associated cause. In the case of carbonic acid, another break was obtained at about −37° C. The gases used were dried through a suitably cooled U-tube before entering the box round the membrane shown in Fig. 1 (*a*); but there is the possibility that water in some form of colloidal association is a constituent of the complicated structure of the rubber membrane. The second break in the carbonic acid curve at −37° C. suggests a possible combination of the membrane with carbonic acid below this temperature.

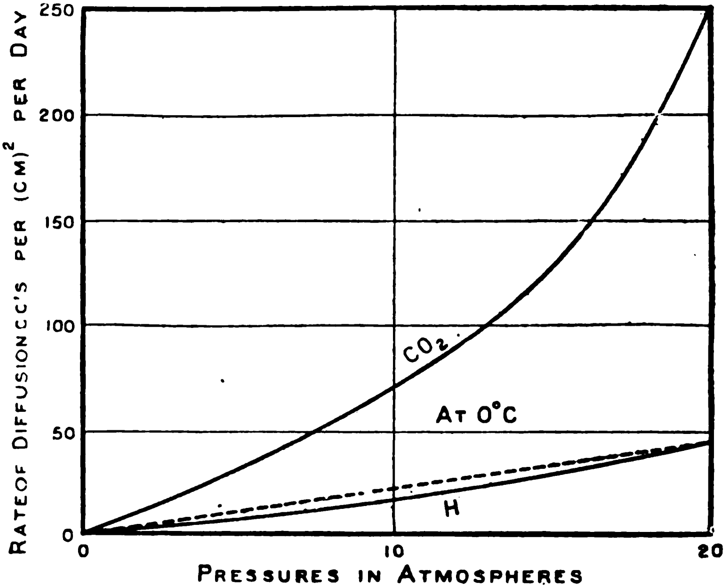

Fig. 19.

These results were obtained with gases at the atmospheri pressure passing into the vacuous space of known volume behind the membrane. Working at higher pressures—up to twenty atmospheres—this curious break in the logarithmic graph is much less evident. The conditions of experimenting were not quite comparable, as the gases passing through the membrane were collected at atmospheric pressure and measured either over brine or heavy mineral oil in the case of carbonic acid, or over water when hydrogen was employed.

When a gas-bottle charged to 10 atmospheres with carbonic acid was connected to the membrane box, Fig. 16 (*b*), the gas issued from the exit tube of the gas-box at the rate of two or three bubbles per minute.

The variation with increasing pressure at constant temperature is seen in Fig. 19.

It is definitely not quite linear in the case of hydrogen ; and with carbonic acid the rate increases at a very much greater proportion than the pressure. Comparable experiments made at fractions of an atmosphere reveal the same behaviour, though to a less degree. Wroblewski's conclusion would, therefore, seem not to be strictly true.

COMPOSITION OF PRODUCTS OF AIR DIFFUSION THROUGH RUBBER.

Since the different components of the atmosphere diffuse at different rates through rubber, it is of interest to examine the products obtained after an extended period of diffusion. The values found for the increase of oxygen percentage may be tabulated as follows :—

Time of Diffusion	Area of Membrane	Rate for Air at 15° C.	Volume of Product	Oxygen Percentage
1 day	50 sq. cm.	1·44 c.c. per sq. cm. per day	70 c.c.	46·5
4 days	50 ,,	1·44 ,, ,,	290 ,,	36
4 ,,	25 ,,	0·82 ,, ,,	82 ,,	31
5 ,,	25 ,,	1·08 ,, ,,	135 ,,	37

The presence of helium, as well as neon and hydrogen, could be readily detected in the gas passing through the membrane. For this purpose charcoal cooled in liquid air was used to maintain a low pressure behind the membrane by condensing the diffusing oxygen and nitrogen. On the side of the charcoal further from the membrane a sparking-tube of small volume was sealed, to afford the means of detecting any helium or neon and hydrogen : they would scarcely be diminished in amount by absorption in the small amount of charcoal used. The more readily to accumulate a sufficient pressure of these gases to give a good discharge, the free space in the apparatus was kept as low as possible. It was found necessary to dry the air coming into contact with the membrane, as otherwise the narrow space in the charcoal tube in liquid air became plugged with condensed ice from the water vapour diffused in from the air.

The form of apparatus found most convenient is here figured.

A is the charcoal tube about 7–8 mm. diameter and about 25 cm. long, for convenience of standing in a deep vacuum vessel in order to remain undisturbed in liquid air for extended periods. A is enlarged to a bulb below sufficient to contain a gram or two of good

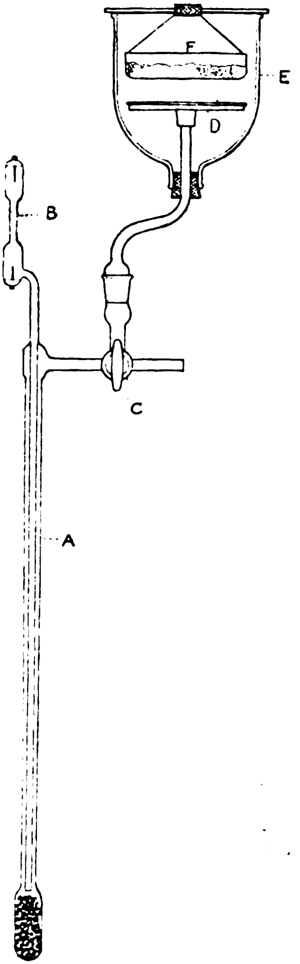

Fig. 20.

charcoal. Inside A is a smaller tube about 5 mm. in diameter reaching to the charcoal; it is sealed to the top of the outer tube and continues up to the sparking-tube B. The Regnault three-way stopcock C is sealed near the top of the annular space of A by a short connection of about 2 mm. bore. The vertical branch of C terminates in a ground cone as close to the stopcock as possible. The brass cone of an indiarubber membrane support D fits this vacuum tight. To limit the free space below the membrane the drum support is reduced to very little more than the depth of a few thicknesses of stiff dried porous paper, which are placed inside to support the membrane. The remaining branch of C serves for connecting the preliminary exhaust. The membrane support passes through a cork into an inverted bell jar E, from the loose cover of which a dish F of phosphorus pentoxide is hung. The air gaining access to the membrane through the loose cover is thus dried. To equally serve this purpose the membrane may be enclosed in its gas-box, Fig. 1 (*a*), with the inlet tube bent down into a U immersed in the liquid air round A.

It is only necessary, after heating and exhausting out the charcoal and sparking the Pt electrodes of B, to turn C to connect D to the charcoal tube, and then arrange a flask of liquid air to keep the charcoal continuously cooled. The annular construction of A, when in the liquid air, keeps B free from any volatile impurity coming from the membrane or the stopcock lubricant.

The air diffusion rate of the membrane used was previously determined in each case; thereby a knowledge of the amount of air diffusing through and condensed in the charcoal in any time is determined, the area of the membrane being also known.

For a fairly rapid indication, a membrane of 50 sq. cm. area, having an air rate at 15 C. of 1·44 c.c. per day per sq. cm., was employed with half a gram of charcoal. Within 12 hours the whole helium and neon spectrum was visible when a discharge was passed between the platinum wire poles of B. 36 c.c. of air would now be condensed in the charcoal. The F line of hydrogen was faintly seen, with the beginning of some nitrogen lines. This experiment continued for four days, when on removing the charcoal from the liquid air 290 c.c. of gas was obtained containing 36 per cent of oxygen.

To study the manner of growth of the gas discharge more steadily, another experiment was made with a thicker membrane of half the area, i.e. 25 sq. cm.; the air diffusion rate was only 0·82 c.c. per day per sq. cm. A contained 2 grams of charcoal. The resulting observations were as follows:—

18 hours: 15½ c.c. condensed: He yellow line alone, quite bright, with much phosphorescence, therefore low pressure. Nothing further showing until

42 hours: 37 c.c. diffused in; bright gas discharge. Helium: yellow brilliant, green feeble. Neon: orange set quite distinct. Hydrogen: very feeble F line only.
72 hours: 63 c.c. condensed. Same appearance as shown at 42 hours intensified, with F line of hydrogen quite plain.
90 hours: 78 c.c. condensed. Helium, neon, hydrogen and nitrogen all visible. Neon orange and yellow about equally intense with the complete set of 5 helium lines; F line subdued comparatively, so also blue, green and violet nitrogen. Full red gas glow discharge with still some phosphorescence.

The day after this the neon greatly intensified, while the helium faded out in the blue and green. The hydrogen then disappeared; then the nitrogen intensified, and the remaining lines greatly diminished.

When, instead of platinum wire poles, B has external electrodes, conveniently formed by small deposits of silver top and bottom, then the green line of helium is the first to show, and all the helium spectrum develops before anything else.

The sequence of the experiment was as follows:—

Large double film of 98 sq. cm. membrane surface. Rate of diffusion for air at 15° C., 1·15 c.c. per sq. cm. per day, or 4·7 c.c. per hour through the whole surface. Weight of charcoal 1½ grams.
4 hours: 19 cc. diffused in; faint shadow of the green helium line, with much phosphorescent discharge.
9 hours: 42 c.c. in. Helium green line well seen; no other.
11 ,, 52 ,, Same as in 9 hours, but more definite.
22½ ,, 106 ,, All the five principal helium lines only.
23½ ,, 111 ,, Neon orange set visible in addition to the helium lines previously showing. No sign of hydrogen.

The total gas extracted from the charcoal had 40 per cent of oxygen in this case.

RELATIVE RATES OF HELIUM AND NEON DIFFUSION.

The three gases helium, neon and hydrogen are not of course affected to the same extent by charcoal at liquid air temperature. Helium is hardly absorbed at all; neon is only very slightly taken up, whereas the condensation of hydrogen is quite appreciable. Therefore, in an equal mixture of the three gases over cooled charcoal, the helium and most of the neon would be free, while the pressure of the hydrogen would be sensibly reduced. Now, the relative proportions of the three gases present in air may be taken as 20 of neon, 5 of helium, and from ½ to 2½ of hydrogen (London air), expressed as parts per million volumes of air. If, therefore, they diffused through the indiarubber at the same rate, the neon should be apparent in one quarter the time of the helium, while the hydrogen would only appear in about twenty times the period necessary for the neon.

The second experiment just detailed showed that when the helium was visible spectroscopically in 18 hours, not until 42 hours could the neon be seen distinctly, and then the hydrogen is also just visible. One may infer that the ratios of helium and neon diffusion are as 9 to 1, and that hydrogen and helium are nearly alike. But considering the partial condensability of the hydrogen in the charcoal, the actual hydrogen rate would be greater, as indeed is shown by the direct measurements. In the same way, having regard to the slight condensability of the neon, the above estimate of one-ninth the helium rate would be a minimum. The spectroscopic method cannot, however, be regarded as strictly reliable under such circumstances, as the electric discharge does not necessarily induce the emission of spectral lines proportionally to the several constituents.

It must also be noted that when the discharge-tube had outside electrodes, the first helium line to be seen was the green; and that it showed in at least one-fifth of the time necessary to bring out the neon lines in these conditions, compared to about two-fifths of the period required by the tube provided with platinum wire poles, which gives the yellow helium line first and not the green. The method, nevertheless, is capable of useful extension.

PROPORTION OF "UNCONDENSABLE" GASES IN THE PRODUCT OF AIR DIFFUSION THROUGH RUBBER.

One experiment was made with the object of ascertaining the proportion of the uncondensable gases—helium, neon and hydrogen—in the total material diffusing through a rubber membrane in air. The method of condensation at 20° Abs. was employed as described in the Discourse a year ago.

From this it appeared that after two hours' diffusion through a membrane of 48 sq. cm. surface, with an air rate at 15° C. of 0·74 c.c. per minute, the amount of uncondensable present was 17·8 parts per million; but that this value was increased to 120 after 4 hours, or nearly 7 times as much. The value for atmospheric air being about 27 parts per million, there would thus appear to be a five-fold increase of the uncondensable material in this diffused gas.

The total pressure of diffused gas passing into the highly exhausted McLeod gauge of 560 c.c. volume was 9·11 mm.; so that the partial pressure left at 20° Abs. was 0·0011 mm. However, no precaution was taken to dry the air coming into contact with the membrane, as described in the previous experiments. It is therefore possible that the metal of the membrane support was attacked to same extent by the water vapour, thereby giving an increased proportion of hydrogen, so that the present result requires confirmation.

DIFFUSION THROUGH RUBBER MEMBRANES IN LIQUIDS. WATER VAPOUR TRANSMISSION.

The diffusion of water vapour has already been alluded to in connection with the necessity of drying the air over the membranes when studying the composition of the gases diffusing for an extended period. The first rough measures on the oil manometer of the rate of water-vapour transmission, by comparing wet and dry air, seemed to indicate that at ordinary temperatures water vapour had at least forty times the rate of dry air reckoned at the same pressure, but that at high temperatures the proportion was much less. More exact determinations were necessary to settle this, and the method adopted was to weigh the diffusing water vapour after condensation in a cooled U-tube in the exhausted space behind the membrane, the cooling agent employed being liquid air or solid carbonic acid.

A preliminary trial on these lines showed that when air from the room was diffusing through a 25 sq. cm. membrane and the cooled U-tube, into the exhausted McLeod gauge, then a visible ice deposit was obtained in the U-tube in less than 10 minutes, and that nearly 30 mgms. of water were obtained in a day.

A mounted membrane of 12½ sq. cm. area was then immersed in water at 15° C. In 18 hours 36·8 mgms. of water were obtained, which is a rate of 3·93 mgms. per day through each square centimetre. To compare with the values given above for air this may be transferred into volume of water vapour reckoned at N.T.P., when the rate becomes 4·88 c.c. per day per sq. cm.

Neglecting any effect of the water immersion on the character of the membrane, and assuming that the diffusing water vapour was merely that due to the tension of water at 15° C., then we have only 12 mm. pressure instead of 760 mm. for the air diffusion. Therefore the proportional value for water vapour at an equal pressure would be 94 times that given by air, reckoned weight for weight, or 163 times in relative volumes, the figures then becoming 249 mgms., or 310 c.c. of N.T.P. water vapour.

The effect of dissolving a salt in the water round the membrane was next tried. Taking a 40 per cent solution of calcium chloride at 15° C. as an example, the rate of transmission of water vapour was reduced to 2·45 mgms. per sq. cm. per day, equal to 3·04 c.c. of N.T.P. vapour, being about 70 per cent of the rate of pure water. It may be added that the tension of water vapour from such a solution at 15° C. was found to be one-half that of water.

In contrast to the salt a colloidal substance was tried, as having no effect on the vapour pressure. Gum acacia was used as an example of this; a distinctly viscous solution of 1·07 S.G. gave the same rate very nearly as pure water on the same membrane. The actual rates per sq. cm. per day were 4·89 mgms. for pure water and 4·76 mgms. for the gum, or not more than 2½ per cent reduction.

A determination with the membrane in pure alcohol gave 56 mgms. condensed from 4½ hours' diffusion, which is a rate of 24 mgms. per sq. cm. per day, equivalent to 11·6 c.c. of alcohol vapour reckoned at N.T.P.; this is about the same rate as hydrogen diffusing at atmospheric pressure. The tension of the alcohol being about 32 mm., the proportional rate at 760 mm. would be 570 mgms., or 275 c.c. of N.T.P. vapour, per sq. cm. per day. This is some 145 times the volume rate found for air at 15° C. on the same membrane.

Transmission through Rubber of Gases Dissolved in the Immersing Liquids.

The rate of diffusion through the indiarubber of gases dissolved in various liquids was investigated along with the above measurements of liquid vapour transmission. Various gases were dissolved in the water, alcohol, etc., in which the membrane was immersed. The increase of gas pressure was measured in the exhausted McLeod gauge to which the membrane and condensing U-tube were connected, and thereby the amount of diffusion of the dissolved gases through the membrane is revealed. Instead of a gas rate of the order of 2 c.c. per day per sq. cm. obtained with the membrane exposed in air, the following values were obtained :—

I.—Water and Aqueous Solutions round Membrane.

	c.c. per day per sq. cm.
Freshly boiled out, distilled water	0·09 to 0·04
Air saturated distilled water	0·414
Hydrogen saturated distilled water	1·62
Carbonic acid saturated distilled water	15·5
Cane sugar solution, 30 per cent. Freshly boiled ...	0·017
,, ,, 36 ,, ,,	0·011 to 0·006
,, ,, 36 ,, Exposed to air 2 days	0·09
Gum acacia solution, S.G. 1·07. Freshly boiled ...	0·010
Calcium chloride, 60 per cent solution. Freshly boiled	0·007
Hydrochloric acid solution, 3 per cent — Not boiled; metal protected by rubber	0·19, falling to 0·06
Acetic acid solution, 3 ,, — Not boiled; metal protected by rubber ...	0·90
Formic ,, 3 ,, ...	0·45

II.—Alcohol.

Freshly boiled under exhaust	0·4 to 0·6
Air saturated	1·26
Hydrogen saturated	3·5
Carbonic acid saturated	25·0
1 per cent alcohol in water, boiled	0·05
5 ,, ,, ,, ,, ...	0·11
10 ,, ,, ,, ,, ...	0·27

III.—Glycerin.

Immersed in 96 per cent glycerin	0·0084
,, 50 ,, ,, (freshly boiled water used)	0·027
,, 50 ,, ,, air saturated	0·09

It would thus appear that the relative rates of gases in solution is not so low as their proportional lowering of volume concentration in the liquid, for water at 15° C. dissolves about one-sixtieth of its volume of air or hydrogen, but the rate of diffusion from air-saturated water is only reduced to one-quarter of the rate in air. Roughly the same relation is shown by hydrogen. The effect of expelling the gases by boiling is very marked, especially in the case of solutions. The effect of glycerin in checking diffusion is also very evident.

The behaviour of alcohol is quite the opposite. Air goes through the membrane equally rapidly whether alcohol is round it or not. By freeing the alcohol from dissolved gases, the diffusion is checked, as is the case with water; but in about one hour has regained its initial rate.

[J. D.]

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A LECTURE,

ON

THE PLAN

WHICH IT IS PROPOSED TO ADOPT FOR

IMPROVING

THE ROYAL INSTITUTION,

AND

RENDERING IT PERMANENT.

Delivered in the Theatre of the Royal Institution, March 3d, 1810.

BY H. DAVY, ESQ. SEC. R. S.
PROF. CHEM. R. I.

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Historic Address delivered to the Members by Humphry Davy in the year 1810. *Reprint for permanent reference ordered by the Managers to be inserted in the "Proceedings" for the year* 1916.

LECTURE.

I HAVE been honoured, by the request of the Managers, to lay before you some General Views of the Plans that are in contemplation, for the Support and Improvement of the Royal Institution, and for the promotion of the general objects of science and utility, to which it may be applied.

These plans I am aware have been ably and comprehensively discussed at the General Meetings of the Proprietors: but as there may be some of the Body now present, who had it not in their power to attend those meetings; and as the Life and Annual Subscribers who honour the establishment with their support, have as yet received no information on the subject, the work that I am about to undertake may not be unacceptable to a considerable part of my audience;—for I trust that a considerable part is interested in the fate of the Royal Institution, its welfare, and its future prospects.

I should feel very great hesitation in offering myself as an advocate, on such an occasion, if I had not been honoured by numerous opportunities of attending the Meetings of the joint Committee of Managers, Visitors, and Proprietors of the Royal Institution, and of listening to the opinions, and I hope imbibing the ideas, of some of the most enlightened scientific men, and of some of the most liberal patrons of science in this country.—I shall endeavour to communicate the information that I have been able to collect on the subject; but at the same time you must not consider what I shall bring forward as an official declaration. I trust I shall make no considerable mistakes in developing the great principles to be pursued, and the outline of the improvements to be adopted: the general reflections that I shall venture to offer on the connections and relations of the subject, may be very imperfect, and in some instances incorrect; for in these respects, I am obliged to trust to my own feeble powers of discussion and illustration.

Before I venture to speak of what the Royal Institution is, or what it may be, I must endeavour to direct your attention to its original design, to its progress, and to the modifications that it has undergone. If in these historical details, I omit all reference to the names of individuals, I trust you will ascribe it to the feeling, which I cannot but entertain, that it would display a want of propriety and of decorum ; and that it would even be presumptuous for me to offer praise or criticism. I shall dwell with pleasure upon the efforts that have been made in this building, to direct the public attention to useful and important scientific objects. You are acquainted with the sources from which these efforts have been derived. You know that the different lists that have been published of the active Members of the Body, contain names, venerated at home, and celebrated abroad ; names distinguished for patriotic views, for active virtues, and for exalted talents.

The first plan of the Royal Institution was that of a School for promulgating the Knowledge and Use of important Mechanical Inventions ; for connecting the Views of Men of Science and Artisans, and for laying open the general Principles, and teaching the Application of all Improvements in the Sciences, in their Connexion with the Arts of Life.

The great feature of the establishment was intended to be a Collection of Models, of Mechanical Contrivances, and of all Implements employed in the Common Purposes of Life, in their most perfect state ; and it was designed to diffuse the knowledge of these, and point out their applications as connected with the general doctrines of science, by courses of Public Lectures.

According to these views, the establishment was equally intended for the instruction of manufacturers and workmen, and for the promotion of the useful arts. It was hoped that the practical man would profit by being instructed as to the correct scientific theories of his peculiar branch of labour, and that he would freely communicate to the philosophical inquirer, the nature of his methods and his operations, that they might be corrected or confirmed by an examination according to strict scientific principles.

At first view it would scarcely appear possible, that a design so important and so benevolent, should meet with obstacles to prevent its execution : as it was to produce a common and a general benefit, it might be supposed, that it would meet with public and general encouragement, and that the persons, who it was conceived were to derive most advantage from it, would be the persons most ready to promote its objects.

As far as patronage from the higher classes of society could assist the scheme, this patronage was bestowed ; and the impracticability of it arose from the condition of the arts and manufactures which it was intended to assist, and from the views of the persons devoted to them, and partly from the nature of our commercial system.

There are two methods by which ingenious men usually obtain remuneration for any inventions applicable to the common purposes of life.

The one is by obtaining a temporary monopoly under a patent, in which case the principle of the discovery is made public:—the other is by concealing their operations or methods, and becoming the sole venders of the manufacture or the produce.

So dignified a body as the Royal Institution, could not humiliate itself by affording a place of exhibition for patent inventions, so as to be subservient to the selfish views or interests of individuals. Such inventions were necessarily excluded; and yet perhaps the most important improvements of modern times, have been brought forward and promulgated by this kind of monopoly.

If patent inventions were not permitted to be introduced, so neither was there any desire amongst manufacturers to communicate any useful discovery, which was a source of profit to the inventor.

On this account, the Model-Room of the Royal Institution, which was to be devoted to important inventions, remained almost empty. That which might be worth introducing, it was against the principle to admit; and that which could be admitted, it was for the interest of the manufacturer or workman to refuse; and thus the principles of science, and the principles of the arts, instead of becoming harmoniously united, were placed in this respect in a state of hostility.

Soon after the foundation of the Royal Institution, a request was made to one of the greatest practical mechanical philosophers of the age, that he would examine the details of the establishment, and become in some way connected with the body. His refusal was prompt, and his expression of disapprobation strong. "Your object," says he, "is one that every practical inventor ought to discountenance. You would destroy the value of the labour of the industrious, by laying open his invention, you would take away the great stimulus to exertion. Suppose a man, by a great devotion of time and of labour, by skill and ingenuity, has made an important combination in chemistry or mechanics, your object is to publish the details of his labours, to enable every speculator to profit by his knowledge. This, could it happen, would be ruinous to individuals, and would ultimately interfere with the commercial prosperity of Britain; for your enemies would profit by such disclosure, more than your countrymen, and it would be absolutely throwing away your superiority. Were I persuaded such a plan of models could be executed, I should be seriously alarmed for the manufacturing interest of the country; but I am convinced, from the nature of this part of the scheme, that it will be ephemeral, and that it will die even in its cradle."

I am not sure that these were the very words of this able reasoner, but I am sure that they convey his sentiments, which were expressed in my hearing. Besides these objections, which flow from the nature of trade and manufacture, there are others which arise from the peculiar and active state of society. Improvements in the common arts are so rapid, that no standards can, with propriety, be set up. Who will say, that an invention cannot be carried to a higher state of excellence? Who will pretend to set limits to human ingenuity? That which is deemed admirable to-day may be imperfect to-morrow, and consequently no collection of models could be permanent:—the articles composing it, would be liable to monthly, weekly, or perhaps daily, change, and an expense and labour would be required to keep it up, for which the object afforded no compensation.

The very principle of commercial prosperity, is this progression in the useful arts: this change, whether resulting from improvement, from the love of novelty, or from fashion. In consequence of this, the manufacturer is constantly active; every faculty is employed to produce new combinations; emulation is preserved alive. The security that talent will be rewarded, brings talent into the market; the idea that invention is fettered by no fixed rules, guided by no unalterable models, leads to enterprize; hence arise new objects of discovery in industry; new branches of manufacture, new or improved arts, which perpetuate and exalt the commercial empire of the country.

That part of the plan of the Royal Institution connected with the establishment of a collection of models in the arts, failed in consequence of the circumstances that have been described; and the object which at first was only secondary, that of teaching the principles of the sciences, and the application of the sciences, by Courses of Public Lectures, soon became the prime object. This, as it could not fail to be of advantageous and useful to all classes of the community, could not fail of being carried into effect; and the only difficulty resulted from the nature of the audiences, which are always to be expected in the lecture rooms of the metropolis; and in which students, men of science, persons in search of amusement, persons in search of information, are necessarily associated together.

To afford satisfaction to all, by one series of subjects, was impossible. Numerous courses were consequently established, in the rotation of which it was conceived the most different tastes might be gratified. Science, Literature, and even the Fine Arts, were alternately made the objects of illustration.

By the exertions and contributions of a few noble-minded and enlightened individuals, a great Library of Reference was added to the Institution, containing the best authors in all current languages, and equally calculated to unfold to the public, the treasures and benefits of science, and the graces and elegancies of literature; and the Catalogue of this Library, which, in the opinion of the best judges, has been skilfully and ably drawn up, will afford no inconsiderable aid to the extension of its utility.

A Mineral Collection has likewise been formed, chiefly composed of British specimens; and calculated to afford to students the means of acquiring mineralogical and geological information; and to promote the search after those subterraneous riches, which are so abundantly distributed in various parts of the British islands.

An object not perhaps the least in the establishment, or an object which I hope I shall be pardoned for being partial to, is the Laboratory; which though formed upon a small scale, and supplied with a small apparatus only, may yet by its effects, tend to demonstrate the importance and uses of such a foundation in the metropolis.

This Laboratory has been always open for carrying on any investigations connected with the progress of chemical science or the arts. It has afforded the means of promoting inquiries of some importance to the community. A number of individuals have received aid from it, in ascertaining the nature of various mineral productions, and substances applicable to manufactures. The practices of a very important branch of trade, at an early period of the existence of the Institution, were submitted to chemical investigation within its walls: I allude to the art of Tanning and Leather-making; and I believe there are many respectable and enlightened manufacturers who will cheerfully acknowledge, that they received the first just ideas of the scientific principles of improvement of their art, from these illustrations and experiments.

I will not dwell longer upon this subject: it would be indelicate for me to be the historian of the whole of the progress of this part of the establishment; nor should I have entered upon it at all, except in consequence of the circumstance, that though it is generally known that some new philosophical facts have been ascertained here, yet it is not generally known, that the chemical apparatus of the Royal Institution has given aid upon several occasions to the useful arts; and that assistance has been offered and afforded from the Laboratory to various great public bodies: soils and manures have been analysed here for the Board of Agriculture, and for many of the great agriculturists of the kingdom, and upon several occasions it has been of use to the different National Boards, which watch over the interests of our navy, our army, and our trade.

There is another object not unconnected with this, upon which I can dwell with more pleasure and more propriety, I mean the Voltaic Apparatus, which has been formed in consequence of a fund raised by subscription.

This instance alone would demonstrate the benefits to be derived from such a public establishment as the Royal Institution.

The expense connected with such inquiries, is within the compass of few individuals, and those who have the power, unfortunately have seldom the inclination to follow the pursuit. Without a public establishment, this great light of new science might have been lost to us; and it is not the less honourable to the character of the nation, that the efforts of private individuals have effected what in other countries flows only from the government: and as the means employed in this way have not been less magnificent, so neither, I trust, will the effects, as to the progress of science, be less important and extensive.

It will not I feel be necessary to enter into a vindication of the utility of any of the more minute details of the establishment. The collection of Scientific Journals, or the means of diffusing such scientific information as can be procured from abroad or at home.

Sufficient I trust has been said to prove, that even in its present form, the Royal Institution has not been useless to the British public:—it might I conceive be demonstrated, that it has not only assisted the progress of genuine science, but has likewise diffused a general knowledge of the advantages and importance of scientific pursuits; and as far as it has been subservient to amusement, that amusement has been at least of a rational kind, of a moral tendency, and connected with no improper, no undignified objects.

The New Plan of the Royal Institution is intended to exalt and enlarge all those parts of the establishment which are acknowledged to be useful and profitable, so as to create a permanent foundation, and means, which can never be misapplied, for the advancement of every species of useful knowledge.

In addition to the popular philosophical lectures which have been annually delivered, it is proposed, that there shall be more Elemenmentary and more Scientific Lectures, which may afford the rudiments of instruction to the uninformed, and which, in their different states of progression, shall arise from the humble beginnings of science, to the great and general laws which are applicable to the system of the universe.

It is intended, that the Histories of Inventions shall be minutely discussed; the different steps of Improvement pointed out, and the Progress of the Sciences, as connected with the progress of the useful arts, made an object of distinct elucidation, in all the different departments of human industry.

It may not be improper to point out the sources of interest in some of the most obvious of these branches of inquiry, and which are yet almost novel objects, and have not usually been brought into any public courses of instruction.

One of these branches will necessarily relate to mechanical improvements, and it offers a flattering picture of human ability and resources.

Man, in his rude state, is submitted to the mercy of nature and the elements. He becomes active only from want, and finds in the gratification of his wants the sources of new enjoyments. He feeds on the spontaneous productions of the earth, or upon animals killed in the chase; he is armed only with the club or the bow, and if he builds a hut it is for shelter. From these simple rudiments, it is easy to trace in slow progression, the arts of agriculture, of war, and of architecture; the cultivation of the earth, the combination of military tactics, and the elegant or magnificent forms of palaces and temples. The more simple cloathing—the leaves or barks of trees, or the skins of animals, were succeeded by complicated manufactures, elegantly and beautifully formed; the difficulties of manual labour, were gradually removed by machinery, and hence new powers and agents, which are still far from having attained their highest state of perfection. Almost all the substances upon the surface of the earth, have been applied to some purpose, or for some end, either as objects of comfort or of luxury. The simplest method of reckoning time, the hour glass, or the graduated candle of Alfred, slowly led to the pendulum clock, and the spring watch, which have become instruments enabling us to measure even space by motion: and the rude wheel and pulley for raising water, led to a succession of inventions terminating in the Steam Engine; the various applications of which are, at present, such great sources of the superiority of our country.

In Astronomy, and its connection with navigation, there has been the same slow yet certain progress, and perhaps a more extraordinary exhibition of the powers of the human mind. The heavenly bodies, at first the objects of superstitious worship, became gradually subjects of scientific observations; and the regularity of their motions led to the idea of some great law in nature. For ages this law was only viewed faintly and indistinctly, and pursued at a distance; and it was reserved for modern times, and to be the glory of a genius in our own country—to give to it a luminous and distinct form—to establish it as an unalterable truth. The progress of discovery in this science will be stated to you by a practical master; and it will be no uninteresting labour to follow the various attempts made for the explanation of the systems of the universe; from the moveable spheres of the Greeks, and the cycles and epicycles of Ptolemy, to the grand principle of gravitation. The knowledge of the position of a few stars, enabled the early navigators to steer their vessels, but they dared not venture far from the land, and coasted along the old world. The Astronomical Discoveries of the moderns, assisted by the Invention of the Compass, have led to the knowledge of a new world, and to the entire subjugation of the ocean. This is a triumph of human power: but perhaps it is a still greater triumph that a mere atom in the scale of things, fixed as it were on a point in space, should be able to weigh and measure the great bodies of the universe, ascertain their distances from each other, predict their motions and their changes; and judge of his own system in its relations to an infinity of worlds.

Natural History depends much more upon accurate observation than upon experiment, yet even this department of Science, has received its greatest elucidation in modern times. The connection of Mineralogy with the Metallurgical arts, and with the mining interests of the country, ought to render the courses of mineralogy and geology subjects of general attention; and every department of the different kingdoms of nature offers some objects to manufactures and arts. Not contented with what is found on the surface of the earth, man has penetrated into her bosom, and has even searched the bottom of the ocean, for the purposes of allaying the restlessness of curiosity, or of extending and increasing his power.

The relations of animated beings, the analogy of their structure and their functions, are subjects not less interesting than their uses; and upon these subjects we can hardly fail to be enlightened, for they will be illustrated by a gentleman who has devoted his life to the contemplation of them, and who has long justly occupied the first place in our National Society, for the promotion of this branch of knowledge. It can hardly be an unpleasant or an ungrateful study, that of tracing the almost infinite variety of beings, different links as it were, of the same great chain, beginning with mere sensation, and ending in intellect, knowledge and power; to examine the order and economy of the system of the earth, and to discover in its intelligent arrangements, constant proofs of the Infinite Wisdom by which it was designed.

So important is Chemistry to the progress of society, that no people has attained any considerable degree of civilization independent of the chemical arts. The fable of the fire stolen from heaven, to animate the statue of clay of Prometheus, may be regarded as an image representing the earliest and most important chemical invention. Without fire, food could not have been prepared: the metals could not have been worked; man must have yielded to the savage animals; neither houses, nor cities, nor ships could have been cultivated nor subdued. The variety of chemical inventions, and the different degrees of their perfection in different ages and countries, is a subject of discussion which can hardly be devoid of interest. The manufacture of Porcelain, and of Glass; Dyeing, Soap-making; the preparation of Cements: the various methods of applying and combining Metals, are all of them connected with our commercial prosperity, our riches, and comforts; and the philosophical science rapidly increasing in perfection, is constantly enlightening the arts. A few vague doctrines derived from Pharmacy and Alchemy, and founded upon the idea of an elixir of immortality, and the philosophers stone, or the substance for converting baser metals into gold, have gradually led by a succession of investigations and experiments, to a beautiful theory, which has explained some of the most important phenomena of nature, and which is daily gaining new powers and instruments, and acquiring new dominion, and which perhaps offers mines of discovery, more rich and inexhaustible than those belonging to any other branch of human knowledge.

Though the Lectures of the Royal Institution will more particularly relate to those departments of Science which are susceptible of illustration, by experiments, or apparatus, or specimens, yet I believe there is no intention to exclude any branches of Literature, which may relate to the progress or advancement of the physical Sciences or the Arts of Life. But the great object of public instruction in a scientific establishment, ought, it is evident, to be, to communicate that kind of information which cannot be gained from books or from private instruction, and to afford instruments and means otherwise not easily accessible. The effect produced by simple doctrine, or history, or didactic lectures, must depend entirely upon the powers of the orator; upon the excellence of composition, and the graces of delivery: but experimental subjects may impress independent of eloquence. The eye is not less succeptible than the ear; and according to the maxim of Bacon, it is better to imprint things on the memory than words: and the Philosophy of Nature, is purely a philosophy of visible and tangible effects.

The courses of instruction, information, and amusement, will be open to all classes of the Subscribers, and to all who may be desirous of profiting by them. It is the great feature of the New Plan, that it will interfere with none of the Privileges granted to the Life and Annual Subscribers, but will rather enlarge them: and one of its prime objects will be a more extensive diffusion of knowledge.

But besides the diffusion of knowledge, the New Plan will also embrace a design for the Promotion of Knowledge by experiments and original investigations. It is proposed, that the Members of the Body shall meet at least once every week for the purpose of inquiry and discussion. At these meetings, any new facts that have arisen in the progress of Science will be stated. Any important hints for experiments pursued; and in the progress of investigation those subjects will be most particularly attended to, which promise to increase the perfection of Arts and Manufactures. Specimens of Minerals or Soils that are produced likewise will be examined and reported upon; and from the intercourse between Patrons of Science, Scientific Men, and the Promotors of Manufactures and Arts; it is conceived that much new information may be obtained, and that the gratification of curiosity and amusement may be connected with the progress of science.

That the diffusion and improvement of science may not be limited to those persons only who can personally attend the Institution, it is proposed to publish Journals at least quarterly; which shall contain notices, not only of the discoveries made in the establishment, or the labours carried on there; but likewise an account of all new and important facts brought forward in any quarter of the world. This part of the scheme cannot but be of great use, and cannot fail to enchance the utility of the Body. It will demonstrate that its objects are not private, but public; that it is desirous of enabling all to profit by those novel objects which may be developed; not of applying them to selfish or vainglorous purposes, but of constantly bringing them forward to be used, and to be considered as the common property of all cultivated minds.

Such are some of the most important objects in the scientific part of the New Scheme ; it will not be necessary to dwell upon the subordinate ones. The moderation of our wishes will not be urged against the possibility of their accomplishment. The Royal Institution is not proposed to be what Lord Bacon proposed of the great Philosophical Establishment in the new Atalantis, a kind of city peopled by philosophers ; a system destined to change the face of nature ; a scheme, as Cowley has expressed it, "for experiments which cannot be experimented."* It asks only for humble means, which may nevertheless produce important ends : and the words of a celebrated divine, speaking of another kindred body then in its infancy, but which has since become illustrious, will not unaptly apply to this undertaking.

"The supply," says Bishop Spratt, "that is needful to finish it, will neither impoverish families, nor exhaust a mighty income."

"If now this enterprize shall chance to fail for want of patronage and revenue, the world will not only be frustrated of their present expectations, but will have just ground to despair of any future labours towards the increase of the practical philosophy. If our posterity shall find, that an institution so vigorously begun, and so strengthened by many signal advantages, could not support itself, they will have reason at all times to conclude, that the long barrenness of knowledge was not caused by the corrupt method which was taken, but by the nature of the thing itself. This will be the last great endeavour that will be made in this way, if this shall prove ineffectual ; and so we shall not only be guilty of our own ignorance, but of the errors of all those that come after us."†

Having described the philosophical objects of the establishment, it may be necessary to say something of its government, of the privileges of Members, of their election, and of the manner in which it is conceived the income of the Institution may be made permanent and sufficient.

In the original plan of the establishment, as the prime object was the founding a Collection of Models : and the diffusing the Knowledge of useful mechanical Inventions ; it was but strict justice, that these Models should be regarded as private property, and belong to the persons by whom they were orginally purchased, or their heirs, and that this property being of the nature of common property, might be also transferred by sale. As the Institution was an experiment, it was proper, that the persons who so liberally offered their support to the experiment, should suffer as little as possible in case of its failure ; and as it was intended to recommend the cheapest and the best implements, utensils, and machinery employed in the common purposes of life, or in the useful arts; such an arrangement, with respect to Proprietary Shares, did not seem ill adapted to a concern which was intended to be partly commercial, and partly philosophical.

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* Cowley's Works, Vol. III. p. 613.

† Spratt, Hist. Royal Society, p. 437.

But this idea, from the reasons that have been already stated, being found impracticable, and new and more exalted objects having arisen from this foundation, and the Royal Institution having fortunately taken the form of a Body for promoting Experimental Science, and for diffusing every Species of Philosophical Knowledge: a form which the philosophers of our elder school would have contemplated with pleasure; and having become an establishment, though upon a diminutive scale, such as they often looked for and desired; it is obvious, that the Principles upon which its Funds are to be raised, and its Members elected, will require considerable alteration.

The Members of the Body, it is obvious, ought to be either Patrons of Science, Encouragers of useful Public Objects, or Scientific and Literary Men; and to belong to the Body, ought to be regarded as a species of scientific honour. But if a Member of the Royal Institution can transfer his Share and his Privileges by Sale, all idea of honorary distinction must necessarily cease; his successor may be not the most proper man, but the best bidder.

From the hereditary nature of the proprietorship, an infant even may become a governor of the Royal Institution, and it is needless to dwell upon the ludicrous nature of the claim of an infant, to what is intended to be applicable only to persons in some measure distinguished, either by their ardour in the pursuit of knowledge, or their zeal for the patronage or the promotion of knowledge.

In the progress of the Royal Institution towards its scientific form, no inconsiderable difficulties, even with respect to finance, have arisen from the saleable nature of the shares, in cases of the decease of a proprietor, they have often necessarily been disposed of, and there has usually been a price in the market below the price of the establishment, a circumstance which has deprived the Institution of a source of income. Though this has happened in a few instances only, and though it is scarcely possible to find a more dignified assemblage of names collected from the different ranks of life, than those which adorn our Proprietary List, yet still the circumstance of sale has tended to render the Institution unpopular, to diminish the public interest in it, and to lessen the desire of persons to contribute to it, by life or annual subscriptions.

On the new plan it is proposed upon a compensation, which after long discussion has been regarded as the most equitable, to do away entirely the saleable and hereditary right, so as to leave no vestiges of them in the Constitution of the Body; and to elect new Members, properly recommended, only by general ballot.

It is proposed, that the Government shall be open and liberal; and that Laws and Regulations may flow from the Members.

As far as the diffusion of Science by the Lectures, the Collections, and the Libraries can be extended, it is intended to offer every facility of admission to the public; but with respect to the Election into the Body, as it is to be a mark of respect, so it will depend upon the will of the Members to bestow it. It has been conceived indeed by some zealous friends of the Institution, that in order to remove the present financial difficulties, it would be proper to render the admission of Members very open, and to hold out temptations to induce persons to offer themselves; but the very opposite plan will, I doubt not, both as to immediate and permanent effect, be the most efficacious. No society can be dignified, that is not select: a Scientific Establishment must owe its reputation to the reputation of its members. Its glory must be derived from their labours, their respectability and their character. It is nothing without a Name, and it depends entirely upon the principle of selection or free admission, whether that name be honourable or despised. Candidates for honorary distinctions, are never wanting; and these distinctions are the more sought for in proportion as there is more difficulty in procuring them. The principle is universal in its effects upon the human mind. What every one can attain, every one is indifferent to; create obstacles which it is honourable to overcome, and you create a stimulus to exertion. In every department of life, every branch of human labour: this is a constant source of activity, usefulness, and greatness. It is the moving spirit of society, a living spring of its improvement, a strong impulse which directs it towards perfection.

The original scheme of the Institution, by making the proprietary interest perpetual, left no means for the renovation of the funds; except by adding new burdens to the Establishment. In the New Plan, the common laws of mortality will be subservient to their support; and without rendering the Body too numerous, the election of new Members will afford a considerable fund, and a constant supply of income. From the extended use and popularity of the Establishment, it may be expected, that the annual and life subscriptions will increase rather than diminish; and that the liberal and disinterested, will be disposed to assist an undertaking, which in the form that it is proposed to assume, cannot be made subservient to any selfish or interested views, and must be a public benefit. For the present exigencies of the establishment, for the purpose of making compensation to those Proprietors who do not wish to belong to the new Scientific Body, a fund is providing, on the plan of a loan without interest; which in a short time, it may be trusted, will be amply sufficient for all the purposes required; and which, as it has commenced upon a magnificent scale (already exceeding £12,000) so it may be hoped, that it will be continued, not only for use, but as serving to demonstrate the confidence that is placed in a scheme purely directed towards useful objects, and as proving the liberality, public spirit, and disposition to make sacrifices, belonging to so many individuals in the Body.

In giving up their private interests for the purpose of founding what may be called a National Establishment, the Proprietors of the Royal Institution have a right to expect the support and encouragement of their countrymen; and though they will be promoting a general benefit; yet they may perhaps make a peculiar appeal to some of the most distinguished classes of society.

To the great Landed Proprietors of the country, such an Institution cannot fail of being highly useful, even as far as their mere common interests are concerned. It often happens, that specimens of ores, of stones, or of substances supposed to indicate coal, are found, and the consideration of these matters is usually submitted to adventurers, with whom it is a great object to carry on works of research, and to make (using popular language) a job of a pretended discovery. Those persons who may belong to this Body, will not be liable to such impositions. Whatever specimens they may send, will be carefully examined and reported upon. It will be stated whether they are valuable, and whether research is likely to be efficacious or fruitless.

It would be easy to bring forward various instances in which, from a want of accurate information, much expense has been incurred; but I shall rather state cases in which our Laboratory was of use in preventing fruitless labour, and vain expenditure.

A gentleman in the south of England, in consequence of the discovery of a small quantity of coaly matter upon his estate, had been induced to open a mine. A specimen of it was brought here; it proved to be mere wood coal without bitumen, and it required no extraordinary sagacity to decide, that such a production, was no indication of a stratum of pit coal.

Another gentleman had been informed, that a particular clay on his estate, contained a considerable quantity of alkali. He had been at some expense on the subject, and was advised by the pretended discoverer, to erect works for extracting this alkali. He sent a specimen to the Institution; it contained no alkali, and the adventurer and his scheme vanished.

I might multiply these instances; but I trust there will be no occasion, the absolutely tangible and practical uses of this part of the plan must be instantly perceived. Here there can be no instigation for delusion; the simple truth will be stated by men whose character as well as motives will secure them from any suspicion of inaccuracy; and if a general system of this kind is pursued, much error and disappointment, and even dishonesty, will be prevented. The spirit of enterprise will be neither pushed on by false hopes, nor checked by unexpected disappointments; and persons will not be deterred from carrying on useful labours and projects, because they have failed in impracticable ones.

On the attention of the Statesman and the Politician, we have likewise no inconsiderable claim. The Royal Institution is able to offer assistance in investigations of great interest, connected with public works, with articles of manufacture productive to the revenue, in the selection of naval and military stores, and the examination of the arms and materials employed in war, the quality of which is of the greatest importance. On another ground likewise, we have an especial right to their patronage and protection. Our object is one, which in its extension, cannot fail to promote arts and manufactures, and to raise novel objects of industry.

It is no new truth, no recent discovery, that the prosperity and the riches of a country are intimately connected with the progress of the Arts and Sciences; and how much this progress is assisted by public honours and encouragement, may be learnt from an instance in a neighbouring country.

That Colbert, the Minister of Louis XIV, raised the power of the French nation, is known as an historical truth; and it is likewise known, that this was principally owing to the patronage which he so amply bestowed upon Sciences, Arts, and Manufactures: and a succession of such efforts, unless met by corresponding energies on our own part, would do more to diminish the great commercial superiority of Britain, than all the armies, and all the edicts, which have lately been so vainly opposed to our prosperity.

To all persons of liberal views and of cultivated minds, the particular departments of knowledge, to be illustrated or pursued in the Royal Institution, may not be devoid of interest. To the Legislator and the Senator, an acquaintance with the sources from which the great improvements of modern times are derived, cannot be wholly useless; and the History of the Arts of Life seldom forms a part of the common course of education. The diminution of labour by machinery; the riches that may be derived from perfection in the branches of trade; the general principles of Inventions in Chemistry and Mechanics, are not improper objects of study for the political œconomist: and knowledge of this kind must enchance the powers of eloquence; for harmonious combinations of words are never so impressive as when they refer to realities and to facts which cannot be questioned. One of the most celebrated orators of modern times, owed great part of the effect he produced to the copiousness of his instances, to the fullness, variety, and minuteness of his knowledge respecting the Scientific Principles of the refined and common Arts; and it is this circumstance, as much as his vehement and powerful manner, his poetical imagery, and his wonderful sagacity, that will carry his memory illustrious into future ages.

Our doors are to be open to all who wish to profit by knowledge;

and I may venture to hope, that even the female parts of our audiences, will not diminish, and that they will honour the plan with an attention which is independent of fashion, or the taste of the moment, and connected with the use, the permanence, and the pleasure of intellectual acquisitions. It is not our intention to invite them to assist in the laboratories, but to partake of that healthy and refined amusement, which results from a perception of the variety, order, and harmony, existing in all the kingdoms of nature: and to encourage the study of those more elegant departments of Science, which at once tend to exalt the understanding, and purify the heart.

The leisure of the higher female classes is so great, and their influence in society so strong, that it is almost a duty, that they should endeavour to awaken and keep alive, a love of improvement and instruction.

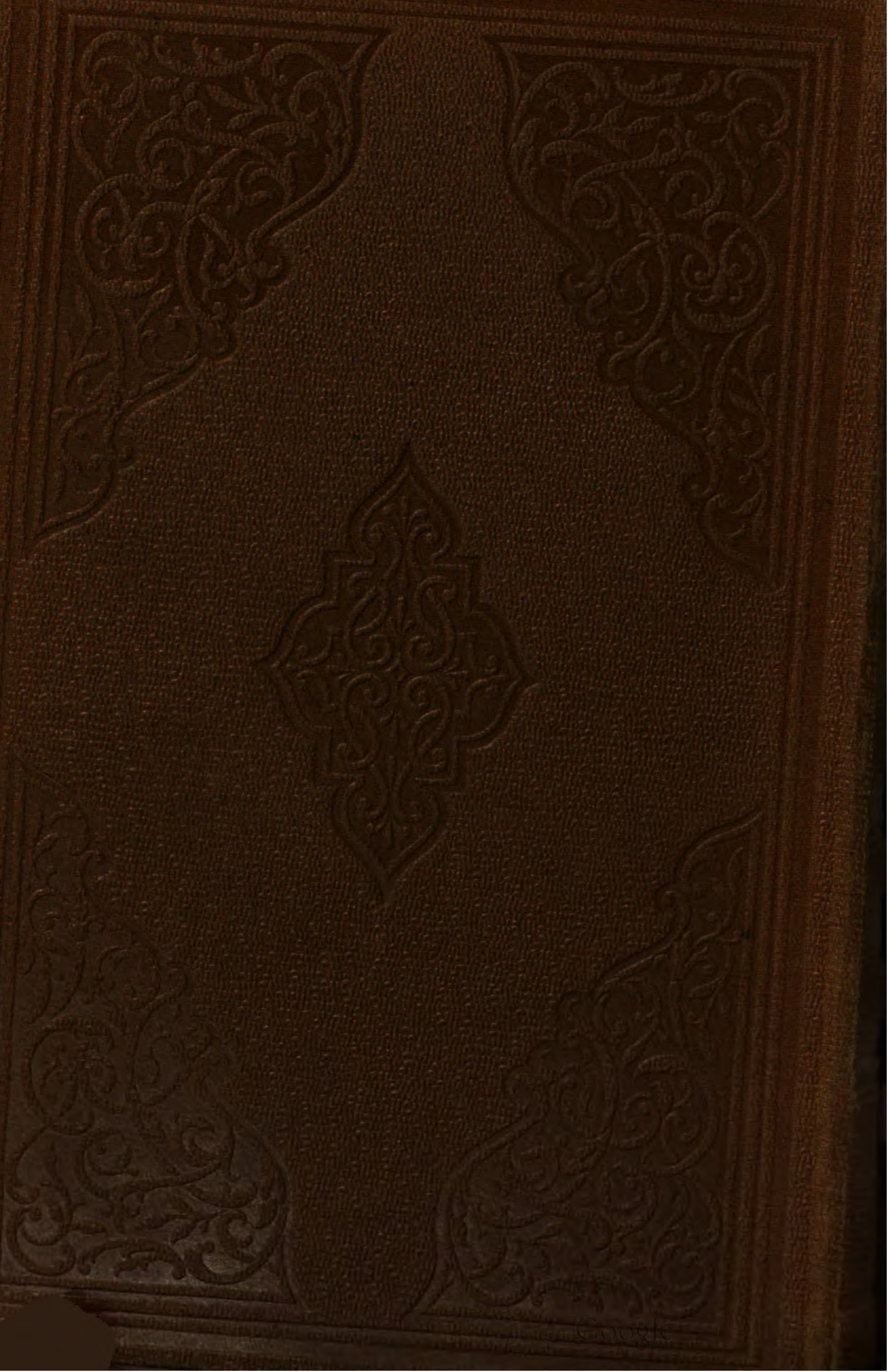
Let them make it disgraceful for men to be ignorant, and ignorance will vanish; and that part of their empire, founded upon mental improvement, will be strengthened and exalted by time, will be untouched by age, will be immortal in its youth.

Even in the common relations of society, how much must be referred to the conduct of the female mind. The mother gives, or ought to give, most of the early impressions to the child, and his future habits may depend in some measure upon her influence. It may in some measure depend upon her, whether he become an honour or a disgrace to his country. Her power of enforcing instruction, is the most effectual, as flowing from love. We know, that without feeling, the human being is mere clay, the dust of the earth, without the living soul. Whatever is to be permanently infixed on the understanding, must be associated with hope or with joy, or with passion. How much more efficacious must instruction be, when communicated by an object, beloved and venerated, and in infancy, almost adored; and when, instead of being afforded with an effort of pain and of labour, it is carried into the heart by kindness, and made delightful by caresses and smiles!

As the object of the Royal Institution is to promote the Progress of Science; so it will not interfere with any other literary bodies, any other Societies; but will rather assist and promote their views.

It has been supposed by some persons who are little acquainted with the nature of the plan, and the general objects of philosophical Associations, that there is a tendency in them to lessen the importance of our elder establishments for education, and to diminish the love of ancient literature. But nothing is further from the truth. The maxim of improvers, is—promote, whatever can tend to assist the progress of the human mind; and Letters will always be the greatest, the most powerful engine to this effect; it is one that all can employ, the strong and the weak, in solitude or in the world. That which is beautiful, that which is pathetic, that which is sublime, can never lose its effects. There is one course of passion and feeling in all times, and in all countries; we should never cease to consider with admiration and gratitude, those models of excellence which have been happily preserved, amongst the wrecks of cultivated nations, to be our guides in the middle ages; to be our shelter in the storm, and our light in the darkness; the beacons to guide us to pure taste, to correct our wanderings, to bring us to nature and truth. Let us regard them with all respect; but let not our veneration for them be exclusive: let us admire them, as we admire the works of Art of Antiquity: the Apollo Belvidere, or the Venus de Medicis, were designed by their artists to be objects of adoration; let us wonder at them as statues, as models of perfection, but not worship them as Deities, nor even make them our only Household Gods. Greek and Roman literature will always maintain their importance, always exert their influence; but let us not neglect that basis on which the greatness of modern times, and of our own country, so peculiarly rests; Experimental Philosophy and the Experimental Arts. Let their merits be justly estimated, and set forth with dignity and truth; let not the countrymen of Bacon, of Newton, and of Boyle, neglect those pure springs of knowledge from which those great men drew such copious supplies, both for profit and for glory: and let it not be forgotten, that Science has its moral and intellectual, as well as its common uses; that its object is not only to apply the different substances in Nature, for the advantages, comfort, and benefit of man, but likewise to set forth that wonderful and magnificent History of Wisdom and Intelligence, which is written in legible characters, both in the Heavens and on the Earth.

[H. D.]



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